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THE
RETROSPECT
OF
PRACTICAL MEDICINE AND SURGERY,

BEING

A Half-Yearly Journal,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY
W. BRAITHWAITE, M.D.,
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PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

1.—OBSERVATIONS ON SCARLET FEVER, ESPECIALLY WITH REFERENCE TO ITS EPIDEMIC CHARACTER.

By Dr. A. W. Barclay, Physician to St. George's Hospital.

[The miasm of scarlet fever, though yet untraced, must be some sensible portion of matter. It may be infinitesimally small, but matter it must be in some state of change, and, like other matter in a state of change, liable to excite similar change in other portions of matter capable of being acted on. The miasm is no doubt an organic compound, not gaseous, and not a vapour, but capable of suspension in air, or in the aqueous vapour combined with it.]

For my own part, I can see no probability of truth in that which would associate its propagation with living organisms. Even if all were granted that has been assumed in regard to the influence of germs floating in the atmosphere on the healing process in breaches of surface, it by no means follows that the metamorphosis of tissue throughout every part of the body should be influenced by the presence of such germs on the outer surface of unbroken tissue, whether applied to the skin or to the mucous membrane, and is, in fact, opposed to the actual knowledge we possess of the action of epiphytes on either.

But I would say farther, that the analogy which has been supposed to exist between suppuration and fermentation or putrefaction fails in one most material point, inasmuch as the living organisms developed from germs are found, and found invariably, in the putrefactive process; and by the introduction of the germ in certain chemical conditions of matter, fermentation or putrefaction may be excited at will; while no such evidence has been adduced with reference to suppuration. On the other hand, it is well known that the course and progress of the healing process is invariably influenced by the presence of dead animal or vegetable matter; that, in broad outline, the law by which suppuration is excited by the dead organic thread of a silk ligature, and is not excited by a silver suture, applies to the reunion of tissue under all circumstances whatever. Consequently, if it be thoroughly established that chemically pure air will not excite suppuration, while air loaded with ordinary dust, the motes that float in the sunbeam, will as certainly do so, the explanation will be found in the law which regulates the action of dead organic matter on divided tissue in the living body.

Another hypothesis here meets us which cannot be so readily disposed of. It is alleged, with some show of reason, that the exudation from the surface of the skin, and more especially the cuticle, is the main source of infection. And the theory is of importance, because upon it has been based the suggestion that the spread of the disease may be to a great extent arrested by careful inunction and ablution of the skin. I suspect that this theory, whether true or false, has originated in an error very prevalent of late years in the writings of medical men, itself the offspring of a wrong use of terms. No expression is more common than fever poison. Persons are said to be poisoned by impure air or impure water. The poison is said to be got rid of or eliminated by the skin or the bowels, as the one or the other happens to be the organ chiefly affected by the disease. In such expressions there is a lamentable want of that precision of language and of thought which is absolutely essential to correct reasoning.

The term "poison" has been unfortunately suggested by the circumstance; that all the symptoms take their origin in the introduction into the system of something *injurious from without*; the facts which have been ascertained, and the theories which have been adopted with reference to ordinary poisons, being as a consequence applied to the so-called fever poisons. In truth, there is scarcely any analogy between them; and the differences in their origin and mode of action are so great, that it would be more consistent with reason to believe that what was true of the one class would be false when applied to the other. In tracing the history of such cases, the first difference that strikes us is the infinitesimal quantity in which miasm acts on the body; and with reference to smallpox, at all events, it may be asserted that in the practice of inoculation the amount of matter introduced under the skin bears no relation to the intensity of the symptoms resulting from it. Poisons, on the other hand, act only in sensible quantities, and the symptoms bear a direct relation to the amount introduced. Next we find in the one class a period of incubation during which no signs of poisoning are shown, whereas no such condition is ever seen in the other. Lastly, from the body of the individual suffering from miasmatic disease an immense quantity of fresh miasm is constantly being given out; but we know that a person who has been poisoned cannot possibly discharge from his system more poison than he has imbibed.

Putting false analogies aside, we may, however, inquire whether there is any reason for assuming that the skin is more likely to be the medium through which the miasm of scarlatina is propagated than the mucous membrane. The two circumstances which, to my mind, offer the strongest presumption in favour of this view are—first, the much greater extent to which the skin is involved in the course of the disease; and secondly, the late period at which infection may be caught from bedding, clothing, &c. In smallpox we have the strongest possible evidence that the most direct element in the propagation of the disease is the fluid contained in the pustules on the skin; but, at the same time, I should very much doubt whether an unprotected person would be safe from infection in a confined apartment where a patient was lying in the early stage of fever before vesication had begun. In scarlatina the affection of the skin, though so different in kind, is yet so marked and so extensive, that if the cutaneous secretion take any part at all in the propagation of the disease, it cannot fail to be an important one. And whatever may be said of other modes of transmission, I think we cannot withhold our belief that the skin is in this disease, too, one of the most direct media of infection.

With reference to the period during which miasm will retain its infectious character, if articles of clothing, &c., be shut up in a box, I have no new facts to add; I would merely call attention to the circumstance, as indicating that it must be associated with some substance of a comparatively stable character. In all infectious diseases we know that chemical change will render the miasm innocuous. Vaccine lymph requires certain precautions to be adopted for its preservation; and if the miasm of scarlet fever were only carried about in the air or suspended in watery vapour, it would probably very soon lose its infectious properties through decomposition. It seems, therefore, not unreasonable to conclude, that when the disease has been propagated afresh after a long interval, the miasmatic character has been transferred to some material which was not liable to rapid decomposition; such a material, for example, as the dry cuticular surface, which, if preserved from moisture, will remain for a very long time without undergoing any great change in chemical character. At first sight it is perhaps difficult to reconcile this condition of chemical stability with what has been already asserted of miasm generally, that it is matter in a state of change. On farther consideration, it appears that the two conditions have no closer relation to each other than is expressed in the statement that chemical stability is absolutely essential to the development of this vital change; that any substance rapidly undergoing chemical change is incapable of developing it; and that the chief means we possess of preventing its development is the employment, in the presence of miasm, of such reagents as tend to promote chemical decomposition, so that we have no better disinfectant than free oxygen.

But while admitting the great probability of scarlatinal miasm being propagated by means of the skin and its exudation, we have still to consider how this is brought about; whether as the result of some specific secretion, or as a *consequence of general* change affecting all the secretions alike; and farther, whether there is any evidence to show that the miasm attaches itself more to one secretion than to another. This question has been already in part answered. If, as we have asserted, there be no poison to eliminate, there can be no special secretion; the ordinary secretions, so far as disease does not arrest them altogether, will continue to go on. But their character, though unchanged to all appearance, must have received a certain impress from the universal action proceeding in every part of the body, and this is only known to us by its capability of exciting the same actions in another. So far as analogy can serve as a guide in reasoning on such matters, I think we should naturally conclude that one secretion was just as likely to have received this impress as another, and that the relative power of each in propagating the disease depends rather on secondary causes than on its direct relation to the miasm so propagated.

The elimination theory asserts, in opposition to this view, that during the existence of specific fever a poison is generated which must be eliminated or got rid of by some secernent action, and that the selection of the apparatus employed for this purpose is the chief cause of the symptoms by which they are recognised. Two questionable statements are involved in this proposition: viz., that there is a poison, and that it must be eliminated. To deny that there is a poison may seem paradoxical; but I merely wish to express that the word "poison" gives a totally erroneous idea of fever miasm. Water may have a striking resemblance to alcohol, because they are both colourless fluids; but they are essentially different in most respects, and to group them under a common name would be only to cause confusion. I need not again repeat the chief points of difference; but in nothing is the distinction more marked than in the fact, that an infinitesimal quantity introduced will multiply itself almost illimitably in the individual suffering from the disease. If a man happen to have swallowed a certain quantity of opium or arsenic, we know that a foreign element is present in his constitution which must be got rid of sooner or later, the total amount remaining unchanged throughout. So long as any portion is retained, it does harm in an exact ratio to its quantity; in short, his life and health depend on the poison being eliminated. When, on the other hand, a man has imbibed a fever miasm, its quantity is infinitesimal, imperceptible; it acts as a disturbing cause in this minute quantity, and the measure of the disturbance is not regulated by the amount absorbed. After a time fresh miasm is generated; but the individual is no longer susceptible to its influence while it remains with him, and no perceptible change in his state is produced when it leaves him. To him the quantity given off is wholly immaterial, whether he serve as a focus of infection at the commencement of his attack or during the period of convalescence. To him it is much more important, be the attack mild or be it severe, that all the ordinary functions of life should go on without interruption; the arrest of any one of them, by exposure to cold or any other accidental circumstance, is of far more consequence than the possible amount of infectious miasm generated during his illness. For him there is no elimination.

If, then, we put aside the theoretical argument derived from the supposed necessity for the elimination of the poison, there remains, as it seems to me, no reason for believing that any specific secretion is concerned in the propagation of scarlet fever. No one who has watched its progress can doubt the extent to which the blood and tissues are involved, can doubt that though the skin-affection be one of the most prominent symptoms, it is by no means a primary element in the disease. Neither can we withhold our belief, that the action taking place in the skin is produced by and dependent on the more general affection. Were farther proof necessary that the skin only becomes a medium of infection in consequence of this circumstance, it seems to be afforded by the fact that in this disease the specific action which specially marks its place among miasmatic fevers affects the mucous membrane as well as the skin, producing ulceration of the throat in a large number of cases. The conclusion seems unavoidable that, so far as we at present know, any secretion, or more probably all the secretions, may contain matter in a state of change, which, when meeting with other portions of matter capable of undergoing the same series of changes, *is liable to excite* them, except when prevented from doing so.

by some chemical or vital law. In fact, it seems not improbable that the secretions themselves, while not otherwise sensibly modified, have this special impress stamped upon them by their coming from an individual for the time suffering from the disease.

With reference to the means at our disposal for arresting the spread of the disease and aiding the recovery of those who are already under its influence, few words may suffice, as they are subjects now so well understood by the majority of well-educated medical men. We have assumed that the phenomena of abnormal vital action are excited by some portion of organic matter which is at the time of its entrance into the body in a state of change, and that the whole of the tissues may or do pass through a series of analogous changes, so that any effluvium or particle of matter given off is liable to excite the same changes in another person. Experience teaches that the miasm so given off is to a certain extent unstable, and may be rendered innocuous by chemical decomposition, and we therefore turn to chemistry for means to accomplish this end. The agents which have hitherto been chiefly employed may be divided into three classes—deodorisers, antiseptics, and disinfectants, according to their most prominent effects. Such terms are necessarily only relative, and do not imply that their action is limited to that which is exactly defined by the strict meaning of the word. Among deodorisers I would class such substances as Burnett's solution and the perchloride of iron, which at once deprive sewage and decaying animal matters of their fetid smell. They possess, in common with many other metallic salts, powerful chemical affinities, and will more completely neutralise free ammonia and decompose sulphuretted hydrogen than many other substances which are perhaps of more real value as disinfectants. At the same time it is probable that this power of decomposition extends beyond their more sensible effects to the organic miasms which accompany the fetid gases. It is not unlikely too that the chlorine they contain may be of some importance, though its action must be very different in such forms of combination from that with which its presence is usually associated in our minds. Antiseptics belong to a different class, and it is difficult to say to what extent they act as deodorisers, because the smell of the substance itself is usually so powerful. The one great representative of this class is carbolic acid in its various forms. Its power seems to consist in restraining organic matter from passing into that series of chemical changes to which it is liable as soon as it is removed from the controlling power of vital action, an effect which may be hastened or retarded by a variety of causes. If fever miasm consisted simply of matter undergoing putrefactive change, there could be no question that carbolic acid would be the most certain disinfectant; and it is not unreasonable to believe, that if it possess the power to restrain putrefactive change, it may also arrest that other series of changes which converts the exudations from the sick into a means of propagating disease to the healthy. With this idea it has been very much employed of late, and possesses the advantage of being very diffusible through the atmosphere, and of indicating by its odour, which is not disagreeable to most persons, the extent to which this diffusion has taken place.

True disinfectants must consist of those substances which deprive the organic matter of its power to excite in any other living body that tendency to change which itself is for the time being subject to. The principle of their action would seem to be, that by chemical affinity the peculiar tendency of the organic particle is altered so as to bring it under the ordinary laws of decaying animal matter. Great heat and oxygen gas possess this power in the highest degree; and it is not improbable that all the substances employed as simple disinfectants owe their power to the fact that they tend to set oxygen free. Thorough ventilation of the sick-room acts not only by diluting the miasm and carrying it away as speedily as possible, but also by exposing it more freely to the action of the oxygen contained in the atmosphere. At the same time it would seem that oxygen liberated from some chemical combination acts most energetically in its nascent state, and hence chlorine gas and the permanganates are believed to destroy fever miasm more perfectly than any other agents. Unfortunately the vapour of chlorine is very irritating, and the permanganates are not diffusible through the atmosphere. The effect attributed to the burning of sulphur with the escape of sulphurous acid gas is probably explicable, though less directly, in the same way. To those who are inclined to hold the germ theory of disease it might offer some corroboration of their view, that this acid is especially destructive of the lower forms of vegetable life. The same remark will also apply to such temperatures as are employed for depriving articles of clothing of infection; for the very same degree of heat serves to destroy the life of the lower organisms, though they survive a temperature which the higher classes of animals cannot withstand.

In the earlier part of this paper I have endeavoured to show that the skin is not in all probability the only medium of conveying infection, but that its minute scales are very likely to retain it longer and convey it to a greater distance than any other exudation. It is therefore essential that, while every precaution is taken to preserve the purity of the atmosphere in the sick-room, special care should be taken that the clothing and bedding are thoroughly disinfected. The plan of inunction which has been recently suggested would seem to have this one recommendation, that during the stage of desquamation the minute particles are more likely to be scattered about the room, and perhaps carried about in the atmosphere, if the skin be dry, than when it is moistened by an unguent; but it does not in the least degree render the adoption of other measures less necessary, both during the attack and after the patient has been removed from the room; the absolute prevention of the escape of a single particle of cuticle would not restrain the spread of the disease through other channels.

[Stimulants contribute to the end of sustaining the powers of life not only by their action as pure stimulants, but by their power of retarding the metamorphosis of tissue.]

Just as excess in stimulants in health will produce nausea, depression, and biliary disturbance, so in disease we must be careful to subordinate them to other forms of nutriment, and to have regard to the actual necessity for their administration. So long as food is taken well, and the digestion does not seem to fail; so long as the circulatory and the nervous systems do not point to the needs of stimulus, it seems unwise to have recourse to them too early. Sooner or later, in larger or smaller quantities, they must find a place in almost every case of scarlatina; but in every instance it should be a matter of thought and consideration, and not one of mere routine practice, when stimulants are ordered. When the necessity is great, they must be given without stint, at least for a time; and the unexpected rallying of patients suffering under very severe forms of the disease is at once the justification of the treatment, and the explanation of the excessive mortality so often found among the poor.—*St. George's Hospital Reports*, 1870, *vol.* v., *p.* 167.

2.—ON SCARLET FEVER.

By Dr. E. COPEMAN, Norwich.

[There are three important weapons necessary to the treatment of scarlet fever, viz., fresh air, pure water and good diet. The air dilutes and rarifies the poison: the water washes away impurities, and prepares the surface for the excretion of morbid matter; and the food fortifies the constitution so as to render it less obnoxious to the depressing effects of disease.]

One of the most common of the sequelæ of scarlet fever is dropsy, with albuminous urine; an untoward complication, and one which generally supervenes at a time when all idea of danger has been removed, or most probably in cases where no danger has been apprehended. It occurs more frequently after mild cases; and early exposure to cold during convalescence appears to be the most frequent cause. Many years ago I published a short paper on Scarlatina in the Medical Gazette, vol. xxx. p. 96, chiefly with reference to the treatment of the dropsy which often supervenes. I there remarked, that this affection (dropsical effusion) is generally considered to be of an inflammatory character; and bloodletting and other antiphlogistic remedies are recommended for its removal. The general *success of such measures* is also established by the concurrent testimony of various authors; but during the late epidemic I had occasion to treat the disease in patients who had been so much debilitated by the previous attack of fever, that I had not the courage to further reduce the powers of the constitution.' I was then induced to make trial of iodine, which I prescribed in the form of Lugol's solution. 'Of this solution I ordered from five to ten drops for children, and from ten to twenty or twenty-five to adults, three times a day in water. In the first case in which it was used it rapidly effected a cure; in consequence of which I prescribed it in every succeeding case that presented itself, and with the same complete success. If iodine have not hitherto been employed for the cure of anasarcous swellings after scarlet fever, I believe I shall have the happiness to have introduced to the profession a safe and effectual remedy for a troublesome and obstinate affection. If others have employed it I have the gratification to add my testimony to the good effects of a mode of practice not so well known or recommended as it seems to me to deserve.'

I have transcribed the above remarks because they are still applicable as regards my further experience of the treatment of scarlatinal dropsy. I have always used the same remedy (now in the form of the liq. potas. iodid. comp. of the London Pharmacopœia), and may now, as I did then, confidently affirm that it has invariably been attended with success. Sometimes it has for a while appeared to fail; but on increasing the dose the desired effect has been produced, and the patient has recovered. Many such cases have presented them selves at the Lind Infirmary for the diseases of children. All that have come under my care have been thus treated, and in no instance without a favourable result. In private practice also many similar cases have been cured by it, where the disease had resisted other treatment, and led to my being called in consultation. The following is an instance of one of the most severe forms of the disease, in which this remedy was rapidly and entirely successful:

A child, aged 6 years, had been attacked with a very mild form of scarlet fever three weeks before. There had been but little eruption, and the child appeared to be rapidly recovering; but after exposure to the air, having been allowed to go out, she was seized with anasarca, which gradually extended so as to become general. There was also effusion into the cavities. The urine was scanty and albuminous; and the child was suffering so much from dyspnœa, that the slightest alteration from the upright position almost produced suffocation. The pulse was very rapid and indistinct; and it seemed that a fatal issue was imminent, the more so because the case had been treated with the greatest care and attention, and the only remedy I had confidence in under such circumstances had been already tried. On enquiring, however, as to the dose, I found that a much smaller one than I had often prescribed had been ordered, and I strongly urged another trial of the liq. potas iod. c. in drachm doses every four hours. There seemed but little chance; but happily on the following day the breathing was less laboured, and the child was actually able to lie down with the head but slightly raised. Suffice it to say that a continuance of the treatment produced the entire removal of the dropsy; and when I called again in about a week, the little girl was playing about almost well. She had no return of the disease.

In conclusion, I wish to make a few remarks upon a subject confessedly of the utmost importance, and which has been frequently discussed of late, namely, the means of preventing the spread of scarlet fever. It is without doubt a very great scourge in this country, and no very satisfactory conclusion appears to have been arrived at as to how it is to be prevented, or how its progress may be stayed. Opinions vary much upon the subject; but, after a long professional life, I hope I may be excused if I express, without any desire to dogmatise, the conclusions which my own experience has led me to adopt. Without discussing how scarlet fever first originates in any particular district, I dwell upon the mode of its propagation, and how it gradually increases in severity. A boy at school gets an attack of mild scarlet fever, and is sent home to a distance; his father's house is recently built and well situated, and the locality is healthy. This boy passes through a mild regular attack, and recovers. But soon a younger child is attacked, and then the nurse; and both rapidly die. Another child dies; and in a day or two the mother also; then a fine boy of four years old is seized in the morning, and dies at night in the stage of collapse, without presenting any of the distinctive marks of scarlet fever. One child, the eldest, only remains, and he is sent to an old almost unoccupied farmhouse, goes through a mild attack of scarlet fever, and recovers. The other members of the household are sent away to the seaside, and an examination of the premises is made to ascertain, if possible, the cause of this dreadful mortality, all the deaths having occurred in the course of one week. I should say there was no scarlet fever in the neighbourhood at the time of this sad occurrence, nor did it spread to any other house in the village. I found nothing to complain of in the house —a new parsonage, clean and convenient in all its arrangements; but there were two water-closets; one up-stairs for the ladies, and the other on the ground floor for general purposes. The waste-pipe of the former emptied itself into a covered drain running along the bottom of a fence to a great distance from the house; the latter discharged its contents into a square open bin situated close to the back door. Into this latter closet all the excreta of the patients and bedroom slops had been emptied; and to the exposure of them to the air in the open bin I attribute the increased virulence and fatality of the disease after its first importation into the house.

Very recently I was summoned to a neighbouring town to visit a young gentleman at school there; but being out of the way at the time, another practitioner from Norwich went in my stead. This was on a Sunday. On the previous Friday the boy had been playing cricket, and was not ill till that night. My friend found him moribund, and he died in a few hours. He seemed to have died of some acute attack in the head, and it was difficult to say what killed him in so short a time.

After a few days I was called in consultation to another pupil of the same school, but who had been sent to his own home, as his father resided in the same town. He was fourteen years of age, and previously in good health; and I found him labouring under a severe attack of scarlet fever, with eruption fully out all over him of a bright scarlet colour; with hot skin, rapid pulse, ichorous discharge from the nostrils, and drowsiness. His throat was sore, but not very much affected; he could be roused pretty easily; and I discovered no very dangerous complication. He was treated simply with liq. ammon. acet. and sp. æth. nit., the usual fever-mixture, and enemata when required. The nostrils were treated with nitrate-of-silver solution applied on a brush. I saw him again next morning, and found him not at all worse, and the nostrils decidedly better. Before I left the house I particularly cautioned his father not to allow any of the excreta to be put down a water-closet, but to be at once removed and emptied into holes in the garden, at a distance from the house, and covered with mould. I heard afterwards that he made a good recovery. But three other cases occurred in boys from the same school, two of whom died, showing a mortality of three out of five cases. My patient's brother had afterwards a very slight attack; his father had a little sore-throat; but his mother, a delicate lady, who nursed him night and day throughout his illness, escaped the contagion altogether.

The result of my experience and my firm belief is, that scarlet fever, whatever may be the nature of its infectious principle, is propagated and intensified by the exposure of the excreta of those who are affected by it, and that they are the pabulum on which its miasm, whatever it may be, delights to feed. I am equally convinced that a speedy removal and disinfecting of these excreta will prove the best means of rendering the disease milder in its character, and less likely to spread its venomous influence in the localities in which it may chance to prevail.—*St. George's Hospital Reports*, 1870, *vol.* v. *p.* 55.

3.—ON THE ETIOLOGY OF TYPHOID FEVER.

By Dr. John Ewens, Cerne Abbas, Dorset.

[Dr. Ewens has long had the opinion that typhoid fever is not in the ordinary sense of the word contagious. In country places, a more correct conclusion on such matters can generally be arrived at than in large towns, where the origin of cases cannot be so easily traced].

In the spring of 1854 or 1855 (a very dry season) an epidemic of typhoid of a very severe character occurred in the hamlet of Whatcombe, about half a mile from the village of Whitchurch, in this county. It was entirely confined to the inhabitants of the houses which obtained their drinking water from a well (situated immediately opposite the doors of three cottages), the mouth of which was much below the level of the surrounding ground, and consequently easily admitted of the flow of refuse matters, thrown out of the houses, into the well. Adjoining houses, which obtained water from other sources, were free from the disease; and the neighbouring village of Whitchurch, with which there was constant communication, was absolutely free from typhoid, with the exception of two men, who worked at Whatcombe, and were accustomed to take their midday meal at a cottage near the well, and, doubtless, drank of the water. Suspicion soon fell upon the well, which, being examined, was found in a very impure state. It was closed *pro tem.*, and no other case occurred. In due course it was cleansed, and the surrounding ground raised, with good results.

In another village an epidemic of typhoid broke out in two adjoining cottages, every inmate of which (eight in all) was attacked, and three died. The cause was easily traced to impure water. Two of the nurses were attacked, were removed, *did not* communicate the disease, and made good recoveries.

The village of Cheselbourne, near here, was formerly often the scene of more or less severe epidemics of typhoid fever. The inhabitants (about 440 in number), almost without exception, obtained their water from a stream which was exposed to contamination from various sources. In dry seasons, when the water is low, the impurity is less, owing to the absence of surface water, which, of course, brings foul matters from the farm-yards, which otherwise would drain away into the soil, and it so happened that typhoid was always most prevalent in wet seasons. After a very severe epidemic, urgent representations being made in the proper quarter, some wells were sunk, and since that time there has been a marked difference in the health of the place as regards typhoid fever. Sporadic cases have now and then occurred, as, in spite of all that can be said, people are always to be found who prefer drinking foul water, easily obtained, to going a short distance to get pure water.

One more case occurs to me. The inhabitants of two cottages, isolated, who ordinarily obtained their water from a well, were driven, through the well being out of repair, to get water from a small stream flowing down from a village where typhoid was epidemic, and which must have received poisonous dejecta. Some of these people were speedily attacked with the disease.

My experience leads me to confirm Dr. Latham's opinion as to the *comparative* rarity of nurses, &c., attending on the sick becoming affected. That such cases do happen cannot be denied, and I have sometimes been at a loss to explain them consistently with the idea of the ordinary non-contagious theory. Dr. L.'s explanation removes in a measure the difficulty. As a rule, I find it much more common to meet with only one or two cases in a family, and it is the exception for more to be affected at once. It is very desirable, if possible, that the question should be settled, as medical practitioners are constantly interrogated as to the danger of infection, and it is not always easy to explain the peculiar circumstances of the case so as to satisfy the minds of timid people. I have heard of medical men speaking of the disease as the "least contagious kind of fever," I suppose because they find it rarely spread by what appears to them to be contagion. But might we not on the same principle (if the reasoning were correct) say that a case of scarlatina, which, as we all know, may prove a solitary case, was less contagious than another which proved the nucleus of a severe and wide-spread epidemic? But the solitary case is the exception generally in scarlatina, and due to some cause not always easy to determine. I believe it will be found, as a rule, that wells sunk through a sandy soil are more likely to be contaminated by the proximity of a cesspool than those sunk in chalk, and this perhaps may be the reason why manure heaps are often seen very near the mouth of a well in the latter stratum without the water getting impure. *Lancet, August* 12, 1871, *p.* 250.

4.—ON THE ETIOLOGY OF CONTINUED FEVER.

By Dr. P. W. Latham, Fellow of Downing College, Cambridge, and Physician to Addenbrooke's Hospital.

In the *Lancet* of July 1st, the following account, on the authority of Dr. Flint, is given of the propagation of typhoid fever; and an annotation is appended that "it would scarcely be possible to devise an experiment which would better test the communicability of typhoid fever by contagion than this":—

"A stranger, who had been sick for several days, stopped from off a stage coach at a tavern in a little village eighteen miles from the city of Buffalo, called North Boston, and, after a few days, died. The village consisted of nine families, closely grouped together around the tavern. All these families, save one named Stearns, used water from a common well, and visited the sick stranger at the tavern. Prior to the arrival of this stranger there had been no typhoid fever there, but twenty-three days after his arrival a member of the landlord's family was taken down with the disease. Other cases quickly followed, and in a month more than one half of the population, numbering forty-three, had been affected, and ten had died. Stearns' family, being on ill terms with the tavern keeper and most of the other families, who were his tenants, did not visit the sick stranger, and had been forbidden the use of the common well, and as they alone, of all the families immediately surrounding the tavern, escaped the disease, he was accused of poisoning the water of the said well—a charge which chemical analysis showed to be entirely unfounded."

Most certainly these facts conclusively prove the *communicability* of typhoid fever, but I hesitate to accept them, though with considerable diffidence, as any proof that the disease is *directly contagious*. An equally remarkable instance, which I shall presently relate, of the spread of the disease, came under my own observation three months ago, and seemed decidedly to prove that the disease had spread by personal contact; further investigation showed the contrary.

The theory which my own experience leads me to support is as follows:—1. That *almost* invariably the disease proceeds from a special poison contained in the alvine excreta. 2. That this poison is *directly* introduced into the alimentary canal, either in the food or, most generally, in the water drunk. 3. That it is not yet proved that the poison is contained in, or is disseminated by, the exhalations from drains, privies, &c., or that it can be absorbed into the system through breathing air contaminated with such exhalations.

The epidemic at North Boston lends support to my first point, and Dr. Flint himself says:—"The fact that all the families in which the disease prevailed were supplied with water from a common well, and the fact that the family in which no case occurred did not obtain water from this well, afford ground for supposing that a virus derived from the excreta was conveyed in the water drunk." Though the stranger died within a few days after coming to the tavern, it was not until twenty-three days after his arrival that the first case occurred. This is a long period of incubation, assuming that the disease was contagious, but not at all too long if we assume that the cesspool or drain into which the excreta was poured was leaky, and so allowed the poisonous material gradually to filter through the ground into the well.

The epidemic to which I have referred above as coming under my own observation occurred at *Harston*, a small village about five miles from Cambridge. Between the 1st of January and the end of April of the present year, no less than twenty patients were placed under my care in Addenbrooke's Hospital, suffering from typhoid fever. A large proportion, and those the most severe cases, were sent from Harston, where I learnt that whole families were being affected, and several deaths had occurred. I wrote to my friend Mr. Trestrail, asking him to investigate what I had heard respecting the introduction of the fever into the village, also for information about the wells and drainage, stating my belief that the disease was spreading through contaminated drinking water. He replied:—"I find that you are correct in stating that there is a distinct history of the introduction of typhoid fever into this village. It appears that a man who had barely recovered from the fever came as a lodger to Mrs. P., who resides in the village, and whose child was subsequently attacked. The second case was that of a Mrs. C., a patient of mine, who had been in the house of Mrs. P. occasionally. Mrs. C.'s sister was afterwards taken ill with typhoid fever. The next case was that of a child who had also been to see the child P. frequently. The next two cases were persons living very near; and then cases around them. Nearly all the cases have been near each other, where the houses are overcrowded—no gardens nor any proper drainage. It is usual for these persons to drink water from one of the *springs*. Now these springs are continually running at the rate of many gallons per minute; they come from upwards of 200 feet below the surface, and the water is conducted up from this distance by tubes. It is therefore highly improbable that these springs can become contaminated."

Here, then, we have the disease introduced into a district and spreading rapidly, the first persons attacked being in direct communication with each other, and the drinking water not liable to contamination. Evidently a clear proof of its contagiousness! But wait. In a subsequent letter Mr. Trestrail writes:—"I have discovered the cause of the spread of the fever—namely, there is running directly through the village a stream of water, into which nearly all the drains from the houses enter. The water of this stream has been generally used by the poor for cooking purposes, as the springs are at some little distance from their houses. Since the attention of the people has been called to the great importance of using only spring water I have not heard of any fresh case of fever in the village."

The outbreak of typhoid fever last year in Islington is strongly corroborative of the view that the disease spreads in the manner I have mentioned. As stated in the *Lancet* for Nov. 26th, 1870, at a certain dairy the milk cans were washed in water taken from a tank communicating with the drains, and a little water of course remained behind: there was no charge of gross admixture of water with the milk. The dairyman himself died of typhoid, and, of 140 families supplied with the milk, 70 contracted the disease. The majority of the cases occurred in close proximity to the dairy; but "in one long road a mile and a half from the dairy there were three families thus supplied with milk; two of these had typhoid in them, and they were the only houses in which the malady occurred."

Another point against the contagiousness of the disease is the rarity of its infecting, in hospitals, the physicians, nurses or the other patients. I have, however, seen this happen, and I have no doubt that such cases have led to the assumption that the poison is oftener introduced by the way of the trachea than by way of the œsophagus; but, to my mind, the simple explanation is, that after examining the rose spots on the back or abdomen, or after auscultating the chest, the hands of the physician have been imperceptibly soiled by contact with the skin or linen; as he continues his round he perhaps unthinkingly brushes his hand across his lips, and so the poison finds an entrance. With respect to the nurses, there is sometimes, after removing the soiled linen of the patients or the bed-pans, not that scrupulous attention to cleanliness which is so essential, and thus, eating or serving out the diets of other patients with unwashen hands, some little speck of fever-poison is introduced with the food. I say "in hospitals:" for in private houses the nurses and friends are often exposed to the same influence which caused the illness of the patient, and so may be infected, but not necessarily through the patient.

I would, in conclusion, make two practical suggestions with regard to the *treatment of typhoid-fever patients*:—

1. That every evacuation, as soon as passed, should be disinfected with carbolic acid.

2. That no surface-well water or pump water, whether boiled or not, should be used in a district where there are any cases of typhoid fever, but that all drinking water should be obtained from some spring not liable to contamination.

I need not say that this precaution is of the greatest importance to the patients themselves. I am quite sure that neglect of it gives rise to many of the relapses in typhoid fever cases, perhaps more frequently than any other cause except the too early administration of solid food. And with respect to this last point, though I am now going beyond the subject of my paper, I would just point out the supreme importance of the thermometer in telling us when solid food may safely be given. It should not be given until the temperature of the patient at 8 a.m. and 6 p.m. has remained, for two days at least, about the normal point, or between 98° and 99° F. The patient's tongue may be clean and moist, the appetite ravenous, the patient crying out for food, and yet the typhoid ulcers still unhealed. The thermometer alone will tell us this; it will probably show at this stage an evening temperature of about 101° F., with a morning temperature 1·6° to 2° lower; and a mutton-chop now might be sufficient to induce fresh irritation of the intestinal ulcers, fatal hemorrhage or perforation. It is only after the evening temperature has remained, on at least two successive days, below 99° F. that we can be sure the ulcers have healed, and that solid food may be given without risk.

Out of twenty patients mentioned as admitted into Addenbrooke's Hospital only one died; but had not careful thermometric observations been made, the mortality would, I think, have been greater. In two who recovered there was, after the fortieth day of the disease, considerable hemorrhage from the bowels, and this after the tongue had become moist and clean, the motions perfectly formed, and the patients for some days have been bitterly complaining at only being allowed beef-tea, milk, and wine. The evening temperature still rose as high as 101° F., and no solid food was given. If it had been, I have little doubt it would have turned the scale in each case against the patient's recovery.—*Lancet, July* 15, 1871, *p*. 81.

5.—ON THE MODE OF EMPLOYMENT OF SULPHATE OF QUININE IN SIMPLE (NON-PERNICIOUS) INTERMITTENT FEVER.

By Dr. Auguste Nonat, formerly Physician to the Hôpital de la Charité, and Vice-Professor at the School of Medicine, Paris.

[Sulphate of quinine is used too indiscriminately in the treatment of intermittent fever, without a clear idea as to the dose suitable to the case. Either large doses are exhibited where smaller ones would have sufficed, or small doses have been vainly employed and the use of the drug thrown up where large doses were necessary, and would have afforded satisfactory results. The following rules of practice are the result of much careful study of the subject, and by attending to them quinine may be exhibited with a certainty of success, and a cure often obtained in cases which had been given up in despair by others.]

The mode of employment of sulphate of quinine in simple intermittent fever must depend upon four main conditions.

1. We must take into account the *intensity* of cause, and therefore make inquiry concerning the locality where the disease was contracted: for instance, Algeria, Paris, Rome, Sologne; the cause being more or less powerful according to the infecting localities.

2. The oldness of the complaint must be taken into serious consideration. The older the fever, the stronger must be the dose of quinine.

3. The type of the fever. Thus quartan fever requires a much larger dose of quinine than the quotidian or tertian type.

4. *The degree of tumefaction* of the spleen. And, indeed, when the spleen

is not enlarged, *cœteris paribus*, the dose of sulphate of quinine should be smaller; and on the other hand, the larger the tumefaction of the spleen, the more must we increase the dose of the remedy.

In quartan fever the patient has two good days out of three; he may do his work, attend to his ordinary occupations, and not call in the doctor. Meanwhile the complaint becomes more intense—*vires acquirit eundo.* This, therefore, turns out an old case, and may be coupled with my second condition; namely, the *oldness* of the disease. Indeed, it is much on this account that the quartan type is less amenable to treatment than the tertian or quotidian.

We may therefore reduce to three the rules to which I have referred, and put it down that, in order to determine the due amount of remedy which it is necessary to exhibit, we must take into account *the intensity of the cause, the oldness of the disease, and the state of tumefaction of the spleen.*

Next comes the question of the amount of quinine which it is suitable to administer at the outset of the treatment. When called to the bedside of a patient laying under fever, by what dose must we begin? In a general way the dose will vary from 5 to 23 grains. The *minimum* dose will be administered in *recent* cases of simple intermittent fever unattended by tumefaction of the spleen; the *maximum* in *old* cases of intermittent, accompanied by considerable enlargement of spleen, whilst the intermediary doses will suit intermediary cases. But it should be borne in mind that *whenever* the spleen is enlarged, at least 10 to 12½ grains should be administered every day at the outset.

This remark leads to another important question; namely, the order of administration of doses with regard to strength, and the duration of the use of quinine.

Experience leads me to set down that the maximum dose must be immediately given to commence with, that it must be continued for four or five days, and that we must then go on gradually decreasing the dose, till complete resolution of the enlargement of spleen has taken place. Thus, for example, if we begin with 23 grains, I should say 23 grains for five or six days, then 15 grains for five or six days more, then 7½ grains for the same space of time, and so on. The total duration of exhibition of the drug will generally take about three weeks; still after this time however we must not suspend its use, but continue giving some small doses from time to time, in order to keep up the action of the medicament.

Another important point is the necessity of keeping up the patient's strength during treatment, and of giving him a proper amount of food. I have observed that sulphate of quinine is far better tolerated by patients who are properly fed. Thus, on the very first day, as soon as the access is stopped by quinine, I am accustomed to allow my patient his ordinary number of meals, or only to lessen them according to circumstances. The stomach puts up better with the remedy, and the gastralgic effects of quinine are thus much seldomer noticed than when diet is enforced.

The mode of the treatment which I have now described succeeds equally well with the quartan type as with the others, which shows that the quartan type proves refractory only because the treatment is not appropriate and consists of too feeble doses at the outset.

Practically, and to sum up; to cure intermittent fever we must exhibit sulphate of quinine in due amount and in proper order, to counteract the effects of the paludal poison, when there exists no complication, no inflammation, on the part of the spleen—but when this complication does exist we must master it by local applications.

A word in respect to arsenic. It may be broadly stated that arsenic is not equal to the sulphate in ordinary cases of intermittent. I have found it act on the cachectic state of the patient, but not on the local lesion, and therefore it leaves the patient exposed to a recurrence of the fever.—*Practitioner, Oct.*, 1871, *p.* 193.

6.—DUST AND DISEASE.

By Prof. JOHN TYNDALL, LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution of Great Britain.

After some preliminary experiments and observations on the polarisation of light by fine dust, by the sky, and the coarser particles of smoke, Professor Tyndall proceeded :—

In looking at this illuminated dust, we may ask ourselves what it is. How does it act, not upon a beam of light, but upon our own lungs and stomach? The question at once assumes a practical character. We find, on examination, that this dust is of organic matter—in part living, in part dead. There are among it particles of ground straw, torn rags, smoke, the pollen of flowers, the spores of fungi, and the germs of other things. But what have they to do with the animal economy? Let me give you an illustration to which my attention has been lately drawn by Mr. George Henry Lewes, who writes to me thus.

"I wish to direct your attention to the experiments of Von Recklinghausen, should you happen not to know them. They are striking confirmations of what you say of dust and disease. Last spring, when I was at his laboratory in Wurzburg, I examined with him blood that had been three weeks, a month, and five weeks, out of the body, preserved in little porcelain cups under glass-shades. This blood was living and growing. Not only were the amœba-like movements of the white corpuscles present, but there were abundant evidences of the growth and development of the corpuscles. I also saw a frog's heart still pulsating which had been removed from the body (I forget how many days, but certainly more than a week). There were other examples of the same persistent vitality or absence of putrefaction. Von Recklinghausen did not attribute this to the absence of germs—germs were not mentioned by him; but when I asked him how he represented the thing to himself, he said the whole mystery of his operation consisted in keeping the blood *free from dirt*. The instruments employed were raised to a red heat just before use, the thread was silver-thread and was similarly treated, and the porcelain cups, though not kept free from air, were kept free from currents. He said that he often had failures, and these he attributed to particles of dust having escaped his precautions."

Professor Lister, who has founded upon the removal or destruction of this "dirt" great and numerous improvements in surgery, tells us of the effect of its introduction into the blood of wounds. He informs us what would happen with the extracted blood should the dust get at it. The blood would putrefy and become fœtid; and when you examine more closely what putrefaction means, you find the putrefying substance swarming with organic life, the germs of which have been derived from the air.

Another note which I received a day or two ago has a bearing particularly significant at the present time upon this question of dust and dirt, and the wisdom of avoiding them. The note is from Mr. Ellis, of Sloane Street, to whom I owe a debt of gratitude for advice given to me when sorely wounded in the Alps. "I do not know," writes Mr. Ellis, "whether you happened to see the letters, of which I enclose you a reprint, when they appeared in the *Times*. But I want to tell you this in reference to my method of vaccination as here described, because it has, as I think, a relation to the subject of the intake of organic particles from without into the body. Vaccination in the common way is done by scraping off the epidermis, and thrusting into the punctures made by the lancet the vaccine virus. By the method I use (and have used for more than twenty years) the epidermis is lifted by the effusion of serum from below, a result of the irritant cantharadine applied to the skin. The little bleb thus formed is pricked, a drop of fluid let out, and then a fine vaccine point is put into this spot, and after a minute of delay it is withdrawn. The epidermis falls back on the skin and quite excludes the air—and not the air only, but what the air contains.

"Now mark the result: out of hundreds of cases of revaccination which I have performed, I have never had a single case of blood-poisoning or of abscess. By the ordinary way the occurrence of secondary abscess is by no means uncommon, and that of pyæmia is occasionally observed. I attribute the comparative safety of my method, first, to the exclusion of the air and what it contains and, secondly, to the greater size of the apertures for the inlet of mischief made by the lancet."

I bring these facts forward that they may be sifted and challenged if they be not correct. If they be correct, it is needless to dwell upon their importance; nor is it necessary to say that, if Mr. Ellis resigned himself wholly to the guidance of the germ-theory, he could not have acted more in accordance with the requirements of that theory than he has actually done. It is what the air contains that does the mischief in vaccination. Mr. Ellis's results fall in with the general theory of putrefaction propounded by Schwann, and developed in this country with such striking success by Professor Lister. They point, if true, to a cause distinct from bad lymph for the failures and occasional mischief incidental to vaccination; and, if followed up, they may be the means of leaving the irrational opposition to vaccination no ground to stand upon, by removing even the isolated cases of injury on which the opponents of the practice rely.

We are now assuredly in the midst of practical matters. With your permission, I will recur once more to a question which has recently occupied a good deal of public attention. You know that as regards the lowest forms of life, the world is divided, and has for a long time been divided, into two parties, the one affirming that you have only to submit absolutely dead matter to certain physical conditions to evolve from it living things; the others, without wishing to set bounds to the power of matter, affirming that in our day no life has ever been found to arise independently of pre-existing life. Many of you are aware that I belong to the party which claims life as a derivative of life. The question has two factors: the evidence, and the mind that judges of the evidence; and you will not forget that it may be purely a mental state or bias on my part that causes me throughout this discussion, from beginning to end, to see on the one side dubious facts and defective logic; and, on the other side, firm reasoning and a knowledge of what rigid experimental inquiry demands. But, judged of practically, what, again, has the question of spontaneous generation to do with us? Let us see. There are numerous diseases of men and animals that are demonstrably the products of parasitic life, and such diseases may take the most terrible epidemic forms, as in the case of silkworms of France in our day. Now it is in the highest degree important to know whether the parasites in question are spontaneously developed, or are wafted from without to those afflicted with the disease. The means of prevention, if not of cure, would be widely different in two cases.

But this is by no means all. Besides these universally admitted cases, there is the broad theory now broached and daily growing in strength and clearness—daily, indeed, gaining more and more of assent from the most successful workers and profound thinkers of the medical profession itself—the theory, namely, that contagious disease generally is of this parasitic character. If I had heard or read anything since to cause me to regret having introduced this theory to your notice more than a year ago, I should here frankly express that regret. I would renounce in your presence whatever leaning towards the germ-theory my words might then have betrayed. Let me state in two sentences the grounds on which the supporters of the theory rely. From their respective viruses you may plant typhoid fever, scarlatina, or small-pox. What is the crop that arises from this husbandry? As surely as a thistle rises from a thistle-seed, as surely as the fig comes from the fig, the grape from the grape, the thorn from the thorn, so surely does the typhoid virus increase and multiply into typhoid fever, the scarlatina virus into scarlatina, the small-pox virus into small-pox. What is the conclusion that suggests itself here? It is this:—That the thing which we vaguely call a virus is to all intents and purposes a *seed;* that in the whole range of chemical science you cannot point to an action which illustrates this perfect parallelism with the phenomena of life—this demonstrated power of self-multiplication and reproduction. There is, therefore, no hypothesis to account for the phenomena but that which refers them to parasitic life.

And here you see the bearing of the doctrine of spontaneous generation upon the question. For if the doctrine continue to be discredited as it has hitherto been, it will follow that the epidemics which spread havoc amongst us from time to time are not spontaneously generated, but that they arise from an ancestral stock whose habitat is the human body itself. It is not on bad air or foul drains that the attention of the physician will primarily be fixed, but upon disease-germs, which no bad air or foul drains can create, but which may be pushed by foul air into virulent energy of reproduction. You may think that I am treading on dangerous ground, that I am putting forth views that may interfere with salutary practice. No such thing. If you wish to learn the impotence of medical science and practice in dealing with contagious diseases, you have only to refer to a recent Harveian oration by Dr. Gull. Such diseases defy the physician. They must burn themselves out. And, indeed, this, though I do not specially insist upon it, would favour the idea of their vital origin. For if the seeds of contagious disease be themselves living things, it will be difficult to destroy either them or their progeny without involving their living habitat in the same destruction.

And I would ask you to be cautious in accepting the statement which has been often made, and which is sure to be repeated, that I am quitting my own *métier* when I speak of these things. I am not dealing with professional questions. I am writing no prescription, nor should I venture to draw any conclusion from the condition of your pulse and tongue. I am dealing with a question on which minds accustomed to weigh the value of experimental evidence are alone competent to decide, and regarding which, in its present condition, minds so trained are as capable of forming an opinion as on the phenomena of magnetism and radiant heat. I cannot better conclude this portion of my story than by reading to you an extract from a letter addressed to me some time ago by Dr. William Budd, of Clifton, to whose insight and energy the town of Bristol owes much in the way of sanitary improvement.

"As to the germ-theory itself," writes Dr. Budd, "that is a matter on which I have long since made up my mind. From the day when I first began to think of these subjects, I have never had a doubt that the specific cause of contagious fevers must be living organisms. It is impossible, in fact, to make any statement bearing upon the essence or distinctive characters of these fevers, without using terms which are of all others *the most distinctive of life*. Take up the writings of the most violent opponent of the germ-theory, and, ten to one, you will find them full of such terms as 'propagation,' 'self-propagation,' 'reproduction,' 'self-multiplication,' and so on. Try as he may—if he have anything to say of those diseases which is characteristic of them—he cannot evade the use of these terms or the exact equivalents to them. While perfectly applicable to living things, these terms express qualities which are not only inapplicable to common chemical agents, but, as far as I can see, actually inconceivable of them."

Once, then, established within the body, this evil form of life, if you will allow me to call it so, must run its course. Medicine as yet is powerless to arrest its progress, and the great point to be aimed at is to prevent its access to the body. It was with this thought in my mind that I ventured to recommend, more than a year ago, the use of cotton-wool respirators in infectious places. I would here repeat my belief in their efficacy if properly constructed. But I do not wish to prejudice the use of these respirators in the minds of its opponents by connecting them indissolubly with the germ-theory. There are too many trades in England where life is shortened and rendered miserable by the introduction into the lungs of matters which might be kept out of them. Dr. Greenthow has shown the stony grit deposited in the lungs of stone-cutters. The black lung of colliers is another case in point. In fact, a hundred obvious cases might be cited, and others that are not obvious might be added to them. We should not, for example, think that printing implied labours where the use of cotton-wool respirators might come into play, but I am told that the dust arising from the sorting of the type is very destructive of health. I went some time ago into a manufactory in one of our large towns, where iron vessels are enamelled by coating them with a mineral powder and subjecting them to a heat sufficient to fuse the powder. The organisation of the establishment was excellent, and one thing only was needed to make it faultless. In a large room a number of women were engaged covering the vessels. The air was laden with the fine dust, and their faces appeared as white and bloodless as the powder with which they worked. By the use of cotton-wool respirators, these women might be caused to breathe air more free from suspended matters than that of the open street. Over a year ago I was written to by a Lancashire seedsman, who stated that during the seed-season of each year his men suffered horribly from irritation and fever, so that many of them left his service. He asked me could I help him, and I gave him my advice. At the conclusion of the season this year he wrote to me that he had simply folded a little cotton-wool in muslin, and tied it in front of the mouth; and that he had passed through the season in comfort and without a single complaint from one of his men.

The substance has also been turned to other uses. An invalid tells me that at night he places a little of the wool before his mouth, slightly moistening it to make it adhere; that he has thereby prolonged his sleep, abated the irritation of his throat, and greatly mitigated a hacking cough from which he had long suffered. In fact, there is no doubt that this substance is capable of manifold useful applications. An objection was urged against the use of it, that it became wet and heated by the breath. While I was casting about for a remedy for this, a friend forwarded to me from Newcastle a form of respirator invented by Mr. Carrick, an hotel-keeper at Glasgow, which meets the case effectually, and, by a slight modification, may be caused to meet it perfectly. It consists of a space under a partition of wire-gauze, intended by Mr. Carrick for "medicated substances," and which may be filled with cotton-wool. The mouth is placed against an aperture, which fits closely round the lips; and the air enters the mouth through the cotton-wool by a light valve, which is lifted by the act of inhalation. During exhalation this valve closes; another breath escapes by a second valve into the open air. The wool is thus kept dry and cool; the air passing through it being filtered of everything it holds in suspension.*—*British Medical Journal, June* 24, 1871, *p.* 661.

7.—ON CANCER.

By Campbell De Morgan, Esq., F.R.S., Surgeon to the Middlesex Hospital.

What is the origin of Cancer? Does the disease consist solely in a local change of structure; or is it the local determination of a general morbid condition of the blood; or are the blood and the tissue equally interested in its formation?

It is not surprising that surgeons have come to regard cancer as from the first a constitutional or blood disease. Indeed, it is no easy matter to account for all that we observe in cancer without adopting some such view. But on the other hand, there are many facts which oppose themselves to it, and there are other modes of accounting for some of those characters which most strongly support it. I do not think that the importance of the question is in general so fully realised as it should be. Some, indeed, seem to consider that, practically, the question is not worth discussing. Yet, surely, if cancer be a constitutional disease, we must strive to find some corrective to the constitutional taint. And we should try in that case to determine whether the local disease is the result of some error in the organising or nutrient function of the tissues, or whether it is the actual deposit of some poison previously existing in the blood. If the former, our aim must be to find out the source of this error; if the latter, to learn the nature of the poison, and to be able to detect it. Then we might have a reasonable hope of being able to do, what has as yet proved beyond our reach—viz., to cure or even to modify cancer by general treatment. But if cancer be, at any period of its existence, a purely local disease, we should hope that by earlier and more complete removal better and more generally permanent results may be obtainable than we can at present boast of.

* *Mr. Ladd, of* **Beak Street, sells these respirators.**

Admitting fully the difficulties which lie in the way of arriving at a perfectly satisfactory conclusion, and notwithstanding the high authority of those who adhere to the more commonly received opinion, I may state that the more I consider the question, the more do I hold to the view which has been so ably advocated by my late colleague, Mr. Moore, and which is maintained by many in this country, and by Virchow and many German pathologists. That there may be a constitutional tendency to cancer I fully admit, but that it is in any sense to be regarded as a blood disease seems to me to require stronger evidence than we have yet obtained. I should say that cancer and allied diseases partake more of the nature of parasitic disease than of the results of previous blood-poison. The final destiny of such tumour will depend, not on any special chemical or nutrient element, but on the nature of the growth itself. This seems the view taken by Mr. Huxley. In his presidential address to the Association for the Advancement of Science (1870), he says, speaking of xenogenesis, "It is only in pathology that we find any approximation to true xenogenesis; and it is furnished by the various structures in which, under the influence of certain external conditions, elements of the body which should have developed in due subordination to its general plan, set up for themselves and apply the nourishment they receive to their own purposes. From such innocent productions as corns and warts there are all gradations to more serious tumours, and in the terrible structures known as cancers, the new growth has acquired powers of reproduction and multiplication, and is only distinguished, by form, from the parasite worm, the life of which is neither more nor less closely bound up with that of the infested organism."

To a certain extent—to a great extent indeed—I admit a constitutional tendency to this disease; but I admit equally a constitutional tendency to warts, or to fatty and other simple tumours. I cannot suppose that there is any material in the body which must work itself out of it, and which selects this situation or that indiscriminately, or perhaps makes use of one in preference to another from finding a tissue better prepared for its reception. I cannot suppose that there are various kinds of this morbid material—one breaking out as scirrhus, another as epithelioma, &c. If the cancer poison be of one kind only, we should surely find it more frequently appearing in the same subject at one time as encephaloid, at another as scirrhus, at another as epithelioma, or some other form of malignant disease. Yet this is seldom the case, though it does occur. I quite admit that the nature of a cancer will be modified by the character of tissue in which it appears, and by the general activity of the tissues. We may find this in gout, or rheumatism, or syphilis. But the sufferer from either of these diseases will be liable to attacks of it at different times in every possible form, or in many forms at once, and this we do not so often find in cancer. Other general or blood disease, as the exanthemata, will produce structural change only in certain tissues. Now, however mild and however limited the local manifestations of any recognised blood disease may be, we find evidence of a general affection of the system as well. We cannot say as much for cancer.

We may consider in detail those special characters which have led to so firm a belief in the blood origin of cancer. Perhaps that which seems to be the most conclusive evidence to the large majority of those who hold this view is the almost constant recurrence of the disease after removal. Entire and permanent immunity does occur, but it is undoubtedly rare. In such cases it would be said, first, that the blood-poison having worn itself out, or been eliminated by the formation of the tumour, no further development took place when the local tumour was removed; or, second, that two conditions were necessary for the formation of cancer—one, the blood-poison; the other, a fit state of tissue in which it might manifest itself; and that, a tumour having been removed, there remained no fit nidus for its reproduction. The first view is that taken by Mr. Simon, who considers that the tumour stands in the same relation to the *materies morbi* as a secreting gland does to the matter eliminated by it. The second view is that maintained by Mr. Paget. It need scarcely be remarked that these two conceptions necessarily lead to opposite modes of practice; for if the tumour be an eliminative organ it would be undesirable to remove it at all, as the disease must find *some outlet; and many* surgeons, acting on this view, object to early operation. But if it may be that on the removal of a tumour no other tissue is in a condition to take on the diseased action, it would be well to operate early if only on the chance of such a want of consent between the blood and the tissues.

These cases of permanent cure after operation are, we may admit, exceptional. The more common event is that, after removal of a tumour, in favourable cases there is immunity for a longer or shorter period—some months or some years. Is the blood diseased during this time? Are the other tissues free from disease? It is most important here to consider the mode of recurrence of the disease. It is a recognised fact that cancer rarely returns either in an organ corresponding to its original seat, or indeed in any organ which is the usual seat of the primary disease. A cancer in the breast, *e.g.*, rarely returns in the opposite breast; so in the testicle, or the eye, or the side of the lip. Primary scirrhus, again, is rare in the lymphatic glands, in the integuments of the trunk or limbs, in the lungs; while its by far most common seat is the female breast or the uterus. Now, supposing that recurrent cancer is a development, *de novo*, the result of a diseased state of blood in combination with a fitting tissue, should we not expect that very often the return would be in an organ which is the common seat of the disease? Take, for example, an excision of the female breast. The patient remains well for two years, and the disease then returns. If this is a new development dependent on constitutional causes, it would be strange indeed if we did not often find it taking place in the other breast, or in the uterus; still stranger that it should occur in nineteen cases out of twenty in tissues or organs which are rarely the seat of primary cancer. Yet such is the fact. Mr. Sibley, in his careful analysis of 520 cases of cancer observed in the Middlesex Hospital, did not find one in which a recurrence of the disease took place in the ordinary seats of the disease. Surely this fact alone should make us pause ere we admit that a cancer-poison in the blood is one of the factors in the production of cancer. I quite admit the force of the argument that an operation may so change the tissue of the part operated on as to make it a fitting seat of future disease. But this will only apply to those cases in which the return is in or around the cicatrix, not to those where distant lymphatics or lungs or other internal organs are affected, the cicatrix remaining sound. And still the difficulty of accounting for the fact that, with a cancerous state of blood existing for a length of time, no recurrence takes place in the organs most prone to the disease would remain the same. The influence of local condition must be at least more powerful than that of blood.

And during all this time, as well as before the discovery of the original tumour, the patients are usually in robust health. Nothing can be more erroneous than the belief entertained by those who have not had much experience of the disease that there is a cachectic condition in the early stages of cancer. To use the words of Professor Humphry, "So much is said and written about cancerous cachexia, that cachexia comes to be regarded as a necessary associate of cancer. Hence, physicians and surgeons rely upon it as a means of diagnosis, and conceive that a disease cannot be cancerous because the patient's health is good. Whereas in reality, cancer, especially in early and middle life, fastens itself often, I would say oftenest, upon those who are well-nourished and florid, who seem the most healthy and robust, and so give promise of long life and vigour." This point is of the utmost practical importance; yet it is generally ignored, or rather the opposite opinion is generally entertained. Tumours are discovered, and are allowed to grow till too late for removal, the patient, or perhaps the medical attendant, considering that there was nothing of consequence in them because there was no appearance of cancerous cachexia. In reality, however, the fact that a tumour is developed in the breast of a person in otherwise sound health, and over thirty—a tumour painless, and only discovered when already of some size,—is enough to justify a more than suspicion that it is cancer. If cancer should ever be removed by operation, I suppose few will deny that the sooner it is done the better. But what chances of successful removal are daily thrown away owing to the presence of those very conditions which should most surely excite suspicion in the mind of the surgeon. I hope I may not be misunderstood in what has been here stated on this point. It refers to the large majority of cases. There is no doubt that cancer does attack those who have been delicate, and have suffered from one complaint or another all their lives; but even in such persons there is usually nothing from which one could predicate the after development of cancer as one usually can in the states of health leading up to tuberculosis.

There is another circumstance in connection with the recurrence of cancer after operation, which to my mind, is very significant. I have noticed, and it has been verified by the observation of many others, that concurrently with or following on the development of cancer, small outgrowths of warty, or vascular, or dermoid structure are frequent. Now, one would imagine that, if there were a cancer-poison in the blood these or one of them would become the seat of the disease. But it is never the case, although a large outbreak of cancer has taken place in other situations, such as are the usual seats after operations, but the rare ones independent of them. The frequent co-existence of cancer with other growths will be again alluded to.

The hypothesis of the double origin of cancer, a state of the blood and a state of local tissue fit for its invasion, presents another difficulty. It implies that this diseased blood-condition may exist, and be long present—indefinitely long for aught we know,—without any, even the slightest deviation from robust health. Something allied to it may be seen in syphilis, but with a difference. A person may have a syphilitic taint, which will not for a length of time have any external manifestation. But should this person have an accident, or be in any way put out of condition, the influence of the poison will display itself. In cancer, on the contrary, whether previous to its recognised development or while it is present, or in the interval between operation and its recurrence, up to the time when cachexia has actually set in, any injury, a wound, a bruise, a fracture, may occur, and no influence of the disease will be manifested. Unless in the case of syphilis—and even there the presence of sound health rarely accompanies its presence in the system,—all blood diseases will be attended by a state of deviation from sound health, even when there is no external or local expression of disease.

There are other and stronger grounds perhaps for considering that the persistent re-development of cancer is no proof of the presence of a blood-poison. There are few forms of unencapsuled tumours which do not from time to time exhibit precisely the same tendencies to reproduction which we see in cancer itself. Fibroma, enchondroma, myxoma, sarcoma, myeloid, &c.—all may show the same tendency, though no trace of cancer structure can be detected. In many forms of tumour—as the recurrent fibroid, for example—the disease after apparently complete extirpation, returns only in the original neighbourhood. It may be remarked, too, that a long period may elapse between the operation and any appearance of return. But often we have a general dissemination as widespread as we ordinarily find in recurrent cancer. Thus, in a case of fibro-plastic tumour which I brought before the Pathological Society, there were secondary tumours in the liver and kidney, and the lungs were remarkably studded with them. These secondary growths in the lung were all encapsuled, and in no place could any trace of cancer structure be found. In another case, recorded in volume xx. of the Pathological Society's Transactions, a widespread deposit of cartilaginous and of spindle-cell tumours in the abdomen and chest followed the removal of a cystic disease of the testicle two years previously. The original tumour contained cartilage-nodules, and was perhaps, as Virchow has pointed out, originally an enchrondroma. No cancer structure could be found. Such cases are by no means rare: indeed, they are now so universally recognised that it is hardly necssary to do more than call attention to them.

Although, however, we have this widely recurrent dissemination in tumours not cancerous, there is often, it must be admitted, a difference in the seats of recurrence. In the fibrous or cartilaginous, or other non-cancerous growths, the secondary tumours appear in the line of the absorbents leading to the more central parts, as in the neighbouring lymphatic glands, or those still more removed, or else in the direct line of the venous circulation, as in the lungs or liver. But we rarely, *if ever, find* recurrent non-cancerous growths in organs out of the direct line—if we may so speak—of infection. Thus we should not expect to find a myeloid or a sarcomatous tumour of one limb followed by a similar outgrowth in the brain, or the muscles of a distant part, or in one of the other limbs. In cancer, scirrhus and encephaloid particularly, we are not surprised to find recurrence in any situation or in any tissue. This is, after all, a question of degree. Epithelioma is as truly cancer as scirrhus, yet we find very little disposition in it to contaminate distant organs. The neighbouring lymphatics and the parts surrounding them are the chief homes of secondary deposit and growth. It must be admitted, too, that cancer gives rise in its later stages to a cachexia far more marked, in proportion to the amount of actual structural disease, than is ever seen in the case of non-cancerous growths. But, as we shall see, the cancerous cachexia is the result for the most part of cancerous ulceration.

No doubt exists in the mind of any pathologist as to the mode of recurrence in many tumours, cancerous or non-cancerous. The diseased structure finds its way into the absorbents or the veins. In the former it lodges and grows; in the latter it is carried away and deposited just as other minute emboli would be, and sooner or later grows at the seats of arrest. Mr. Paget relates a case in which the matter of an enchondroma was found in the lymphatics of the testicle, and had made its way by ulceration into the interior of the vena cava; the lungs were studded with secondary enchondromatous deposits; many small shrub-like growths were also attached to the inner membrane of the branches of the pulmonary artery. Nothing could be clearer than the course of the disease in this instance: a cartilaginous tumour in the testicle; cartilage-growth traced up the lymphatics of the cord; one of these growths ulcerating into the cava; particles of this growth washed away in the current of the blood; some adhering to the pulmonary artery, others forced onwards to the smaller ramifications, arrested and forming encysted nodules of the disease. We have, in fact, the ordinary conditions of embolism; but the emboli are living and growing, instead of being mere inert exudations. It is far easier to explain the recurrence of disease, whether cancerous or not, in such cases, than in those which, without ever extending to distant organs, pertinaciously refuse to be eradicated from the site of their first development. Take keloid or some form of recurrent fibroid for example, which cannot surely be reckoned amongst blood diseases. In the former, especially, the whole course of the disease is so purely local that we could no more attribute it to a general cause than we could a wart or an atheroma or a nævus. The cicatrix keloid of Dieburg is frequently associated with the removal of some innocent tumour, as chronic mammary. As the wound heals, which it may do readily, the cicatrix assumes the peculiar character of keloid. The points where sutures have been inserted take on the same character. The affection increases up to a certain point, and there stops. Remove it, with a large amount of the healthy skin around it, and as healing takes place the keloid forms. Here, then, we have a condition dependent on the cicatrisation of an apparently healthy skin; stationary, or nearly so, when once formed, but recurring after removal, however freely the operation may have been performed. But while it will take place on cicatrices in some situations, wounds in other parts may heal without any appearance of the disease. Save in the disposition to recurrence in a cicatrix, there is no point of similarity to cancer or other malignant disease. The course of some recurrent fibroid tumours is very similar, but in them there is a far greater tendency to extension and to recurrence at a distance from the original seat of disease, yet still within narrow limits. Now, although these could under no circumstances be regarded as blood diseases, but must be the results of some peculiar state of tissue, the tendency to recurrence is, I believe, far more difficult of explanation than it is in such diseases as recurrent enchondroma, where the morbid structure can be traced into absorbents and bloodvessels, or as cancer itself, where, besides in these, it can be found spreading in the connective-tissue spaces.

[Mr. De Morgan enters at some length upon the dissemination of cancer.]

We may start from the recognised physiological fact that every tissue has its own special power of reproduction. The blood brought to the tissues is the *same; the tissue uses what it* requires, and produces its like. I do not enter on the question of where, in the tissue, the power resides—whether in muscle alone or in cell. The experiments of Ollier have demonstrated that the scrapings of the soft inner layer of periosteum will form bone-nodules in whatever tissue they may be implanted. The recent practice of transplantation proves that epithelial cells planted on granulations will germinate and form layers of new epithelium. It is in tissues of low vitality and great tenacity of life that such experiments succeed; but they are sufficient to prove the fact. Hence we should expect that in cancer this growth from transplantation would be even more likely to occur, as it exceeds other tissues in the low vitality and tenacity of its elements. Intentional experiment has, of course, not been made on the human subject; but unintentional experiments have not been wanting. Several cases are recorded of men, whose wives have had uterine cancer, having themselves been victims of cancer of the penis. Of this I met with another instance only a short time ago. These *may* be mere accidental coincidents. They probably are, for there is no sufficient proof that cancer may be communicated from one person to another. Further evidence is still required on this point. There is no want of evidence of auto-inoculation. My colleague, Mr. Shaw, attended a patient in whom a pendulous breast, the seat at its lower part of ulcerated cancer, rubbed against the skin of the thorax. At the point of contact cancerous ulceration took place, the intervening skin between this and the fold of the mamma remaining healthy. Dr. Reineke has related two cases in which abdominal cancers were tapped, in error, with a trocar, and cancerous growth took place in the abdominal walls along the track of the puncture. Cancer is constantly found in the serous cavities at points opposed to those which have been the previous seat of the disease; and surgeons are familiar enough with the fact that, whenever a cancer is cut into in operation instead of cut out, the neighbouring parts become at once the seat of widespread disease. Dr. Moxon showed at the Pathological Society a very interesting specimen, in which it was clear that numerous small nodules of epithelial cancer, occupying the lower lobes of the lungs, had been transplanted from a primary growth in the trachea. Dr. Dickinson lately brought a specimen to the same Society, showing numerous outgrowths in the peritoneum from the bursting of a spindle-celled sarcoma into the cavity. Nor does it, I think, admit of doubt, that in a case of my own, where encephaloid tumour within the cranium was followed by small cancerous growths at the lowest part of the cerebro-spinal sheath, the same kind of migration and implantation had taken place. The fact of the auto-inoculation of cancer and allied diseases is clearly established, and is now generally admitted.

What thus takes place on surfaces has been shown to occur within the tissues. That the absorbents and blood-vessels take up and disseminate cancer is admitted. There is as good evidence that the same may occur in any space through which minute particles can travel. The original observations of Van der Kolk have been confirmed, that dispersed cancer-cells may be found in the connective-tissue spaces of the gland to a considerable distance from the parent tumour. The same may be found in the connective tissue of fat. If careful sections be made around and beyond the cancer tumour, cells similar to those found in the tumour may be seen clustered here and there, and lying free, in the areolæ of the connective tissue. This occurs in parts which appear to be perfectly healthy, and it shows that we may, and probably do, often believe that in operation we have got beyond the range of disease when in reality these germs have spread beyond our reach. I may recall one case which illustrates this. In that of the patient just referred to as having had encephaloid at the lower part of the spinal cord, the original disease was an intraocular cancer. The eye had been extirpated, and it was believed that the operation had been effected beyond the range of the disease. But the cancer rapidly returned; and on carefully examining the stump of the optic nerve in the eye which had been removed, Mr. Hulke found that lying in the meshes of the delicate connective tissue, between the outer fibrous sheath of the nerve and the mass of nerve-filaments, were small clusters of cancer-cells, and these were traced up to the point of section of the nerve. Of course they did not end there, but extended into the nerve beyond the point of section; and hence the speedy return. The nerve, be it remarked, appeared to the naked eye perfectly healthy.

If now we look to clinical cases, we constantly find evidence of the fact of this travelling of the cancer elements to a distance from the parent tumour. A very common phase of cancer is this:—A tumour is seated in the mammary gland, and the skin becomes adherent and puckered. The tumour does not tend to grow faster than is usual; but at some distance from it, from half an inch to three or four inches, small hard tubercles will be developed in the skin. There may be only one or two at first. The surrounding skin will appear to be soft and natural. By and by more will appear, and will perhaps coalesce and form broad hard plates. No surgeon would ever think of operating when these outlying tubercles have appeared. He would know that the disease would return at once in the seat of operation.

It may be said that I am begging the question in taking this fact as illustrating the dissemination of cancer from a parent stock. When, however, it is considered that the fact of dispersion is proved in the dead-house and by the microscope, that primary scirrhus of the skin is extremely rare, that here we have it developed in a number of separate nodules around an original tumour, I imagine that no one would be disposed to deny that these outgrowths were from germs thrown off from that original tumour.

We have, then, three channels through which cancer may diffuse itself—the lymphatic system, the vascular system and the interstitial system of the various tissues; and through one or other of these channels we may conceive that any part of the body may be sooner or later infected by the migration of some of the cancer elements. Even admiting this, however, there are difficulties in the way of accepting the doctrine that cancer is at one period of its life a purely local disease. How account for the simultaneous development of cancerous growths in various parts of the body? Why the almost constant return after removal? Why should an operation give immunity for years, and then the disease return? Why should cancer sometimes retrograde and disappear, especially when another disease is making progress?

The simultaneous development of multiple cancerous tumours has been already alluded to. Evidence is wanting of the existence of such a case. Indeed it is scarcely capable of proof; for how can it be shown that in no part of the body was there a tumour existing before these multiple ones appeared? The rapidity with which melanosis and encephaloid will be disseminated, when once a tumour has formed, is well known. I have mentioned a case in which, on the removal of a solitary encephaloid tumour on the sole of the foot, a host of similar tumours were rapidly developed in the whole limb and lymphatic glands. Until some undeniable case of the multiple simultaneous development of primary-cancer can be produced, it is needless to argue the point; more especially as, even then, it would be no conclusive proof of blood-poison, any more than would a simultaneous eruption of warts.

The next question is—why the almost constant return after removal? The conditions of cancer, viewed by the light of recurrent disease, give us a solution. Enchondroma, fibroma, &c., we find, return after removal in distant parts. We have the evidence that these structures may be carried by the lymphatics or blood-vessels, and when arrested at any point may grow. If this may occur in the case of structures so coarse (if one may use the expression) as those named, how much more likely would be its occurrence in connection with a structure the very essence of which is absence of coherence of its elements. It may be illustrated by the difference we see when a recurrent non-cancerous growth and a true cancer are cut into. In the former case, in order to get at any of its component parts for microscopic purposes, we must cut off or scrape up a portion of the tissue. In the latter, the cells, which constitute the active part of the cancer, exude on the cut surface, which presents a creamy character. Nothing is more easy to conceive than that the elements which compose this creamy matter, lying free and without limitary membrane, in the midst of soft tissues, would be taken up by them, by their lymphatics or blood-vessels or connective tissue spaces, and be carried anywhere and everywhere through the body. Principally, one would expect to find them within the range of the primary tumour, on the same side of the body; and so, in reality, we do. But there is no real limit. Just as we find that when a paper of pins is swallowed they may course through the body to any extent, upwards or downwards, or in any direction—so, only infinitely more readily, might these minute germs of disease travel to any extent or in any direction. The differences observable in the various forms of cancer offer a support to this view. Take the four forms of rodent cancer, epithelioma, scirrhus, and encephaloid. It is, I am well aware, a disputed point whether rodent ulcer is really cancerous. I believe it to be so, as the structure of the deposit around the ulcer is like that of epithelioma, and on account of its strong tendency to recurrence. Its elements are, however, very localized—it does not travel usually even to the neighbouring glands. Epithelioma, of which the elements are grosser and more coherent than those of the higher forms, will affect the neighbouring glands, but will not readily pass beyond them, though it *may* do so, and give rise to secondary growths, just as will scirrhus. Scirrhus readily affects the glands, and will contaminate distant parts, but with nothing like the rapidity that is often seen in encephaloid, the structure of which is far softer, and the fluid more abundant, and laden with minute elements, nuclear and granular; yet it is found that of all the true cancerous diseases encephaloid is the one which may go on to spontaneous cure or to long quiescence. This would appear to militate against the view that cancers contaminate in proportion to the fluidity of their contents. But an explanation is found in this: encephaloid is often the least infiltrating of cancers; it pushes the tissues aside, it forms connective-tissue capsules around it, and hence in a large number of cases it is not brought into connection with the tissues, as scirrhus is, so as to be disseminated amongst them. And this very fact that the most malignant of cancers, the most rapidly growing, and the most destructive when it is disseminated, is often encapsuled, often non-infiltrating, and often less venomous than other forms, confirms the notion that it is not to constitutional but to local conditions that we must attribute the malignancy of the disease.

The laxity of tissues and the activity of vital action going on in them exercise a marked influence on the dissemination of cancer. In young persons—in those in whom there is much fat—tumours grow more rapidly, and are more quickly and widely disseminated. I have removed cancers from the midst of abundant fat, where minute points of disease have been traceable in all directions around, to the distance of from two to three inches. Surgeons know that they cannot take the fat away from around a cancerous breast too widely. Surely no one would consider these as evidences of blood-disease setting up cancers *de novo*, considering that the fat is not a tissue in which primary cancer is formed.

[Mr. De Morgan, from careful inquiry into the histories of private patients, is inclined to consider the disease as more frequently hereditary than does Mr. Paget.]

I should place cancer, then, at the top of a scale, at the lowest point of which might be placed the simplest forms of outgrowth identical in structure to the parts amongst which, or from which, they grow. In all cases I should regard the morbid product as the result of undirected or ill-directed growth force, the elements of the structure thus formed possessing, as the natural elements do, the tendency to produce their like. Consequently, I should consider that a special condition of blood had no more to do with the formation of a cancer than it had to do with a lipoma or an adenoid.

It may be asked, what is practically gained supposing these views are established? Little or nothing, I admit, with our present knowledge, except this, that we should know the direction in which we should work. In the first place, it is clear that if removal be the practice adopted—and I believe that in all fair cases it is the proper one—the earlier the operation is done the better. This I consider a positive rule. It is true that very early operations are often very unsuccessful, and late ones very successful. But the tumour itself will often tell us why. I have removed tumours from the breast when small, and only just discovered, and have found them highly vascular and juicy, and the tissue around containing minute dark specks of cancer. Such tumours would be very likely to return. Others, which have existed a long time, are hard, circumscribed, contracted, with no glandular infection. In such cases the most satisfactory results might be expected from operation, provided always the operation is widely performed. In cases like those first mentioned the operation might be successful, if the parts around were as extensively removed as they would be in the case of a large tumour existing. I believe I may state from my own experience that the most successful results of removal have followed early and wide operations. Even when the axillary glands are affected, and are removed freely, the patients have lived for many years without return of the disease, as they will at times when enlarged glands are left untouched. And I have no doubt that the use of agents which coagulate the albumen to some depth, and at the same time, or in consequence, act as antiseptics, are beneficial. I still use the chloride of zinc, as I know of none which fulfills these conditions better.

The hypothesis of the local nature of cancer precludes the notion of general treatment with a view to alter the condition which produces it. I do not doubt that by general as by local means an impression may be made at times on the rate of progress; but I am quite sure that no system of internal or external treatment hitherto discovered has the slightest *special* effect. And I do not say this without experience; for, in the hopeless state we are in with regard to the treatment of the disease, I have felt it my duty to try many of the remedies which have from time to time been lauded. They have all done good in one way: they have made the patient hopeful for a time, and that is something. But, so far as any real effect on the disease was concerned, pump-water would have been as useful. One mode of treatment has occurred to me; but I have not had the courage to try it, because I could not answer for the results. They might be unfavourable. Cancer is for the most part, and at its commencement, a disease of the healthy and well nourished. As a rule, I believe that it is best in the early stages rather to keep the patient on a restricted diet than to encourage them to take largely of food and stimulants. They have less pain; and the tumour is, I believe, less active. Could the disease be starved out? Many years ago a gentleman of strong health and iron will told me that he suffered severely from hemorrhoids. He disliked the idea of operation: and was told by his medical man that if he would starve himself he might obtain a cure. For weeks he kept himself at starvation-point, or nearly so, and the tumours all became absorbed; nor has he been troubled with them since. Hemorrhoids are not cancers; but the example shows that when the body is feeding on itself it does not spare adventitious growth. One of the most distinguished men in this country, since dead, told me that his wife had had cancer of the uterus. He kept her for a length of time on the sparest vegetable diet—just enough to keep body and soul together. The disease disappeared. Years after the cancer reappeared, and destroyed her; but they were then living separate. May the antagonism occasionally observed between tuberculosis and cancer be dependent on the fact that tuberculosis is associated with waste of tissue? These are the slightest possible grounds on which to suggest a mode of treatment which might prove injurious instead of beneficial. Still I should like to see the experiment tried. It might be made in the case of melanosis in the horse, though I do not know whether that is a true cancer. Still the effect on an abnormal recurrent growth would be worth nothing. In order to be efficient it must be tried in a healthy animal, before any general effect on the system is produced by the disease.—*Lancet, July* 1, 8, 15, *and* 29, 1871, *pp.* 6, 41, 80, 155.

8.—ON FUNCTIONAL MEDICINE.

By Dr. WILLOUGHBY F. WADE, B.A., Physician to the General Hospital, formerly Professor of the Practice of Physic in the Queen's College, Birmingham.

In practical medicine, as in morals, it is well to have a lofty ideal. If we aim at a high mark we shall shoot higher than if we aimed at a low one. But in *both there is* a danger in having too lofty an ideal. We feel so utterly unable to reach that which we profess to ourselves we ought to reach that we do not long continue to aim at it. This is the case with many young practitioners of medicine. They set out impressed with the advantages of minute investigation, of accurate research, and of exhaustive diagnosis by those various means and instruments which they see used in hospitals. They find afterwards, when pressed by the cares and fatigue of a large practice, that they are almost compelled to forego their use; they find also that in many cases they get on pretty well without them, and thus fall into the habit of neglecting them, even in those cases where their use is, for purely practical reasons imperative. They thus in course of time dissociate themselves from scientific medicine, which they come at last to despise. That this conclusion is actually arrived at by a large number of practitioners, many of whom do not hesitate to avow it, I appeal to the experience of every one; and I think I am not wrong in the genesis to which I have attributed it. I have long been convinced that this state of things cannot be rectified by the earnest and genuine exhortations of teachers. If it could have been, it would never have existed. It is the result of too high an ideal and the inexorable logic of circumstances.

The ideas or principles which may, I think, eventually lead to an alteration of this unsatisfactory condition may be summed up in the term "Functional Medicine." In explanation of this term, and of the ideas involved in it, I request your attention to the following considerations. What I mean by it is this: that whenever we come to treat a case, to prescribe drugs or particular diets, rest or action, we should first of all consider what function of the body it is that is improperly performed. To the setting right of that function we should address ourselves. It may be, and indeed generally is the case that more than one function is (it may be several are) astray. We have, then, further to consider whether it is possible or convenient to attempt to rectify all these at once; and, if not, we have to decide which we should begin with.

Now this method has several recommendations. Amongst the first is, that it is not too high an ideal to be constantly aimed at. In a large number of cases it is closely allied to the principle which, as I have told you, many persons act upon—namely, the treatment of symptoms. Because every symptom of disease arises from the imperfect discharge of some function by its appropriate organ. Hence it requires only a slightly higher order of thought than that which is commonly in vogue. Indeed, many persons who profess only to treat symptoms do rather aim at the treatment of functions. They misdescribe their principles, and so do themselves an injustice. Now, though this admission may indicate that the principle I am laying down is comparatively wanting in novelty, it is strong testimony to its practical utility. Practice would be much more imperfect than it is if all those who profess to treat symptoms really contented themselves with doing so. For this reason: if we treat a symptom merely, we often fail to remove that which causes the symptom. To give an extreme instance: If a man has a thorn in his hand, and we merely order opiates for the relief of the pain thus produced, we are very inadequately treating the disease, though we may be adequately treating the symptom. In some cases treatment based upon such a principle may actually aggravate the disease. You will remember that in my last lecture, which was upon certain urinary deposits, I pointed out to you that lithiuria sometimes produces impotency; and I mentioned to you that persons thus suffering, in whom the cause of the disorder had not been recognised, may, and are advised to, take tonics and live freely, both of which, and particularly the latter, have a direct tendency to aggravate the lithiuria, and so to increase the disorder they were intended to cure. Treatment of this kind is directed to the symptom—impotence,—because in some such cases depending upon other causes this treatment may be beneficial. If, then, every person who now professes to proceed upon the plan of treating symptoms would in each case profess to proceed upon the plan of treating functions, there would be a great gain to practical medicine. Miscarriages of treatment of the class I have pointed out—and they are very numerous indeed—would be avoided. Further, this principle would bridge over the lamentable chasm which now divides the science and art of medicine. Accomplished pathologists and morbid anatomists, whose true knowledge of disease has justly led them to recognise the incurability of many organic diseases, would enhance the benefits they have conferred on mankind if they would condescend to rectify those functions of which the apparatus is not totally disorganised, and thus much misery would be averted, and many lives eased and prolonged. On the other hand, those who find by experience the limited control which we can exercise even over functions, and the extreme difficulty with which they can in some instances be influenced, would gladly hail any assistance which science might afford them by its most recondite researches, when these throw any light upon the laws which govern the growth of cells, the distribution of the blood, or other mysterious acts upon which the discharge of function depends.

Another advantage is this: persons who got as far as I suggest would get further. It is impossible that any of you who have paid attention to cases in the hospital, and have watched or assisted in their investigation, can have failed to be struck by the fact that one and the same disorder of a particular function may in different cases arise from very different causes. Take, for example, epileptic fits. You have doubtless seen them, in the surgical wards, caused by external injury; in the medical wards, you have seen them dependent upon the presence of worms in one case, upon disorder of the stomach in another case, and upon other causes in other cases. Now, knowing this, and resolving to treat not merely a symptom, which in the case of epilepsy is usually done by a miscellany of reputed specifics, you would naturally be led to examine into the case closely, and discover, if you could, the function which was permanently disordered, and the cure of which disorder (often lying very deeply hidden) would cure the more apparent and prominent phenomena, of which only the patient complains. Many cases of epilepsy may in this way be relieved. And remember this, that any specific must influence some function, though we may not know what function is disordered, still less the organ by which it is performed. In illustration of this, take the case of potassic iodide and secondary syphilis. A man has syphilis, and some time afterwards skin eruption, or sore-throat, or falling of the hair, or painful swellings, or enlarged glands, or albuminuria, or anæmia, or iritis, or dry pleurisy, or simple cachexia, or a combination of these symptoms. We give him potassic iodide. The symptoms disappear, and there is no evidence that his health is not as perfect as in his pre-syphilitic period. Twelve months after, some of these symptoms return. What do we infer from this? We assume that these symptoms arise from a poison in the system. If so, there must be some organ which manufactures this poison, and for a season ceased to do so; or else there must have been some organ which for a period ejected the poison from the system, and which after a time ceased to do so; some organ, therefore, the peculiar function of which is deranged. What this organ is we know not—still less in what way its healthy action is modified. But the effect of the iodic salt has undoubtedly been to restore that function to its normal condition. So you see that Functional Medicine perfectly well admits of the employment of such a specific, though the discovery of its powers may have been purely accidental and its *methodus medendi* be still unknown. Examples of the alteration of one function by the disorder of another might be multiplied almost infinitely. Take bronchitis only. Upon what an immense variety of causes, direct and indirect, may it not depend. Its treatment may or may not be influenced by our knowledge of its distant origin. It does not at all follow of necessity that the physician who treats it with simple expectorants, after, by minute inquiries, discovering many complications, is in error, though you may have thought so from my remarks in an early portion of this lecture. You see now that, in my opinion, Functional Medicine incorporates all the benefits of the imperfect method of symptom-treatment, and moreover leads directly on to the recognition and employment of the most precise, searching, minute, and scientific methods.

Another great recommendation of Functional Medicine, in my eyes, is, that it explains the necessity, and thereby justifies the use, of empirical plans and remedies. I will explain what I mean by empiricism. We seek to modify the *function of the kidney.* We have but a limited insight into the working of this organ; we know that it consists of certain parts, and we know to some extent the working of these parts. But we know very imperfectly. We find that in a state of health the quantity of the urinary secretion may be increased in numberless ways. When we come to use some of these means in a case of dropsy, where hyperuresis would be useful, they totally fail. We cannot tell why this is. Or we find that one amongst many succeeds. We can no more tell why this is. We only know the fact. Empiricism recognises this imperfection of our knowledge; and it also tells us how best to counteract it. Do not persist in the use of a drug which does no good, or even possibly does harm, because, judging from some data or others, we have to come to the conclusion that it *ought* to do good; on the other hand, do not cast aside drugs or methods which seem to do good, or have done good, in apparently similar cases, because, judging from some data or others, they ought not to do good. Many proofs might be given that such a warning as this, superfluous as it may appear, is not really so. I will give but one. Dr. Bright, who was the chief discoverer of the diseases which go by his name, was in the habit of using diuretics in the treatment of them. I have always done so, and in many cases with signal success. Yet, in spite of this teaching by empiricism, their use has been strongly deprecated, because, judging by certain microscopical appearances, diuretics ought to be injurious. In treating a case of Bright's disease, I try to restore the function which is most seriously disordered. I find that in many cases, not in all, the only way to restore this function to something like its proper performance is to use diuretics.

Functional Medicine leads on a little further in another direction. It leads us to anticipate disease, and so to forestall the enemy. Thus we find that in the course of certain diseases there is special liability to interference with some particular function, as in the case of the kidneys in diphtheria. Precautions may in this instance be readily taken which, though perhaps not infallibly, will probably prevent the functions of the kidneys being seriously interrupted, and thus we may prevent the very dangerous consequences which this interruption entails. We can similarly prevent scarlatinal dropsy.

Functional Medicine also justifies, and may therefore restore, some old and wise practices which have been submerged under the advancing wave of modern science. When it was not well recognised that many diseases tended naturally to cure themselves, methods were adopted for their relief which are now disused, not because they have been proved to be wrong in practice, but because they became unnecessary in theory. For example, the use of a sharp calomel purge at the outset of the exanthemata, once universal, is now rarely resorted to; yet the beneficial effects of this plan in ordinary cases need not be insisted on to those who in their own proper persons have been so treated. But where we find, from our knowledge of the natural history of a disease, that there is a tendency to pernicious diarrhœa, as in typhoid, then we should either refrain from such treatment or use it with extreme caution. This is another example of anticipation in Functional Medicine. Speaking of typhoid calls to my mind another example of the justification of an old treatment by Functional Medicine. I have never seen any more decided benefit produced by any treatment than I have seen, years ago in the Paris hospitals, from the application of leeches or cupping over the cæcum in certain cases of typhoid. Yet typhoid is a disease of debility, and we are urged nowadays in cases of debility to ignore all special functions and "restore" the patient. How, then, are we to explain the success of this anomalous treatment? Very simply. The cupping restored the function of that part of the mucous membrance the disorder of which, in the particular cases referred to, had aggravated the general disorder of the system. In the weakest patient it is often better to restore a function the irregularity of which is keeping the patient back, than to try to force them into strength by stuffing them with food and tonics and stimulants. How often do we see an enfeebled and disordered stomach thus overburdened and exasperated, instead of soothed by appropriate diet and quieting medicines.

The last consideration which I shall offer you as evidence of the propriety of the ideas which I have summed up in the term Functional Medicine may not be at first sight one of *great practical* weight. It is, nevertheless, in my estimation, anything but trivial. It is that this principle of action is in harmony with the latest and highest conceptions of philosophical medicine. To you who have probably not been much accustomed to abstract or metaphysical considerations, I may perhaps find a little difficulty in conveying my meaning. I will, however, try to do so in a plain and simple manner. What is disease? What do we mean by the term? In old times people used to think that a disease was some actual entity or thing which had got into the body in some way, and was there lying hidden and secreted, and was to be cast out. We know that there are a few instances of this kind of thing. For example, we know that some disorders depend upon the presence of a tapeworm in the intestines, and that to cure them we must hunt the worm out of the body. This idea, which we now know to be true only in a few specific instances, was at one time general. The germ of this idea still runs through a great many of our conceptions of the nature of disease. This is more particularly the case in the pathological conceptions of ill-educated laymen. But many traces of it are still to be found amongst ourselves also. It is not long since the most illustrious and philosophical of existing pathologists has been dealing with this question; and the conclusion he has come to is that all disease is disordered function. Here, then, I say, is the highest justification for all treatment being based upon the principle of restoring disordered functions to order, and this it is which I have ventured to term Functional Medicine.—*Lancet*, *July* 1, 1871, *p.* 4.

9.—THE SECRETIONS AS GUIDES TO TREATMENT.

By Dr. H. W. FULLER, Physician to St. George's Hospital, London.

[Failure of treatment frequently depends upon a neglect to examine carefully the character of the secretions. Our diagnosis of the precise nature of a disorder may be perfect, but this alone does not always suffice for success in its treatment.]

Let me take as an example the case of a man at present in the Cambridge ward, who was admitted suffering from anasarca, occasioned by a weak and dilated heart. In addition to extensive dropsy of the extremities, he had urgent dyspnœa with orthopnœa, congestion of the lungs, effusion into the pleural cavity, engorgement of the liver, with pale-coloured fæcal evacuations, and scanty, loaded, non-albuminous urine, tympanitic distension of the abdomen, and slight effusion into the abdominal cavity—the ordinary assemblage of symptoms which result from long-continued interference with the central organ of the circulation. Before I saw him he had taken salines with digitalis, squills, scoparium, and other diuretics; but his urine had not increased in quantity, and his symptoms had been gradually increasing in severity. This was his condition when he was admitted into the hospital. Having regard to the pale color of his motions, I felt convinced that no real good could be effected unless a free flow of bile could be induced; and that the failure of the treatment he had undergone before admission into the hospital depended principally, if not solely, upon the neglect to stimulate the action of the liver and the secreting apparatus of the bowels. I therefore prescribed five grains of the compound digitalis pill—containing three grains of blue pill, a grain of squills, and half a grain of digitalis—three times a day; and within three days he had begun to improve. Before the end of a week the motions had assumed a healthy colour, the quantity of urine was trebled, the flatulence had almost disappeared, the anasarca was rapidly decreasing, and the dyspnœa subsiding. Indeed, it was obvious that the very medicines which before had proved inoperative had become active agents for good as soon as the secreting apparatus of the liver and bowels had been stimulated to healthy action.

Take another class of cases. It sometimes happens that patients are admitted suffering from ague, with which they have been afflicted for many weeks. They have taken quinine perseveringly, but have not obtained relief. On examination the case is at once apparent. Their internal organs are in a state of engorgement, and consequently are sluggish; their bowels are costive, and their motions unhealthy. In these cases, the administration of remedies calculated to stimulate the viscera to action is all that is needed to restore them to health. Two or three doses of colocynth and rhubarb in combination with quinine will at once arrest the disease, which weeks of mere quinine taking had failed even to control.

In many forms of dyspepsia the same holds good. The secretions of the stomach and bowels are disordered, the liver is gorged, and the tongue is covered with a yellow fur. The patient has been purged by means of senna, colocynth, or rhubarb; alkalies and alkaline earths, together with vegetable bitters, have been given; possibly the mineral acids have also been tried; moderately strict dieting has been had recourse to; and other expedients have been adopted, but in vain. The disagreeable taste in the mouth, the acidity, waterbrash, flatulence, drowsiness after meals, and restlessness at night, continue unabated. And what is the cause of this failure of treatment? Though the bowels act regularly, the motions are pale and lumpy, or else dark-coloured and offensive; and the urine is scanty, high-coloured, and loaded with lithates. Cases such as these often come before us in the wards, and occur still more frequently in private consulting practice. They convey a lesson which must not be forgotten, even though healthy. Dogs, with artificially-made biliary fistulæ, do not appear to secrete an increased quantity of bile under the influence of moderate doses of calomel. The lesson which they teach, and which you will do well to remember, is, that so long as the secretory apparatus is inactive or out of order, the remedies which are ordinarily most efficacious in relieving the symptoms of dyspesia are of little avail; whereas they exert their beneficial influence directly that disorder is rectified, and secretion is re-established. In cases such as these, a few doses of calomel, combined with opium, if necessary, will in a few days effect a change for the better, which cannot be brought about by other means in as many weeks. Under their influence the motions will lose their offensive odour, and assume a healthy colour, the tongue will clean, the urine will become clear, the symptoms of acidity and discomfort will pass off, and your patient will give you credit for affording him relief. When this healthy condition of the secretions has been attained, you need have little anxiety as to your patient's recovery, for the remedies which had previously proved ineffective will speedily quiet the irritability of the stomach, increase its tone, and restore your patient to health.

Let me take yet one more class of cases—the most common perhaps with which we have to do, and that in which, perhaps more than in any other, a want of proper regard to the secretions leads inevitably to unsuccessful practice. I refer to cases of so-called debility, in which stimulants, high feeding, and tonics are constantly recommended, and are often fruitlessly had recourse to. The cases to which these observations apply are of every imaginable description. In private practice it often happens that a person, not otherwise out of health, is accidentally deprived of his usual exercise; and, his appetite being unimpaired, he takes more food than is absolutely required for the repair of his body. The wear and tear of his tissues being much less than usual, owing to his inactivity, and the supply of fresh materials, owing to his unimpaired appetite, being in excess of the actual requirements of the body, one of two things must necessarily happen: either his excretory organs must do an unusual amount of work, and throw out of the system the whole of the matters which have been taken in excess of the actual requirements of the tissues, or the surplus materials must accumulate in the blood, alter its quality, and oppress the nervous system. The alternative is not doubtful. When the blood is surcharged with materials which, however good and nutritious, are yet in excess of the requirements of nutrition, the nervous centres are oppressed; and not only does languor, or general debility, as it is termed, occur, but the liver, kidneys, and other secretory organs become gorged and sluggish, the urine becomes scanty, and the motions become clay-coloured, or else dark and offensive. In private practice, cases such as these are constantly met with, in which, notwithstanding the unhealthy *state of the secretions*, tonics have been given for months, together with stimulants and every variety of strong food, and in which a few days of active purgation, some alterative doses of mercury, enforced exercise, and a restricted diet, by leading to the elimination of the surplus materials, and so to a purification of the blood, do more than all the previous tonics and rich feeding to put an end to the patient's languor, and restore his physical and mental power. In hospital practice, these particular cases less frequently come before us; but in another form, and under other circumtances, similar instances not unfrequently present themselves. In the Roseberry ward, at the present time, is a stout, strong-looking, hysterical servant girl, Sarah Fleetwood, suffering from amenorrhœa. She had undergone treatment for a considerable period before admission, and throughout had taken iron in various forms, but had not obtained relief. On admission, she complained of extreme languor and debility, and of utter loss of appetite. Her tongue was clean, and her bowels were said to be regular; but her urine was scanty and loaded with lithates; on examination her motions proved to be very scanty, and almost white, lumpy, and offensive, and had been so, according to her account, throughout her illness. She is a perfect type of the class of cases which I have been endeavouring to describe. She is oppressed by the presence in the blood of matters which ought long since to have been eliminated, but which the inactivity of her secretory organs has caused to be retained in the system; weak, in the sense in which a healthy person on the eve of a bilious attack is weak, but in no other; weak from a cause which no strengthening food and no tonics will remove, nay, rather which they will tend to aggravate, and which protracted semi-starvation or the skilled aid of the physician is required to rectify. The iron and other tonics, the port wine and other stimulants, which the poor girl had taken prior to her admission into the hospital, only tended still farther to surcharge her system with materials which it was incapable of assimilating—to render her blood more noxious to the brain and other nervous centres, and thus to increase her languor and aggravate her suffering. Yet these very remedies have nearly effected her cure, now that healthy secretion has been re-established. Already, under the influence of a few doses of calomel and colocynth, the urine, instead of being scanty and loaded, has become abundant, and in every respect healthy; the motions are no longer pale and lumpy, but have become normal in character; colour has returned to her lips; she sleeps more quietly, feels stronger and less languid; and no longer exhibits a repugnance to food. And now that the iron is able to do its work, its beneficial influence will soon be manifested still more decidedly, and the menstrual discharge will speedily return. In a precisely similar case which I saw with Sir Charles Locock many years ago, the symptoms did not yield until after a protracted course of calomel; and experience justifies me in warning you that in several forms of amenorrhœa, and in many of the so-called cases of general debility, a deranged condition of the alvine secretions lies at the very root of the symptoms, and that all remedies will fail to relieve the patient until steps have been taken to rectify them. Purgation may or may not be necessary, and the aid of calomel may or may not be needed; but if either or both prove requisite to attain the desired object, they must be used without hesitation. No theoretical considerations must be permitted to countervail such clear and unmistakable indications for treatment. Experience confirms what theoretical considerations would lead us to expect, namely, that the true mode of restoring strength, under the conditions we are now considering, is, not by administering food and tonics which the patient is incapable of assimilating and making use of; nor is it by abstaining from administering alterative or aperient remedies, lest by so doing we should weaken the patient—both of which courses can only tend to continued mal-nutrition and a gradually-increasing failure of strength—but rather to endeavour, by appropriate means, and at whatever risk of present discomfort to the patient, to re-establish healthy secretion; and thus place him in a position to profit by wholesome food, the natural restorative of health and strength.—*St. George's Hospital Reports, vol.* 5, *p.* 17.

10.—THE APPLICATIONS OF ELECTRICITY TO MEDICINE.

Electricity, as used in Medicine, is either static (derived from the friction of a glass-plate or cylinder), or dynamic (the direct product of chemical action), or induced from the contact-current by an induction-coil. Static electricity is not much employed. Contact-electricity or constant-current galvanism is of much greater importance; but its value is only now becoming fully understood.

In a former article on this subject (*vide* p. 611, vol. i., 1871), we discussed these two varieties of electricity—viz., Frictional, or Franklinic Electricity, and Current, or Galvanic Electricity—and we considered them with special reference to the modern doctrines of force or energy. Putting on one side all further consideration of electricity as related to other forces, we shall next proceed to discuss that form of electric action which was discovered by our countryman Faraday, and which is commonly known as Induced, or Faradic Electricity.

The induced electricity (the interrupted current, or faradisation) is that most commonly employed, and is derived either from electro-voltaic or electro-magnetic instruments. In the former, the electricity is primarily developed by chemical action; in the second, by the rotation of a kind of wheel.

Suppose we construct a coil of copper wire, covered with silk to insulate it. This wire had better be rather thick, and the number of turns need not be great, especially if wound round a core composed of soft-iron wires. If now we construct another, also of copper wire, finer in character, and made up of a great number of coils, but built so as to constitute a cylinder, into which the former may be introduced, we have an induction apparatus. All that is now necessary is to connect the extremities of the thick wire with a battery, and the extremities of the thin wire with some object capable of testing the existence of electrical effects, when it will be found that the moment the circuit through the thick wire is closed the poles of the thin wire also give evidence of a galvanic current, stronger than that produced by the battery and passing in the opposite direction —this, too, without any direct communication between the contained and containing coils, the former of which is called the primary, the other the secondary. But when the current has been fairly established in the primary coil, its effects on the secondary cease until the former is arrested, when signs of a current, this time in the direction of the primary one, are again manifested by the secondary coil. Thus, in induced electricity the current is to and fro, and can only be produced by breaking or closing the primary current. To keep up its effects, therefore, some apparatus must be introduced, constantly opening and constantly closing this current. This is commonly known as the *trembler.*

But there is another form of induced electricity still more frequently employed. • It is called electro-magnetism. Thus, if a bar of soft iron, shaped like a horse-shoe, be surrounded by either extremity of a coil of protected wire, wound in opposite directions, and if through this wire is passed an electric current, the iron bar is converted into a magnet. But suppose something quite the reverse of this—that is to say, a magnet, horseshoe-shaped and powerful—is the origin of the energy, instead of a battery. If, then, we have two bobbins made of copper wire covered with silk, and surrounding soft-iron centres, the two being connected on an axis by a soft-iron plate; if, further, we have the means of causing these to rotate rapidly on this axis, either iron core coming close to both poles of the magnet in the course of its revolution, we have the well-known magneto-electric machine. Here magnetism is transferred with each approximation from the magnet to the soft-iron core; the soft-iron core in its turn induces an electric current in the copper-wire coils, whence by appropriate mechanism it is conveyed to the galvanometer or human body, as the case may be.

These are the two forms of electric force most generally employed in Medicine, chiefly on account of the ease with which they may be produced and applied.

A word next as to the means commonly employed of giving rise to current electricity, whether for applying it directly to the human system, or for producing an induced current for that purpose. This is attained usually by constructing a series of couples, forming what is known as a galvanic battery. The electricity so produced is, *of course,* current electricity, and this current is continu-

ous. The current in the case of induced electricity, on the other hand, is to and fro, or, as it is also termed, *interrupted.* Now, in batteries constructed on the principles we have laid down—that is to say, made up of electro-positive and electro-negative plates, with a liquid intervening—there is great difficulty to be faced in rendering the current equable. Hence these have now generally fallen into disuse, and batteries with two liquids instead of one are commonly employed. The reason why the galvanic action of the two elements becomes enfeebled is twofold. Thus, in the zinc-copper combination, the sulphuric acid combines with the zinc, rapidly at first, more and more slowly afterwards, as the acid comes nearer and nearer to neutralisation. But this is not the principal difficulty; for, as the sulphate of zinc forms, hydrogen is set free, and this clings most pertinaciously to the inactive copper plate, interfering with the circuit of the current; and not only so, but, by reducing some of the sulphate of zinc, it coats the copper plate with a layer of zinc, and so gradually approximates the character of the two plates.

To remedy this last difficulty a second liquor is introduced, and so placed that the hydrogen set free in forming the sulphate of zinc shall attack it rather than the inactive plate, which in this way is untouched, and thus the strength of the current remains unimpaired, so that the current so produced is known as the *constant* current. One of the best forms of constant-current battery was invented by the late Professor Daniell, and is known by his name. This is a zinc-copper battery, but the copper is immersed in a solution of sulphate of copper. Between this and the dilute sulphuric acid a diaphragm of glazed porcelain is interposed. By this means the two liquids are kept from uniting too speedily, without interfering with the passage of the electric current. When in action the hydrogen attacks the sulphate of copper, reducing the metallic copper which is deposited on the copper plate. The sulphuric acid passing in the opposite direction tends to renew the acid attacking the zinc. Provision is easily made to keep up the supply of sulphate of copper, and so the current remains constant for a very long time.

Some form of this battery is that most useful in Medicine, provided a constant current be desired, but only if the battery is to be stationary. The bulk of the elements and the mass of sulphate of copper crystals requisite, if it is to be used for any length of time, interfere with its portability. One of the best for the Practitioner, or rather, we should say, for Hospital practice, is constructed by Messrs. Elliott and Co., Strand. This will continue to act steadily for a very long time, but it is bulky, and cannot be readily transferred from place to place. As copper and zinc are not very far removed from each other in the electro-motive series, inventors have sought to increase the electro-motive power of a battery, by selecting as the negative element platinum or carbon. The former is the principle of Grove's, the latter of Bunsen's battery. In either case, nitric acid is used instead of sulphate of copper. This, being attacked by the hydrogen, produces nitrous acid, and so protects the platinum or carbon. These batteries are exceedingly powerful, but the nitrous acid fumes, evolved after a time, are extremely objectionable, and so chromic acid is frequently used as a reducing agent. The battery in most common use nowadays for Medical purposes is constructed on this principle. It is made by Stöhrer, of Dresden, and consists of zinc-carbon elements, with chromic acid as the reducing agent. Two forms are in use —one where one or two of these elements are employed to give rise to an induced current; another where fifteen or twenty are combined, for the purpose of producing a constant continuous current.

In all these batteries two liquids are used, but in Smee's battery a nearly constant current is obtained with one liquid by mechanical means. The elements are zinc-platinum, but the platinum plate is roughened by covering it with spongy platinum, whereby the disengagement of the hydrogen is greatly facilitated, and the resistance is reduced to a minimum. The most useful Medical battery for producing a constant current and, at the same time, retaining portability, is a modification of this, made by Weiss and Son, the well-known instrument-makers.—*Med. Times and Gazette, July* 1, 1871, *p.* 16.

11.—OSTEO-ARTHRITIS, OR SO-CALLED RHEUMATIC GOUT: ITS ANALOGIES, NATURAL AFFINITIES, AND ANTAGONISMS.

By Dr. H. W. Fuller, Physician to St. George's Hospital, London.

[A safe means of estimating the true nature of a disorder is to trace the analogies which it offers to other forms of disease, the affinities which it displays, and the antagonism which its existence seems to present to the inroad of other forms of complaint. For instance, there is considerable similarity in the general symptoms between osteo-arthritis, or so-called rheumatic gout, and scrofulous affections of the joints; there is in both a constitutional taint and the same tendency to the formation of osseous growths around the joint. There are, however, well-marked antagonisms between the two diseases which are both striking and instructive.]

So close is its affinity to phthisis pulmonalis, that out of four hundred and seventy-six patients whose history I have carefully investigated, no less than one hundred and ninety-six, or 40·3 per cent. belonged to decidedly consumptive families, and had lost a father, mother, brother, or sister, from the effects of pulmonary disease. Yet, notwithstanding the strongly-marked hereditary tendency to pulmonary disorder in the families of those who suffer from rheumatic gout, the antagonism to phthisis which results from the existence of osteo-arthritis, is most remarkable. In the whole course of my experience I have only known five persons die of consumption whose joints have been distorted by osteo-arthritis, and in all those five the active symptoms of the articular disease had long subsided before the pulmonary disorder commenced. In other words, the tendency to osteo-arthritis was replaced by the tendency to phthisis, and did not run on concurrently with it.

The antagonism of osteo-arthritis to albuminuria is even more striking. I examine the urine of every patient who consults me in private practice, and thus can testify to the condition of the urine in many hundred cases of rheumatic gout; but I have never met with an instance of albuminuria associated with this form of articular disease. This is the more remarkable from the fact that albuminuria, to a greater or less extent, is met with in nearly 25 per cent. of all cases of acute disease, and in a considerable proportion of chronic disorders; and may be regarded almost as the natural accompaniment of old cases of chronic gout, in which deposits of urate of soda have taken place in the joints—cases which are too often confounded with osteo-arthritis, in consequence of the distortion of the joint which accompanies the latter form of disease. Indeed the contrast afforded by the condition of the urine in cases of osteo-arthritis and in those of chronic gout, may often serve as a means of diagnosis between the two disorders. In the one the urine is usually of normal specific gravity, and never albuminous; whereas in the other the specific gravity is usually much below the normal standard, the presence of albumen is not uncommon, and when chalk-stones are present, is almost constant.

Thus, then, on reviewing the history of rheumatic gout, it is impossible to avoid the conclusion, that the disease, like phthisis pulmonalis, results from a form of mal-assimilation in some way connected with a depressed state of vitality. The causes which usually conduce to its inroad, and which invariably aggravate its symptoms, are all of a depressing nature; many of its more characteristic features point unmistakably to a low state of vitality; whilst its close affinity to phthisis, and the peculiar character of the changes which it induces in the joints—changes which approximate, in many respects, to those which result from strumous and malignant disease—serve to indicate a depressed condition of the general health. It is needless to add, that the result of treatment confirms the impression thus derived from the history of the disorder. You are all aware that I never treat it as a form either of gout or rheumatism, and do not employ colchicum, alkalies, or other of the so-called specifics for those complaints; inasmuch as they only tend to depress the patient, and hasten the onward progress of his malady. Even in the acute form of the disorder—when the patient is usually bedewed with perspiration, and declares that it is agony to move—*I order him to leave the* bed, with the view of getting rid of the relaxing influence which tends to keep up the enfeebling perspiration. If that step does not suffice to check the sweating, I prescribe a cold shower-bath or the dripping-sheet, and at the same time administer the mineral acids and vegetable tonics, with cod-liver oil and a generous diet; and, provided the urine remains clear after standing, I do not hesitate to prescribe iron, and to allow a fair quantity of wine, beer, or porter.

As you have all seen the benefit which results from this treatment, it is needless to do more than beg you to be resolute in insisting on its adoption; for the prejudice of mankind is very strong against any treatment in which warmth does not play a conspicuous part; and in private practice all the tact you possess will be required to get your instructions properly carried out. I do not hesitate to say that there are thousands of persons more or less crippled by rheumatic gout who would have been in full possession of their powers of locomotion if a tonic and bracing system of treatment had been adopted when they were first attacked.

The strange antagonism of osteo-arthritis to albuminuria deserves the closest investigation. At present I can do little more than record the fact; but it seems probable that, with the progress of chemical and other methods of investigation, the cause of this antagonism may be discovered, and that light may be thus shed upon the pathology both of albuminuria and osteo-arthritis.—*St. George's Hospital Reports*, 1870, *vol. v.*, *p.* 14.

DISEASES OF THE NERVOUS SYSTEM.

12.—ON HYPERÆSTHESIA.

By Dr. C. Handfield Jones, F.R.S., Physician to St. Mary's Hospital.

[Besides the classical diseases of the nervous system there are a number of anomalous cases which scarcely find a place in a regular nomenclature. Many of these belong to the debatable ground where it is difficult to decide whether we have to do with diseases or with symptoms.]

1. In the *cutaneous surface* we very often meet with a condition which betrays itself by no objective change yet discovered, but solely by an abnormal sensitiveness. The tactile faculty of the skin is not really increased; it is not a more efficient organ of touch than it was before, but quite the reverse, as its nerves are incapable of tolerating the necessary contact with bodies presented to them. Such a condition is conveniently termed hyperæsthesia; and this name I shall continue to use in what I have further to say about it. This state of hyperæsthesia is met with very frequently in sufferers of the quasi-hysterical class, both male and female. One poor fellow assured me that his skin had been so sensitive that he could not bear the least touch anywhere; the pressure of a stethoscope in auscultation was intolerable; and the touch of a crinoline-hoop against his leg in a railway carriage caused him terrible pain. These complaints were made quite gravely; and though we can hardly avoid smiling at them, yet we must admit that to the sufferer himself such a state of things was no joke. Think what you would endure yourselves, if all ordinary contact were a cause of continual pain. The reality of such suffering is materially confirmed by the fact that, in some cases of structural disease of the cord, certain parts of the cutaneous surface manifest intense reflex excitability, though ordinary sensibility is much impaired. In a most workmanlike report in the *Transactions of the Clinical Society*, vol i., p. 102, on a case of locomotor ataxy, it is mentioned that an area of skin on the outer side of the right leg, about two inches and a half square, was, and had been for about two years, in a condition of extreme sensitivity to impressions capable of exciting reflex movements—such that the patient had frequently been thrown into convulsions, and had dropped down in the street, from the mere brushing of a woman's dress against this part. Even

when he touched this spot himself, either with his hand or his trousers, whilst dressing, slighter convulsive movements often took place. At these times he felt an odd kind of sensation in the part, which then diffused itself rapidly throughout the body. In such hyperæsthesia as this the sensorium is but little concerned; but there is evidently no improbability that the cerebrum might be the seat of such disorder, as well as the cord. In fact, this is actually the case in many instances of arachnitis. If you object, that I am now speaking of coarse structural lesions, this will not apply to the case of rickets, where the nervous centres are often exempt from evident lesion, and yet the general tenderness is such that the child cries if he be but touched.

The hyperæsthesia which affects the region of the spine, or, more exactly, the line of the spinous processes, needs to be somewhat specially noticed, because it has been much dwelt on, and is often accepted as a sign (with others) of meningitis or myelitis. Brown Séquard speaks decidedly on this point; he says that "it is almost always found that pressure upon the spinous process of the vertebræ (sometimes even a slight one), when made at the upper limit of the inflammation, causes an acute pain" (*Lancet*, July 14th, 1860). Dr. Radcliffe, on the other hand, is inclined to the view that this pain is mostly absent, at any rate, in uncomplicated myelitis. My own experience leads me to look for it as a nearly constant symptom, but not in that degree of acuteness which characterises the neurotic disorder. This, at any rate, is certain: that pain in the back *per se* is no evidence whatever of inflammation being present in any of the component structures, any more than headache is of the cranium and its contents being so affected; and, indeed, the two dysæsthesiæ are in many respects homologous. Spinal hyperæsthesia extends sometimes over the skin on one side, and not at all on the other; sometimes light pressure is painful, and firm relieves; sometimes the latter is the worse. In some cases, I believe, the bones are involved in the neuralgia, without being at all inflamed.

2. The *lingual and buccal mucous membranes* are sometimes the seat of extreme and most distressing hyperæsthesia. The affected parts may appear quite natural, or at most there may be undue redness and insufficient development of epithelium, giving a "beef-steak" appearance, but not at all more than I have often seen in patients who were quite free from this hyperæsthesia. The following case is a good example of this happily infrequent disorder. Mrs. C., aged 73, was seen August 28th, 1866. She had been suffering the last five weeks with her mouth, which was so tender that she could bear nothing in the way of food but lukewarm milk and sopped bread. Cold things gave her pain, as well as hot. The buccal membrane was very red, especially that of the lower lip. The whole surface, including that of the tongue, was extremely tender; but I saw nowhere any breach of surface, nor anything resembling an ulcer. There were certainly some superficial fissures on the dorsum, but they were no more than often exist without causing any suffering, and they did not appear to involve the corium. The tongue was perfectly clean and moist. Pressure on the mucous membrane did not render a spot pale even for an instant after its removal. The bowels were regular; pulse 84, of good force. The appetite was very good, but she felt quite exhausted from being unable to take food. She got very little sleep, from the distress caused by her mouth. She had suffered much from rheumatism, and felt the changes of the weather very much. My notes extend over more than four months, and show scarce any change to have occurred during the whole of that time; and I am tolerably certain that she continued to suffer in the same way, though less severely, to the close of her life, or till about April 1869. She constantly kept a piece of soft rag in her mouth, which afforded her some solace. On one occasion, I took the temperature of her mouth, and found it as high as 100.4° F. The membrane of the buccal cavity always presented the same appearance of considerable hyperæmia. At one time, a little eczematoid eruption appeared at the margin of the lips, but it never extended internally. The suffering she endured must have been terrible; she described it as that caused by a burning fire, and declared she was enduring a living death. Her temperament was by no means excitable, and I do not think she exaggerated her griefs in the least. Many remedies were used; but none were of material efficacy, except a nightly opiate to pro-

cure sleep. The pathological condition was, I believe, one of neuralgia and hyperæsthesia, blended, as sometimes happens, with vaso-motor paralysis, which latter produced the hyperæmia. The neurosis may have been of gouty origin.

3. The *throat* is liable to suffer with such disorder as we are considering, which it is important not to confound with common inflammatory, as the treatment required is materially different. The following case is an illustration. Mrs. N., a young-looking, recently married woman, was seen by me on September 8th. She had been suffering four days with swelling and neuralgia of the right side of the face. Her throat had become sore, so that she had much difficulty in swallowing. On the previous day she could not get down a piece of sopped bread. There was some nausea and vomiting. The throat appeared normal, but with the laryngoscope I could see that the laryngeal membrane was much congested. Her voice, however, was not altered. She had soreness and tenderness at the sides of the neck, lower down on each side than the tonsils. She expectorated a little reddish mucus. She had some difficulty in getting her breath. Her sleep was disturbed; she woke up restless and feverish. She felt very weak. The urine appeared normal. I advised diligent steaming of the throat, sinapisms to the sides of the neck, fifteen grains of chloral at bedtime, and two grains of iodide of potassium and an ounce of effervescing mixture of citrate of potash three times a day. On the 9th, she slept two hours with the chloral, then woke in great uneasiness, and only dozed the rest of the night. At noon, I found her suffering much. The throat was very sore. She had not been relieved at all by the steaming; swallowing was very painful. She had much pain and tenderness in her neck; more, however, at the sides of the muscular part than in the larynx, which I could handle and move pretty freely. Her throat felt dry. Tongue coated; pulse 93. I ordered twenty grains of muriate of ammonia with ten minims of tincture of belladonna in water every four hours, hot fomentations to the neck, and aconite liniment. She also had immediately fifteen minims of chlorodyne, which gave her two hours' sleep; and at 9 p.m. I found her decidedly better, though she had a good deal of strange pain, hardly describable, on both sides of her head. The dose of chlorodyne was repeated at 10 p.m., and in the course of the night she had fifteen grains of chloral; but she was in high fever and delirium all night long. The room seemed whirling round; she felt as if she must get up and go out—as if she should fly. She saw numbers of persons in the room, with great heads and little bodies, etc. At 9.30 a.m. on the 10th, when I saw her, her throat was much better; she took some toast. The catamenia had reappeared, which had stopped, after a day of normal continuance, on September 3d. I ordered a drachm of tincture of cinchona flava four times a day. On the 11th the throat was much better; she had still dysæsthesiæ about the head, and a large patch of urticarious erythema had appeared about each elbow. On the 12th, she was so well as to call on me and take her leave before setting out for the North. She had sweated copiously the preceding night, had a good deal of eruption on the legs and thighs, but made no complaint of her throat. The attendant neuralgia, the peculiar distress, the nocturnal delirium, the urticaria, and the "juvantia," all go to prove the neurotic character of the disorder, and that it was no common sore-throat, but one where the hyperæsthetic greatly predominated over the inflammatory element. The abrupt cessation of the catamenial flow may have had to do with the supervention of the disorder; but I think the most essential factor was influenza.

4. From the throat we descend to the *stomach;* and among the woes of this much-enduring organ we may well expect to find hyperæsthesia. In fact this condition, in a greater or less degree, is of very frequent occurrence, and constitutes the chief disorder in those cases to which the term "irritative dyspepsia" is appropriate. In many of these hyperæmia and catarrh exist also, but as secondary complications, rather than as primary maladies. The hyperæsthesia, I believe, as a paralytic neurosis, is prone to implicate the vaso-motor nerves and impair the tone of arteries and the retentive power of capillaries. Such disorder may be simple—*i. e.*, dependent on the state of the general nervous system; or it may depend on reflex irritation, as in pregnancy. In some of these states, pain is the more predominant symptom; in others, vomiting; in others, both

are present in a marked degree. Andral relates a case where the tongue was natural, and the patient distressed by a very acute hunger; but the lightest food given to her was thrown up by vomiting, or, if it was retained, caused indescribable suffering. The hyperæsthesia was overcome by cold affusion and persevering administration of food. Here the disorder was simple; and such seems to be generally the case, though in some instances, as in the hyperæsthesia of pregnancy, the influence of reflex irritation is evident. Nitrate of silver is a really valuable remedy in cases where pain is distressing; and, as it need only be given for a short time, you run no risk of spoiling your patient's complexion. It may be seconded, however, or replaced, very frequently, by subcutaneous opiates. I have employed them myself with the best effects. Dr. Clifford Allbutt writes strongly in their praise. One of his cases was that of a merchant who regarded himself as a hopeless dyspeptic, dreaded his meals, and especially his nights. He was cured (at least for the time) by one-fifth of a grain of morphia subcutaneously, at 3 p.m., and at 10½ p.m., followed by rest, during ten days, with one-fourth of a grain at night, only for ten days more. I prefer myself to make these injections at the epigastrium, but this is not necessary. When the hyperæsthesia expresses itself in immediate vomiting without severe pain, I have great faith in strychnia, administered in doses of one-twentieth of a grain in not more than half an ounce of water. Let me cite the following instance. A slight-made female, aged twenty-two, married four months, subject to indigestion, and weakened by a miscarriage, was brought to me with severe sickness of ten-days' duration, no food having been retained the last three. I gave her, in my room, one-twentieth of a grain of strychnia, and made her lie down. After she had retched a few times ineffectually, she became much better, "did not feel at all sick," and took successively four teaspoonfuls of milk before she left. I saw her again in ten days, when she was greatly better, had only been sick once, and was able to take chicken.

6. That the *tracheal* and *bronchial* mucous surface is liable to hyperæsthesia, cannot be doubted. There are coughs of the most vexatious and obstinate character, which are either unattended with *râles* or expectoration, or where these are out of all proportion insignificant to the severity of the cough. Some of them are of reflex origin, as in Graves's case, where the expulsion of a tænia put an end to a most violent cough, that went on night and day, and resisted all remedies. Some are due to toxic matters in the blood, as is probably the case in pertussis, and in some cases of gout, where, as Graves says, the cough is dry, annoying, and often very obstinate. Some, again, are simple neuroses, as in the case related by Trousseau, of a young lady who was of an hysterical constitution, but was exempt from catamenial irregularity, and from other nerve-disorders, except a continual cough, which proved refractory to all remedies, but yielded immediately to change of air. The treatment which appears to me the most rational and satisfactory in pertussis is that recommended by Dr. E. Smith, of producing and maintaining moderate narcosis by means of small doses of morphia; and this is quite accordant with the view that the paroxysms depend upon a specific toxic hyperæsthesia. There is another form of bronchial hyperæsthesia with which it is very important for you to be acquainted—viz., that which occurs in certain instances of pneumonia, where after a time the respiration becomes inordinately frequent, perhaps 80 in the minute, without any corresponding increase in the pulmonary lesions or lividity of face. In these cases, the hurry of respiration is a mere neurosis, and yields to opium and a due amount of support.

7. The *heart* enjoys no exemption from hyperæsthesia. Cases are not at all rare where there is more or less præcordial uneasiness or pain, with rapid, vivid action of the heart, unduly forcible impulse against the chest-wall, and loud sharp sounds. The radial pulse may be small and weak, contrasting strongly with the apparently forcible efforts of the heart. Such diseases are often seen in young persons, and are sometimes mistaken for instances of cardiac hypertrophy; from which, however, you may easily distinguish them by observing that the dulness-area is not increased, and the apex-beat not lowered nor displaced to the left. Toxic agents, such as tea and tobacco, reflex irritations, as of the gastric mucous surface, and primary failure or disorder of

nerve-force, may all be incriminated in different instances as efficient causes. In the latter group, it seems most probable that the nervous filaments distributed to the endocardium are unduly impressible, and, being thus over-stimulated by the moving blood, keep the muscular fibres in a state of rapid action. Anæmia, though a very common cause, is by no means necessarily present. In Da Costa's account of the curious cardiac malady frequent among American soldiers in the late war, he mentions, as symptoms, constantly recurring attacks of palpitation and pain, whose chief seat is near the apex of the heart, and combined very commonly with excessive cutaneous sensibility. In some of his cases, the disorder supervened on fatiguing marches; in others it occurred after fevers or diarrhœa. He tells us in his recent paper (*American Journal of the Medical Sciences*, Jan. 1871), that digitalis or digitaline had more influence on the cardiac disorder than any other drug resorted to. Belladonna internally he also found a most efficient agent, particularly valuable in instances of irregular action. Aconite reduced the force of the heart, and lessened the tension of the pulse; it was most suited to cases of commencing hypertrophy. No special commendation is bestowed on other sedatives; and gelsemin, which is reputed to be a cardiac sedative, disappointed him much; for the most part, it had no effect at all. Morphia hypodermically relieved the cardiac pain almost invariably, and so did atropia; but the effect was only temporary. Tonics were often most excellent adjuncts to treatment, but availed little by themselves. Zinc and iron accomplished most. Da Costa's experience has convinced him that there is a real connexion between the functional disorder, cardiac irritability, and the organic one, cardiac hypertrophy; that, in fact, "the one grew out of the other." Dr. Walshe admitted the same connection several years ago. I cannot refuse to admit the doctrine held by these high authorities; but my own experience would not lead me to rate this cause of hypertrophy highly. I am sure that a weak and irritable heart may simulate very much a condition of hypertrophy, and yet, under suitable treatment, subside into a condition apparently healthy. A belladonna plaster is a remedy that generally does good service, and is to be preferred, I think, in most cases, to the subcutaneous opiate, which, in two cases where I tried it, produced more syncope than was at all agreeable to witness. Dr. Clifford Allbutt, however, assures us that subcutaneous injection of morphia in small doses is very valuable in so-called irritable heart, whether this be due to weakness of the organ or instability of its nerves. The first dose administered in this way should not exceed one-tenth of a grain.

8. A sort of diarrhœa, which I have occasionally met with in elderly persons, seems to me attributable with much probability to hyperæsthesia of the *mucous lining of the rectum*. The fæces appear natural enough in all respects; but the lower bowel is intolerant of their presence, and, in a fidgety petulant manner, insists on expelling them much more frequently than is at all convenient. Belladonna has appeared to me of some service in this condition of things, and still more, perhaps, cold sponging of the anus directly after defæcation.

9. Nocturnal incontinence of urine is an instance of hyperæsthesia affecting the *bladder*, to which it may be sufficient to allude. Let me, however, add that recently Dr. Barclay has praised syrup of iodide of iron as unfailing in this disorder. Belladonna in gradually increasing doses, you know, is the most approved remedy; but there is no reason why both should not be employed. A tonic will sometimes quell hyperæsthesia better than a sedative. Hyperæsthesia of the *urethral lining* is a marked feature in many cases of spermatorrhœa, and its removal by means of repeated catheterisation is reputed beneficial.

10. Hyperæsthesia of the *testis* is a very distressing neurosis; it reacts very injuriously upon the mind, and has in several instances induced sufferers to insist on the removal of the affected organ, which in three cases was found healthy by Sir A. Cooper. He considers it of centric origin; but it may be of reflex peripheral origin, as in the following case. A. C., aged 23, was admitted Nov. *18th, 1870.* He was in good health until five days previously, when he was *suddenly taken with pain* in the left testis; it was severe, and the testes began

to swell. In a day or two the right became painful also. These symptoms were succeeded by pain in the loins and head. The pain now affected him only when he stood up, or when the testes were pressed. There was very little swelling of either testis. He had some tenderness of the hypogastric region, and a good deal of straining at stool. He had no sleep at night. Temperature 103·5 deg.; pulse 80. He had thirst and anorexia. On the 15th he experienced a kind of tearing sensation at both external abdominal rings, going down into the testes. On the 16th, the temperature was 97·2 deg.; pulse 60, weak. The bowels had not been open since admission. His extremities became very cold the previous night; and he then became very hot and burning, and afterwards perspired profusely. He could not stand without support; the room and everything appeared to turn round with him. His testes were small, soft, very tender when touched, and the pain shot up along the cords. The bowels were now well opened by castor-oil, and afterwards an opium suppository of one grain was introduced. The testes ceased to be painful about four hours after the bowels acted, before he had the suppository. On the 17th, they bore handling well; he was also less giddy, could walk, but not very steadily. He had no pain anywhere. He was now taking tincture of cinchona and carbonate of iron, and by the 23d was getting quite well. The hyperæsthesia and pain of the testes in this instance were mainly dependent on the irritation caused by fæces in the lower bowel; but the action of this cause was probably much promoted by the existence of influenza enfeebling the nervous system.

11. We should be disposed *à priori*, to expect that an organ with such extensive and powerful sympathies as the *uterus*, would be prone to suffer from sensory disorder, and such appears to be the case. The affection originally termed "irritable uterus" by Dr. Gooch is regarded by him, Churchill, Mackenzie, Tilt, and Ferguson, as essentially a neurosis. The latter expressly says "the local changes have been the fluctuating, the nervous affection the constant, element". The tenderness is so great and constant that great suffering is experienced if the patient incautiously sit down upon a hard seat, or if coition be attempted. One of the seats of this neuralgic malady is the vagina itself, which is so exquisitely tender as to render intercourse intolerable. Here I think we cannot fail to recognise hyperæsthesia as the essential morbid element —at least if we accept the *dictum* of Dr. Ferguson above cited, confirmed as it is by Churchill's authority, to the effect that in the cases seen by him there was no ground whatever for the supposition of inflammatory action. Associated with hyperæsthesia, there may be, as we have before seen, more or less of hyperæmia and inflammatory congestion; but this is not in any degree the cause of the sensitiveness. Dr. Mackenzie's statement is of practical importance, that this neurosis may be of reflex origin, and dependent on gastro-intestinal irritation. In most cases, however, it seems to be primary, and to result from some cause of nerve-exhaustion, as great fatigue, acting, no doubt, on a weakly organised nervous system. The *retentissement* of this neurosis on the higher centres seems to be very pernicious; the natural emotions and faculties become miserably perverted, and all nobility of character is lost in absorbing selfishness. Sedatives, locally applied by pessary or enema, subcutaneous injections, and tonics, are our best pharmaceutical remedies; but change of scene and place, and a suitable climate, may be still more important.

12. Hyperæsthesia of the *female breast*, attended with more or less enlargement, is a well known phenomenon. It may suffice for me to have named it, and to cite Mr. Erichsen's authority for its not being unfrequently dependent on uterine irritation.

13. Hyperæsthesia *of the joints* is well known to surgeons since the publication of Sir B. Brodie's work on local hysterical affections, which I advise you all to read. The most constant symptom is morbid sensibility of the joints and adjacent parts, which is associated sometimes with more or less general tumefaction, apparently from a turgid state of the small vessels. I have known a condition of this kind, attended with sanguineous vomiting, yield to carbonate of iron, after an offer of amputation had been (fortunately) declined. I hope none of you will ever make such a *faux pas*.

14. As regards the *nerves of special* sense, I cannot doubt—taking first the

optic—that, even putting aside the case of strumous ophthalmia, where it might be contended that the photophobia was dependent on irritation of the filaments of the fifth nerve distributed to the cornea, there is really such a disorder as retinal hyperæsthesia. Mr. Soelberg Wells states that in this affection there is intense photophobia, though the eye is quite normal. There is often great ciliary neuralgia, the pain extending to the face and corresponding side of the head. One point which he mentions appears to me of especial interest—viz., that the peripheral portion of the retina is anæsthetic, while in the central vision is perfect and hypersensitive. This is very indicative of the true character of the disorder, of its affinity to paralysis. The causes are such as impair the general health or strain the retina; and the correct treatment is to rest and soothe and recreate nerve-power.

The *auditory nerve* also is liable to hyperæsthesia. Sir T. Watson mentions having observed this in a man moribund from cholera. Dr. Russell Reynolds affirms that hysteric girls do sometimes seem to hear through stone-walls. Intolerance of sound is an occasional symptom in the early period of meningitis, and is not unfrequent in nervous head-ache, and in azoturic sufferers. The only thing common to these various conditions is a lowered degree of vital power.

15. That the *cerebrum*—the great centre where impressions are consummated into perceptions—is liable to pass into a state closely analogous to that of a tertiary centre and sensory nerve when these are hyperæsthetic, seems to me not doubtful. Gooch mentions the case of a deranged lady, whose prominent belief was that her husband was unfaithful to her. The notion, so far from being unreasonable, was, he believes, true, and she had known it for many years without any unnatural disquietude, but now it engrossed all her thoughts: she neglected her ordinary pursuits, took a dislike to her friends, felt no interest about her children, and sat silent and motionless from morning to night. After continuing deranged several months she recovered, although she retained the same opinion. Gooch justly says her insanity consisted not in her ruling idea, but in its overwhelming influence over her feelings and conduct. A modern pathologist would probably express this by saying that her brain had become enfeebled and hyperæsthetic, and was, therefore, intolerant of the idea it had previously endured. The following occurrence is similar. A religious student, who was much harassed by petty scruples and fancies, and well aware of their pernicious effect, observed after a time that he was always more plagued by them while preparing for an examination; *i. e.*, when his cerebral power was most exhausted. The essence of hypochondriasis seems to be a special hyperæsthesia to corporeal impressions emanating *ab interno.* How much the tyranny of these may depend on enfeeblement of the great centres, is shown by P. Frank's account of his own sufferings from the dread of aneurism, induced by mental exertion, and removed by recreation. This is much too large a subject for me to attempt to handle now: all I can do is to commend to your attention the notion of cerebral hyperæsthesia as essential to the due comprehension of many common phenomena.

How far the *seat* of the action giving rise to hyperæsthesia may be purely peripheral, is difficult to say. In the great majority of instances, some nervous centre is probably engaged; but some facts—such as the removal of severe neuralgia for some months after section of the nerve, and the cessation of suffering after amputation of the testis—incline me to believe that the morbid action may have its chief *locus* at the periphery, and that without any apparent structural change in the affected part.

Pathological Change in Hyperæsthesia.—You may be disposed to ask me what this is. I wish I could tell you; but our knowledge of this matter is very scanty. All that we can say is, that it is undoubtedly a condition of imperfect nutrition, inasmuch as it may be generated by deprivation of blood, by injuries to nerves, by miasms in the blood, by remote irritation—in short, by all sorts of things that are injurious. Some particulars under these heads deserve to be mentioned. Hyperæsthesia of the most intense kind is produced sometimes by obstruction of the arteries with fibrinous deposit. Dr. Fuller describes a case where the first symptom was acute pain, associated with such exquisite tenderness of the whole limb that the slightest touch caused intolerable suffering. The hyperæsthesia continued until mortification set in. At the necropsy, all the large arterial trunks, from the iliacs downwards, were found blocked up by firm light-coloured coagula of long standing. No other cause existed for the hyperæsthesia in this case, except the deprivation of blood. In Dr. Waller's experiments on refrigeration of the ulnar nerve, the ultimate effect produced was anæsthesia in the course of the nerve, but this was preceded by hyperæsthesia. It can scarcely be thought that the latter was, under the circumstances, anything but the first stage of the former; and, therefore, the conclusion seems necessary that it implies not an exaltation, but a deterioration of vital power. Again, there is a case on record where, during the gradual recovery of the brachial plexus from an injury, the skin of the upper part of the arm possessed natural sensibility, another (probably the mid-part) was morbidly sensible, while that immediately beyond was quite insensible. Here, again, the conclusion seems inevitable that hyperæsthesia implies a damaged state of nerve-tissue, though not so severe as that which exists in anæsthesia. Much other clinical observation is to the same effect; the same morbid alteration of sensibility (cutaneous) is met with in typhus and typhoid fever, in influenza, in rickets, and in epidemic cerebro-spinal meningitis. On the other hand, there is not the least proof that real increase of functional energy renders an organ less tolerant of its natural stimuli: in fact, the evidence tends quite the other way. A vulture's eye, which can discern a carcase at an extraordinary distance, can easily endure the full blaze of tropical daylight, and is evidently altogether in a different condition from the eye of a photophobic invalid. Several similar instances might be cited; and, on the whole, the general truth of the view here taken seems to me established. In a recent lecture on Experimental Physiology, in the *Lancet*, February 11th, 1871, Dr. Rutherford has noticed this point without explaining it. He says, "the curious point, is that a nerve whose nutrition is to some extent defective should discharge its energy more readily than one whose nutrition is perfect." In what respect defective nutrition changes the nerve-tissue, I have said we cannot tell; but we may, I think, exclude œdema; for Buhl affirms as the result of his observations of continued fever, that the presence of much fluid in the subarachnoid and ventricular spaces corresponds with stupor or unconsciousness. Patients dying of delirium tremens have commonly wet brains, and they die for the most part in stupor. However, if we adopt, as I am much inclined to do, Dr. McDonnell's theory of nervous action, the detection of some material change may not perhaps be the most important matter. He assumes that the various impressions are propagated by undulations, the wave-length of which varies, so that the same conductor may be able to transmit several impressions. Now, on this view, it might be that undulations of a certain length and rapidity produced normal sensation, those of a less extent gave rise to hyperæsthesia, while a still further reduction of the undulations caused pain, and their total arrest anæsthesia. This may seem the more probable as there is undoubtedly a close connection between the three just-mentioned morbid states. They often occur together, acknowledge the same causes, and are removed by the same remedies. Another connection of hyperæsthesia is less frequent, but is extremely well marked in rickets, viz., that with vaso-motor paralysis. I use the latter term hypothetically, for the phenomena actually observed are hyperæsthesia of the cutaneous surface and profuse sweating. The latter, however, as I have argued elsewhere, seems to be distinctly traceable to defective contraction of vessels from a paretic state of their nerves; and if this be so, then the connection is as I have stated. Admitting this, it is difficult to see in the hyperæsthesia anything else than a species of paralysis very similar to that which affects the vaso-motor nerves, and which may itself depend on defective molecular movement.

Allied, however, as these morbid states are, there are differences between them, which are important as regards treatment; thus anæsthesia is more connected with prostration, and is more benefited by stimulants than hyperæsthesia is. The latter is sometimes intolerant of these, and yields better to remedies of the sedative class. Thus in a case of pain with anæsthesia, bark, ammonia, and wine suggest themselves; whereas if hyperæsthesia be in the as cendant, bromide of potassium or chloral, or subcutaneous opiates, seem more hopeful remedies. Yet we must not strain this rule, for it admits of numerous exceptions. Neither must we ever omit a careful inquiry into the cause of the hyperæsthesia, for the knowledge of this may be all important. It is idle to go on administering tonics or sedatives if a focus of irritation need to be removed, or if the disorder be dependent upon poisoned blood. Here is much room for the exercise of *real* diagnostic skill, not that which contents itself with affixing a name to a given case of disease.—*British Medical Journal, Sept.* 30, 1871, *p.* 369.

13.—ON THE INFLUENCE OF THE NERVOUS SYSTEM ON DISEASES OF THE ORGANS AND TISSUES, AS ILLUSTRATED BY THE ACTION OF COUNTER-IRRITANTS.

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[The following is part of a lecture upon the subject of "the Influence of the Nervous System on Diseases of Organs and Tissues." After treating of the morbid results of 'trophic neuroses,' the writer asks what practical lessons in the use of remedies are to be deduced from the facts and conclusions he has brought forward. He takes the class of counter-irritants as illustrative of others; and as they are in daily use, and their *modus operandi*, after numerous recent discussions, very obscure, he endeavours to elucidate their application.]

What is meant by counter-irritation? The question is not so simple as it appears, for the solution of various and wholly distinct problems is included in it. I think, however, we may affirm generally that the change we induce by counter-irritants locally is of the nature of inflammation, either with or without effusion, exudation, and, I may add, suppuration, in the case of setons and issues.

In all observations of this kind we must not, however, forget a general fact deduced from clinical observation and experimental research, and very manifest in the growth of cutaneous hair and scales—viz., that not only do the tissues themselves undergo normal vital changes independently of nerves or of a nervous system, but also that nerves and nerve fibrils perform their appropriate functions independently of nerve-centres. Nerves are produced anew in organised plasma, and join on to the general trunk, in accordance with the order of embryonic development of the nerves from the periphery to the centre. MM. Philipeaux and Vulpian made numerous researches, which prove that nerves separated wholly from the nerve-centres, and completely altered as to nutrition, may become regenerated, although remaining separate, and recover all their vital properties. Numerous experiments, also, on the nerves of muscles show that those motor nerve-fibrils have their own inherent properties in entire independence of brain, spinal cord, or nerve-centres, and not only in separated limbs, but in muscles that have been cut from the limbs. Unless these facts be borne in mind, it is not possible to appreciate the influence of even the sympathetic ganglia on morbid changes in organs and tissues.

Again, varying results follow, according to the nature of the irritant used. When certain drugs are the means, then, besides their local action, there is that which results from their absorption, as, for example, spirit of turpentine, which, when used as a "rubefacient," gives a peculiar odour to the urine, or that element of the cantharides-blister which excites strangury. Leaving this point out of consideration for the present, we may say further that, as to the tissues affected, all counter-irritants (amongst which heat and cold, and other physical irritants must be included) act either locally on the tissues, including the nerves and blood vessels, or else on the nerve-centres, through the nerves, and thence, by reflex action on the same, or a distant organ or tissue. *We thus arrive at this general principle—that the laws of reflex action adapted to a trophic anatomy* *and to neurotic changes in tissues and organs, must be our guide in the use of counter-irritants and rubefacients.*

This being so, one practical conclusion follows directly—viz., that for the purpose of effective counter-irritation, beyond merely local results, it is necessary that there be continuity of sensory or afferent trophic nerve between the surface irritated and the nerve-centre to be acted on, otherwise no change can be effected therein. Again, if there be no continuity of efferent or motor nerve between the part to be modified and the nerve-centre which modifies, no counter-irritation will avail, although the sensory communication be continuous. Thirdly, since any surface may be made available for exciting reflex action, so in like manner other surfaces than the surface of the body may be available for counter-irritation. Hence, when we excite vomiting by tartar emetic, purging by croton oil, scammony, or other drastic purgatives, and strangury by cantharides, we counter-irritate as certainly as when we apply sinapisms to the feet, fumes of ammonia to the nostrils, and fly-blisters, turpentine, or chloroform to the skin. Fourthly, we may go further, and say that certain inflammations of these surfaces, induced otherwise than by counter-irritants, will have a diastaltic action analogous to that of counter-irritation.

Having thus, then, widened our view of matters, let us take up a few practical points. Firstly, we desire to alter the condition of the nerve-centres in centric diseases by counter-irritants. The time-honoured use of sinapisms to the feet and wrists in apoplexy and comatose affections generally, is a familiar example of direct irritation through afferent nerves; so, also, the irritation of the nasal branches of the fifth nerve in cases of failure of the heart's action; but you may widen much the sphere of these remedies. For example, I have seen cephalic snuffs of great use in epilepsy, and I have no doubt they might be applied with advantage in other encephalic diseases of defective nutrient and vascular activity. The ancient Greeks used this class of remedies so systematically in head affections, that they invented a double-piped syringe for the purpose of injecting counter-irritants into both nostrils at once. In cases in which it is advisable to stimulate the nerve-centres through the afferent nerves, hot applications, as Donovan's button, may be applied, or rubefacients, which excite more or less pain and tingling. In certain cases of paraplegia I have found it very useful to cover the lower extremities with sinapisms for half an hour two or three times a day, so as thereby to excite vaso-motor activity in the motor cord through the sensory nerves. In these cases it is necessary that there be at least sufficient integrity of the sensory nerves and nerve-centres to evolve and transmit the regulative or sensory vis nervosa. If there be complete anæsthesia from structural change, little good will be done; but if the lost or diminished sensibility be only functional, you may re-excite centric sensory activity by stimulating the sensory nerve-fibrils, in accordance with the Wallerian law of line of physiological activity that I previously explained to you; for, just as sensory centric disease may begin at the periphery, so, also, may the cure. This rule applies to all those trophesies in which there is sensory defect, but not to the extent of absolute anæsthesia—a mere trophic debility being the result. In this numerous class there is commonly a defective evolution of both motor and sensory vis nervosa; but as the evolution of the one is excited by the other, it is plain we must act therapeutically upon the sensory system in the first instance, and this can be done by counter-irritants. It will be a daily problem for you to determine how far pain or tenderness on pressure should indicate, or contra-indicate, the use of counter-irritants. The facts to ascertain are the causes of the pain. Is it due to local causes, as when a wound is inflicted, or, if there be local causes, has pain preceded or followed them? It is too often assumed that the pain is wholly due to local changes; whereas it either precedes or coincides, as in certain sensory trophesies. The connection between pain, as neuralgia, and metastasis in rheumatism and gout, shows how certain trophic changes are dependent on the sensory system. A morbid change is often the more dangerous, in fact, because painless, as in the case of paraplegia I quoted, or because it ceases with cessation of pain, as in metastasis. In examples of this kind, pain is only a stage to that trophic palsy which is indicated by complete anæsthesia. Hence the *importance of studying* morbid-tissue change in relation to pain and painful states, whether of body or mind, and under this head the relations of neurotic diseases of organs and tissues to mental work, and to mental pleasure and pain.

It may be held as a general principle that diminution and loss of sensibility are associated with defect in that centric regulative property by which tissues and organs resist causes of disease. Now, rubefacients and counter-irritants are such causes; so that, when applied to the skin, when there is that defect I have termed trophic nervous debility, the results may be a ready morbid reaction to the counter-irritant far beyond that desired. This kind of ready reaction is seen in the skin in cases in which there is lowered innervation—as in measles and other exanthemata, in low forms of typhus, diphtheria, and influenza. In such specific fevers fly-blisters for pneumonia are worse than useless, being likely to bring on low inflammation, if not sloughing. There is a like tendency in this class of diseases to low inflammatory action of the skin from pressure, and of the cornea from slight irritants, just as is seen in paraplegia and in the eye and nostrils in disease of the Gasserian ganglion. Such facts alone would indicate that in specific fevers the trophic nervous system is involved.

On the other hand, in a numerous class of cases—in those, for example, termed "idiopathic inflammations"—there is a healthy or normal reaction against injurious agencies, and, of course, a comparatively healthy nervous system. How shall we use counter-irritants in these? I think we can still fall back upon the laws of reflex action for guidance. When an irritant is applied to a nerve, and excites reflex movements, they are of a conservative character; so, also, when it excites the reflex nutrition, which is the result of its action on the trophic system. But this is only seen when the regulative or sensory system retains its functions; otherwise there is no healthy reaction, but rather a tendency to what is unhealthy. If, then, the counter-irritant fails to excite the afferent and regulative system normally, it is useless, and may be injurious. Pain is a natural sign of injury. When, therefore, there is tenderness on pressure, or pain referred to the regions of an inflamed organ, would that indicate the use of counter-irritants? Here, again, the question is more complex than it appears on first consideration. In some cases there is hyperæsthesia of the skin only, and no pain in the organ than what may arise from over-distension or other local and merely mechanical causes. Further, the pain is not always referred to the seat of the inflammation, but elsewhere. Thus, occasionally, in both pleurisy and pneumonia, it is not experienced in the affected side, but in the opposite; so that the question arises whether decussating nerves are affected sensorially, and direct trophic nerves motorially. Upon the whole, in acute cases, both experience and theory are opposed to violent (*i. e.*, inflammatory) counter-irritation—rubefacients, at the most, are all that are needed; but even these are more doubtful than local sedatives with warmth and moisture to the skin. The local morbid condition has already caused those centric changes upon which pain depends, and the normal reaction has followed; this being so, it seems advisable to relieve the pain rather than increase it. In this way the great additional suffering may be spared the patient, which the "heroic" use of blisters, tartar emetic, croton oil, and other inflammatory counter-irritants inflicts.

I think we can also make these views available to a better comprehension of the uses of counter-irritants when applied to induce absorption of fluid in dropsical joints and elsewhere. Do they act directly and solely on the absorbents, as is generally believed, or on the nerve-centres as well? When a blister is applied to the side to excite absorption, it is more difficult to understand how it acts locally than through the nervous system, if we remember that the so-called serous metastasis is nothing more than the rapid absorption of serum from a change in a nerve-centre, as in the case of dropsy I alluded to, in which rapid absorption followed on embolism of the middle cerebral artery of the same side. Again, if we give the so-called hydragogue cathartics in acute hydrocephalus, ascites, and general dropsy, we use very active counter-irritation of the intestinal surfaces, and in this way act as certainly on the nerve-centres as when blisters and rubefacients are applied to the skin. So, also, when we use the hot-*air bath in dropsies,* we do much more than stimulate locally; we apply a powerful agent to the nervous system. On the other hand, blisters applied to joints under certain conditions will excite effusion.

I could multiply illustrations of this kind, if necessary. Irritants to the stomach, for example, act as counter-irritants on the lungs as well as on the brain; irritants to the kidneys, urethra, and cervix uteri, are counter-irritants to both brain and spinal cord—in short, all the diastaltic actions of irritated mucous surfaces and of morbid viscera are of the nature of counter-irritation, and may be induced artificially. I will only mention one curious illustration:—Irritation of the urethra not unfrequently excites violent rigors. The New Zealanders adopt a coarse means for this purpose, as a counter-irritation for the cure of tetanus.

A clinical trophic anatomy should enable us to observe and treat two kinds of morbid neurotic changes—viz., those which occur in the nerves and nerve-centres themselves, and those which occur in organs and tissues in consequence of those changes. We have only studied the latter class; we will now study the former.

Let me premise as a fundamental fact that, apart from certain morbid states of the blood and of the auxiliary tissues, or what have been termed coarse and mechanical diseases of the brain and nervous system, every change in a nerve or nerve-centre is, for the most part, the result of a change in some other portion of the nervous system. So that, to understand the true causes of numerous neuroses, it is necessary to trace the order of events which lead up to them. This we have done generally, and without special reference to the kind of morbid changes which take place; now we have to specialise them as far as is practicable and practical.

In my lecture on the Clinical Observation of Diseases of the Brain and Nervous System, I explained to you the general rules to be observed so as best to ascertain the various changes therein which constitute the predisposing, exciting, and proximate causes of neuroses in general. When, however, we have to consider those of special nerve-centres, it is necessary to determine, at least, two things—viz., the seat of trophic changes in the nerve-cells and nerve-fibrils, and the relations of vaso-motor action to these changes. As to the nature of the trophic and dynamic changes, I need only remind you of this—that no anatomical research, however minute and microscopic, whether conducted in living or in dead tissue, can reveal to us those molecular changes upon which the varying functions of the nerve-centres depend. All we can say positively of these, as of molecular chemical changes, is, that there are such changes, and that each kind must occur in its own proper portion of the nervous system, in accordance with the functions of that portion, and according to fixed laws. All visible structural changes in nerve-tissue are coarse diseases, and coincide with destruction or abolition of function. They indicate the results of trophic disorder, often of long standing, but not the causes of the disorder itself.

In respect to the vaso-motor activity of these nerve-centres we know more—but little that is positive and practical. We have not determined the vaso-motor relations of the nerve-centres to each other; anatomical and physiological research have failed as to the most fundamental centres—the sympathetic ganglia; so that our ignorance of their functions is generally admitted. When we turn more especially to those ganglia which regulate the activity of the blood-vessels that supply the encephalon—viz., the cervical ganglia—we have not only to deal with the carotid and vertebral systems, but with that of the thyroid body and its relations to the functions of the brain and spinal cord. Looking at these relations from the anatomical and vaso-motor points of view, we cannot avoid the conclusion that the thyroid body has more complex trophic functions than is generally suspected. This is shown by its twofold blood-and-nerve supply. The *nervi molles* (so called) are the branches of the system of the superior cervical ganglion which accompany the arteries of the carotid system; these also accompany the superior thyroideal artery as a limb of that system, whereas the inferior thyroideal artery belongs to the system of the vertebral arteries, and receives nerve-fibrils from its own ganglion, the middle cervical. We may reasonably conclude, therefore, that as the innervation and arterial supply of the thyroid body are double, the functions are double. That these functions are in connection with those of the other cervical ganglia is probable from the intricate anatomical relations of the whole chain; these are so close that the thyroideal ganglion is sometimes integrated (an important fact) with the lower cervical, while it has commissural connections, not only with the superior cervical above, but with the cardiac ganglion below. Now, no one, so far as I know, except myself, has called attention to the clinical relations of these facts to goître and cretinism, to vascular bronchocele and "Graves' disease," and to the functions of the ovaria and uterus.

How much considerations of this kind should make us hesitate in drawing conclusions from experimental researches into the functions of the cervical sympathetic is plain enough; and it is precisely such considerations which enable us to understand why, with all the laborious research I have alluded to, it is a general complaint that we know so little, practically, of the pathology of the sympathetic system. According to vaso-motor theories, the cervical sympathetic ganglia should regulate the encephalic circulation; consequently, in encephalic neuroses like epilepsy and its serious accompaniments of morbid brain-nutrition, leading to insanity and dementia in fevers with delirium, and in insanity in general, the pathological anatomy of these centres should elucidate the pathology; yet how little of this is known! They have been found red and swollen in a few cases of death from hydrophobia, and from typhus; but of their state in insanity, epilepsy, hysteria, and various vaso-motor affections of its encephalic tissues, the circulation within which is supposed to be regulated by these ganglia, we have few, if any, observations. The cause of this neglect is not far to seek: it is the chaotic conflict of views as to the anatomy and physiology of the sympathetic system. Feeling strongly these difficulties, I have lately adopted a new method of observation of this great group of encephalic neuroses, which certainly helps to simplify bedside work and to clear up numerous doubtful and embarrassing problems. I shall, therefore, explain it to you. To this end I will first, however, explain the guiding principles of the method.

I have already on various occasions indicated the clinical and practical uses of those great laws of evolution and development which are termed transcendental, and which hitherto have had little attention from Practitioners and teachers. I once again turn to this department of biological science for help. It is generally yet erroneously thought, in accordance with scientific terms, that the nerves "arise" and are developed from the nerve-centres, and the arteries from the heart and large vessels. Hence the numerous discussions as to the "origin" of the sympathetic, and of the "roots" of various nerves both spinal and encephalic. Now, in the development of the embryo the nerves are formed independently of the nerve-centres, and the arteries appear before and act independently of the heart; so that the evolution of the vascular system, with its accompanying nerves, is not like that of the branches of a tree from a common trunk, but like that of a river from a number of rills, or like that of veins. The function of the central ganglia is to unify trophic changes in tissues and the actions of the vessels and of their accompanying nerves; but these changes and actions can and do go on independently of either heart or nerve-centres. If we go a step further and inquire to what uses vessels and nerves are subservient, we learn at once that they subserve to cell-function, and that cells can be grouped together, each group having distinct functions to perform in the body politic. My late friend and colleague, Professor Goodsir, first promulgated the doctrine—afterwards confirmed by Virchow—that not only is the entire organism composed of simple or developed cells, each having an independent vitality, but that there is also a grouping of cells into departments around one capital or central cell. A common and striking example of this kind is the primordial cell of the ovum. I have pointed out in my psychological text-book how the laws of the fundamental process which go on in these cell-groups may be formulated for practical use as the correlative laws of life and thought.

Guided by these facts, I take an area of blood-supply as indicative of an area of cells and tissues in functional and trophic relation with each other, and with a *common source of blood* and of regulative vis nervosa, both vaso-motor and trophic.

These areas may be marked out in the encephalic tissue, and as special centres of functional cell-activity, in two ways—viz., by the descriptive and pathological anatomy of the arteries, and by the observed functional and trophic changes in the corresponding areas of blood-supply; only, instead of looking at the arterial trunks as they give off branches, we must consider them as made up of branches. I think Serres was the first to show, in his "Anatomie Comparée du Cerveau," published in 1824, that the development of the encephalic nerve-centres is dependent on the development of the arteries, and that the nerves, whether sympathetic, spinal, or encephalic, grow as it were towards the nerve-centres with which they are finally connected. Since, as the areas become more and more comprehenisve, a larger blood-supply is needed and more regulative vis nervosa required, it follows that with an increased extent of central control the arteries get larger and the nerves thicker; so that each important trunk or branch of nerve and of artery may be held to indicate a centric trophic area. Guided, therefore, by this principle, I have divided the cerebro-spinal axis into distinct yet mutually dependent arterial areas, each presumably with its sympathetic ganglia, its commissural connexions, its correlative cranial and other structural developments, and its sphere of physiological and pathological changes in organs and tissues. The method is so wholly new that it is as yet imperfectly developed; you will, however, find these arterial cerebro-spinal areas set forth in a tenative way in my psychological text-book. On future occasions I shall apply them to the study and treatment of hysteria, epilepsy, and insanity; at present we will see how far they will help us to a knowledge of the relations of the encephalic vaso-motor or trophic centres to neurotic diseases of tissues, both within and without the cranium.

Serres, in the work I referred to, lays down two laws of development of the central nervous system—viz., the law of symmetry or of symmetrical halves, and the law of conjugation or of integration of the two halves of the nervous system into one organ. The cerebellum, for example, is fundamentally double—a lateral leaflet coming from each restiform body, representing in fishes that which in man constitutes the hemispheres. The median lobe (or superior vermiform process) is developed in relation with the tubercula quadrigemina. The arterial development follows the same laws. On each side there are symmetrical arteries, and where there is conjugation of nerve-centres we find conjugation of arteries either by integration or by anastomosis of the trunks. When the centres are distinctly ganglionic, united by commissural nerve-fibrils, we shall have arterial trunks united by anastomosing arteries; but where the two symmetrical halves combine, both by commissure and ganglion, there may be union of the arterial trunks of each half, either by complete integration into one trunk or by a cross anastomosing branch; so that anastomosis and integration of arteries, and *vice versa*, represent commissural union of nerve-centres in relation to them. The aorta is thus constituted out of two lateral vessels. It has, therefore, a corresponding ganglionic and commissural centre somewhere in the cord or the encephalon. The integration of the two vertebral arteries into the basilar artery at the lower border of the pons Varolii, and in exact accordance with its length, indicates that the pons is the commissural centre of those groups of cells to which the blood is distributed from thence. This great vascular area is in functional relation, therefore, with the most complete commissural centre of the encephalon and its dependencies—the "vital knot," as the French anatomists, with much propriety, term it. Nor is this coincident unity of arteries and development of correlative trophic centres a solitary fact, as we shall soon see, but a general law. After the union of the two vertebrals with the basilar artery, two subordinate areas of innervation in connexion with the pons are marked out by two distinct sets of branches from the basilar. One of these includes the cerebellar arteries, and supplies the cerebellum, pons Varolii, medulla oblongata, and the upper portion of the spinal cord. This cerebellar vascular area is further divisible into two areas, supplied by the posterior and anterior cerebellar arteries respectively. Of these, the posterior as that which corresponds with the commissural connexions of the cerebellum with the cord; for, although often found to be given off from the basilar, it is not unfrequently a branch of the vertebral artery on one side, and of the basilar on the other. These two cerebellar vascular areas indicate, therefore, a group of different nerve-centres, which may be referred to the two commissural vermiform processes; for while the posterior or cerebello-spinal artery is ultimately distributed to—which means in development that it primarily arises in—the inferior vermiform process and the sides of the median fissure, and the inferior surface of the cerebellum, the superior cerebellar artery supplies the superior vermiform process (median lobe), the velum interpositum, and the valve of Vieussens. A small branch arises in the internal auditory meatus, and this seems to correspond to that branch of the auditory nerve which has been traced to the cerebellum. Tinnitus aurium and spectral voices thus acquire a diagnostic significance as to the state of the circulation in cases of brain disease of this occipito-spinal region. A few branches go to—*i. e.*, in development, come from—the under surface of the sphenoidal lobe, thus connecting this cerebellar branch with the function of that lobe.

To understand clearly, however, the important clinical relations of these areas to epilepsy and insanity commencing therein, we must include the blood-supply of the basilar region of the cerebrum and of the ganglionic centres situate therein. We know little of the functions of several of these; but so much is certain, that they appear early in the scale of development, and are obviously in essential cônnection with the corporeal centres proper. The two posterior cerebral arteries of the basilar area indicate the bond of union. They supply blood to the inferior surface of the posterior lobe, to the crura cerebri, tuber cinereum, and corpora albicantia, while a choroid branch connects the velum interpositum and tubercula quadrigemina with the basilar area, as well as with the vascular choroid plexus. All these encephalic nerve-centres and tissues are therefore within this great occipito-spinal and corporeal area of vaso-motor activity, to the neuroses of which the vaso-motor activity of the vertebral arteries is the clue. Their vaso-motor connection with the vascular areas supplied by the internal carotid system is indicated by the posterior communicating arteries. According to my view, these correlate a corresponding commissure, and this, I think, is the fornix or great inferior longitudinal commissure of the hemisphere. So that the posterior communicating arteries are to the fornix of each hemisphere what the basilar artery is to the pons Varolii.

Looking, then, at the anatomy, physiology, and pathology of the encephalon from this new point of view, we can broadly allocate function, blood-supply, and trophic change to three distinct regions, any one of which may be involved morbidly without the other manifesting change, yet connected with each other, and of which each symmetrical half may be involved without affecting the other half; these are—(1) the corporeal region, including the medulla oblongata supplied by the cerebellar arteries; (2) the animal, supplied by the posterior cerebral; and (3) the mental proper, or intellectual, supplied by the internal carotid arteries. The commissural union, or conjugation of this last-mentioned region, is indicated by the anterior commissure; and its correlative anterior development is either the anastomosing anterior communicating artery, or, as is sometimes found, integration of the two anterior communicating arteries, so as to form one trunk, like the basilar. Thus the circle of Willis is constituted so as to represent in the arterial system the commissural connexions of the encephalon.

Various difficulties and objections at once arise at first consideration of these facts and conclusions. Primarily, it will be said that these anastomosing branches are simply for the purpose, and no other, of equalising the circulation within the brain, and providing against any cutting off of the blood-supply to any particular part; this is what the anatomical books teach. Now, seeing that one half of the brain may do the work of both halves, just as one kidney may do the work of both, it is important that there shall be a ready diversion of the proper supply of blood to the working half; and to that extent the explanation is satisfactory. But we have to consider more than the working of a hydraulic machine when we turn to the changes which occur in the blood, in the capillaries, and in the cells and tissues to which the blood is distributed during functional activity of a portion of brain-tissue; so that, although the *equalisation of the blood-supply* be attained, we have still to explain the vaso-motor and trophic changes in the respective nerve-centres in relation to each other, which my method helps.—*Medical Times aad Gazette, May* 27, *and August* 11, 1871, *pp.* 595, 211.

14.—CALABAR BEAN AS A REMEDY IN CHOREA.

By Dr. W. H. Fuller, Physician to St. George's Hospital.

[In consequence of a paper by Dr. John W. Ogle (*Retrospect* vol. lii., p. 71), and another by Dr. Maclaurin, of Greenwich, reporting cases of chorea successfully treated with calabar bean, Dr. Fuller determined to give the remedy a trial.]

As these were the first, and, as far as I am aware, the only occasions on which the drug had then been exhibited as a remedy for chorea, it occurred to me that before accepting the favourable conclusions to which those three cases pointed, it would be advisable to follow up the experiment so begun, with the view of ascertaining whether the drug was indeed what it appeared to be, namely, a valuable remedy in this troublesome disorder. The results of my experience, I regret to say, have not confirmed the newly acquired reputation of the drug. I gave the bean in the form of tincture to seven consecutive cases, which were all of average severity. The drug was administered alone, the dose at the commencement being ♏x. of the tincture* mixed with an ounce and a half of water. This was usually given three times, but in two instances four times a day, and the dose was increased gradually but rapidly, usually by about ♏v. every second day, until a point was reached at which the characteristic poisonous effects of the bean began to manifest themselves. The pulse became accelerated, feverishness supervened, loss of appetite and vomiting ensued, and uneasy sensations were complained of in the head. A drachm and a half or two drachms of the tincture taken three times a day usually sufficed to induce these symptoms. Beyond this, however, no appreciable effect was observed. There was no evidence of the cumulative action of the drug; the drug did not appear to influence the secretions; it did not materially affect the pupils; and it certainly did not exercise the slightest influence over the disease. Thus I am forced to the conclusion, that whatever other claims the physostigma may have to be regarded as a valuable internal remedy, it has none as a curative agent in chorea; and that the improvement which was observed to follow its administration in the two or three cases previously recorded, resulted probably from the careful regulation of the diet and the general improvement of the health which accompanied the patient's residence in hospital.—*St. George's Hospital Reports, Vol.* v., *p.* 13.

15.—TURPENTINE IN NERVOUS HEADACHE.

By Dr. J. Warburton Begbie, Fellow of the Royal College of Physicians, Edinburgh.

[The following is from a paper on the therapeutic uses and actions of turpentine.]

In this painful affection, occurring in young persons of a delicate excitable temperament, without any menstrual or leucorrhœal complication, Dr. Graves placed great reliance on turpentine. He gave it in doses of one or two drachms, to be repeated according to its effects. The best vehicle, he adds, is cold water. Some will bear and derive advantage from two or three doses of this medicine in the day, experiencing from its use a diminution of headache, and removal of flatulency, together with a moderate action of the bowels and kidneys. There is, moreover, another class of sufferers from headache, and this composed of both sexes, who may be relieved by turpentine. I refer to the frontal headache which is most apt to occur after prolonged mental effort, but may likewise be induced by unduly sustained physical exertion, what may be styled the headache of a fatigued brain. A cup of very strong tea often relieves this form of headache, but this remedy, with not a few, is perilous, for, bringing relief to pain, it may produce general restlessness, and, worst of all, banish sleep. Turpentine, in doses of twenty or thirty minims, given at intervals of an hour or two, will not only remove the headache, but produce, in a wonderful manner, that soothing influence to which reference has already been made. One dose is not uncommonly sufficient, but it is rarely necessary to repeat the remedy for more than two or three.—*Edinburgh Medical Journal, July*, 1871, *p.* 50. .

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* *Made by macerating one drachm of the* powdered bean in one ounce of rectified spirit.

16.—A NEW TEST FOR HYSTERIA.

A French work just issued by Dr. Chairon, Chief Medical Officer to the Vesinet Asylum, entitled "Clinical Studies on Hysteria," announces the discovery by him of a new pathognomonic sign of hysteria, which, should it be confirmed by experience, will prove to be a valuable contribution to medicine. Since Dr. Chairon has become connected with the Institution, he has passed under view 26,000 female patients, amongst whom were a great many cases of hysteria. He says that he has ascertained that in every one of them the commencement of the affection has been marked by a special sign—insensibility of the epiglottis.

The determination of this symptom, which is constantly present, is very simple. It is sufficient to introduce gently the finger into the mouth, so as not to frighten the patient, and place it on the base of the tongue. It will be found that the epiglottis may be touched, displaced and scratched with the nail without producing the least regurgitation. When this symptom exists there will be found invariably a congestion of one or both ovaries, usually of the left.

Singular as this proposition is, the author proceeds to prove its exactitude, and has, with that object, quoted a great number of cases collected at Vesinet.—*Medical Press and Circular, May* 3, 1871, *p.* 381.

DISEASES OF THE ORGANS OF CIRCULATION.

17.—ON THE ACTION OF THE HUMAN HEART.

By the Rev. Samuel Haughton, M.D., D.C.L., F.R.S., &c.

[The story of the heart is a wonderful and mysterious one, and deeply interesting both to us personally and as medical men. The following is from one of Prof. Haughton's lectures at the Royal Institution, upon the principle of Least Action in Nature—in other words, the principle that nature will effect a certain result with the least effort possible. Some of the results obtained and explained in this paper have been the product of very great labour.]

We have first to consider the question of *the amount of work done by our hearts.* The heart is a small muscle, weighing only a few ounces, beating perpetually day and night, morning and evening, summer and winter; and yet often an old man's heart nearly 100 years of age is as perfect and complete as when he was a young man of 20. In order to measure the force and power of the human heart, the most obvious way is impossible, because it would require the death of the person on whose heart the experiment was made. We have experimented on the hearts of horses, oxen, dogs, and sheep, however. The first of these series of experiments were made by the celebrated Dr. Hales of the last century, who measured, by direct experiment on the vessels of these animals, the amount of hydrostatic pressure to which they were subjected. These experiments showed that it varies in the horse and ox, amounting to about nine feet; and in the smaller animals to somewhat less. We can calculate the total amount of work done by the heart of an ox, by the heart of a horse, by the heart of a dog, or of a sheep. But it would be impossible to perform such an experiment upon man, because the experiment is accompanied with certain death.

The co-efficient of capillary resistance must be determined. The heart pumps the blood into the capillary vessels, which permeate every tissue of our frames, and the greatest resistance lies in these capillary vessels. The co-efficient of capillary resistance, as I have determined it in some of these animals, is as follows:—in the sheep, $\frac{1}{186}$; the dog, $\frac{1}{196}$; the horse, $\frac{1}{393}$; the ox, $\frac{1}{398}$.

You observe that in the sheep and dog the co-efficients are double those of the horse and ox. These large and small animals naturally group themselves together. Now, with which of these groups of animals are we to associate ourselves in making a calculation of the amount of work done by our hearts? We cannot perform the direct experiment, but an accident on one occasion placed in my power the means of making a close approximation to the true result. When the artery of the horse or cow is cut we can measure with ease the height to which the blood will spout. But we find that the artery does not spout to the height of nine feet, as we should have supposed. There is a hydrostatic pressure of nine feet in the horse, but the blood will only spout to the height of two feet and a half. The reason of this is, that Nature shuts off the pressure to prevent the loss of blood. If we could by any process arrive at the knowledge of the height of blood from the wounded artery of man, and compare it with that of the horse, cow, and sheep, we should find the animals to which man is most closely allied in this respect are the larger. On March 18, 1863, in the Meath Hospital, Dublin, an operation was performed on a poor man in whom I was interested. I was merely a spectator, and was therefore able carefully to watch the following phenomenon in the course of the operation. A large artery was cut through, because it occurred in an unusual place. The blood spouted in jets for a minute or for two minutes before the artery could be tied. When the operation was over, I examined with care the height of the table on which the man lay, and the exact position of the furthest spurts of blood upon the floor; and by the application of a little geometry and mathematics to this problem, I was able to ascertain—by taking the spots of blood thrown to the greatest distance—the velocity with which the blood flowed from the wounded artery. The course of the blood is a parabola, and, two points being given, we can construct the angle of elevation and the velocity of the fluid. As soon as I had made this calculation, I found that, if I cut the artery of a man, it would spout to the height of 2·58 feet, or rather more than thirty inches. Now, taking the mean of all Dr. Hales's experiments upon horses, I find that it is 2·53 feet. We cannot, indeed, compare the hydrostatic pressure of the human heart directly, but we can by this determination of the velocity of the spouting blood show the force of the circulation in our system, and apply to it the co-efficient of resistance which we find directly from the horse and ox. When this co-efficient of the hydrostatic pressure inside the human heart is used, and knowing how often the human heart beats—seventy-five times every minute—we can calculate the amount of work done in a given time by the human heart. This work I shall represent in an extremely easy way, and show you the extraordinary amount which it attains. If I cut out an ounce of muscle from the heart, and ask myself the question—What number of pounds can that contracting muscle lift in the course of a minute? I find that a single ounce of the human contracting muscle will lift 20·576 pounds through the height of a foot in a minute! This I believe to be a close approximation to the power of the heart; but, inasmuch as it is not obtained by direct but by indirect reasoning, I tried to verify it in another way; and in verifying it by a second process I made use of an interesting phenomenon originally observed by the celebrated Dr. Wollaston, and recorded in the *Philosophical Transactions* for 1809. He was the first person who noticed that *when our muscles were contracting they gave out a deep note.* If any of you wish, you may try it for yourselves. If you go into a room alone, and in perfect silence, place your elbows on a table and place your fore-fingers lightly in the ears clenching the hands at the same time, you will hear a sound you never heard before—a deep, low hum; or, if you waken at night and clench your teeth, so as to call the masseter muscles into action, you will hear a hum, the pillow acting as a sounding-board. You can, therefore, soon satisfy yourselves that the action of a muscle is accompanied by something which produces a musical note. My attention was directed to this in a remarkable way. A young Physician of Marseilles—Dr. Collongues—was directed to the study of this curious phenomenon in examining some of the cases of cholera which died. I found the same fact in Dublin at the same time, and quite unknown to Dr. Collongues, as his discovery was to me. The patient in cholera has his temperature much lower than in health. 98° F. is the natural heat of the blood, and it is well known to Physicians that if it rises some 7° or 8° the patient will die; and so, if the temperature of the blood falls about the same number of degrees, the patient will die. But it is a strange fact that, if you examine the body of a patient who dies of cholera, it is warm. The temperature rises after death to 103° or 104°, as if the person was in a fever. This is accompanied by the movements of the limbs which cause such great alarm to those who know no better. Happening to place my ear against a person who had just died, I heard a hum. I placed my ear to the heart, to see if he was yet alive, and if I could save his life, but he was dead. His muscles still continued to live after the heart, which has been called *primum vivens ultimum moriens*—the first to live, and the last to die. After the heart has ceased to live and the brain to think—after the man is dead—the muscles still live and have the last traces of life in the body, like the last notes of the harp which vibrate when the master's hand has ceased to play. I constructed a number of organ-pipes, and succeeded in imprisoning the muscular hum in one of them, where I could afterwards measure its correct kind and force at my leisure. The muscular hum which vibrates is C on the musical scale, two octaves below the C of the bass staff, or D, two octaves below D on the bass staff. I found that it is certainly between C or D, and by fixing a second organ-pipe to my own muscles, and comparing them, I was able to ascertain with a considerable degree of precision the exact hum. This I made to be 35½ vibrations per second.

Well, after I had published this result, Collongues, who had removed to Paris, sent me a book, in which he had succeeded in proving by tuning-forks that the number of the vibrations of muscular contraction is thirty-six. This very remarkable result, obtained by two persons with different instruments, attracted attention, and Dr. Collongues was uneasy about the priority of the discovery. I went over to see him. When I admitted his priority, and said that I was more pleased to find that we had both found out such a remarkable fact than that I should have been the first in its discovery, he embraced me. My object in determining the musical note was to calculate by a second process a co-efficient to represent the work an ounce of muscle could perform. I also placed my arms horizontally, and held them out in that position until they were completely tired, placing weights upon them to produce this result. In this way I tried to find the force with which the muscular contraction took place by means of the musical note. These vibrations seemed to occur thirty-six times in a second. A second co-efficient of power gained from the above experiments came out to be 20 lbs. to be lifted by an ounce weight of the heart every single minute through one foot. This comes in an extraordinary degree close to the hydrostatic amount. I am therefore entitled to consider that 20 lbs. can be lifted through a foot by every ounce of my heart in a single minute. But this conveys to you no adequate conception of the work done by the heart. I therefore devised a plan of comparison for showing you how much you ought to wonder at the great work of the heart. I obtained from Mr. Main, of Oxford, and Mr. Maclaren, the trainer, the cross-sections of the Oxford eight, and other particulars. The time in which this race has been done is on an average 23 minutes 3½ seconds, and the length of the course 4·31 miles. From these data, and from plans and sections of the boats, I was able to determine the amount of work done by the muscles of these young men. I find that, during the twenty-three minutes that the race lasts, every ounce of muscle in the arms and legs is working at the rate of 20·124 lbs. This comes out very like the work of my heart at this present moment. I am not sure but that my heart is just now doing more. If any of you have seen the exhausted condition of these young men, when lifted out of the boats, you will agree with me that human beings could not endure such exertion for forty minutes; and yet an old man, 100 years of age, has a heart which has worked all these years as hard as the young men of the Oxford and Cambridge races!

We have now discussed—and, I trust, to your satisfaction, solved—the question how much work is done by the heart. But the question is naturally asked—How does the heart do that work? I cannot pretend to tell you how that is done by nervous supply, as that is beyond our present knowledge; but I have succeeded in getting some information from the muscular fibres of the heart. I have applied a strict and rigorous testing of least action to the structure of the heart, so as to ascertain, if possible, some law which must be fulfilled by the arrangement of the fibres allowing this principle to be carried out. The law of muscular contraction which must be complied with is this:—Let the length of the muscular fibre be fixed; the order comes from the brain, or some other nervous centre, for this fibre to contract: *it must be shortened to eight-ninths of its length.* Now, wherever a group of fibres are so arranged, as in the triangular muscle, we saw that each fibre could not contract to this extent. So we must find some principle in the heart by which every fibre can contract to eight-ninths of its length. Borelli compared the arrangement of the heart's fibres to a ball of twine; but it is more like two balls of twine wound up inside a third. The whole heart consists of two ventricles, certain groups of fibre running round one cavity, and certain groups running round another. These are called the proper fibres of the heart. But there is a third group of fibres incasing the whole, and these are called common fibres, because common to both cavities. I have taken my diagrams from Dr. Sibson's book on this subject. These fibres start from the tendinous zone, wind round the heart to the apex, and then run back to the zone internally. The outer fibres enter the heart at the apex, and in their return form the lining and internal parts of the heart's cavities. Imagine millions of these fibres, and you will have an idea of the complexity of the form of the heart. We have three systems of fibres running round these cavities, then, according to extremely beautiful geometrical law. The law is—that *the spiral fibre which goes round the entire two cavities of the heart describes* 180 *degrees before it returns; whereas the others describe an entire circumference and a fifth.* This fifth is to give a twist to the cavity, to prevent the blood, even the least quantity, remaining in the cavities, just as you would give an extra wring to a cloth. I have to thank Dr. Sibson for placing at my disposal his unrivalled preparations of dissected hearts—indeed, he placed all his stores of knowledge at my disposal. As he is not present, and therefore cannot blush at what I say, I may add that his great knowledge of the heart is fully equalled by the kindness with which he places his knowledge at the disposal of the humblest student. Jealousy is so generally a quality of scientific men that it is a great pleasure to find one without it. But I suppose this quality of jealousy in scientific men may be regarded as raising them to the level of the gentler sex!

Each of these fibres is so arranged, then, that it contracts to eight-ninths of its length, and the lengths of each group of fibres are the same. Since this is so, you will easily see that, as far as they are concerned, the principle of least action is fulfilled. But there is a manner of applying a crucial test to the principle of least action here. I have got two groups of fibres—one surrounding two cavities, and another surrounding one cavity only. By the application of geometry I was able to prove a remarkable result. If I call L the length of one of these spiral fibres, the volume of the whole will be proportional to the cube of L—so that if I had two hearts I might calculate the difference of their volume. The difference of the heart before and after contraction is the sum of the volumes of the two cavities, thus—

$$\frac{\lambda + \rho}{\lambda} = \frac{L^3 - L'^3}{l^3 - l'^3}$$

And so, also—

$$\frac{L^3}{l^3} = \frac{\lambda + \rho}{\lambda}$$

where L is the length of the common fibres, l is the length of the proper fibres, and λ and ρ are the volumes of the left and right ventricles.

There are theoretical grounds to lead us to suppose that these two cavities are of equal dimensions. But I have taken the mean of measurements made by ten observers, and find that the mean is 2·125 inches. From theoretical grounds, I believe that more accurate observations will lead to 2. I measured the outer fibres of many hearts of oxen, and found them 10·875 inches. I measured the fibres of the left ventricle, and found they had a mean of 8·625. Well, I suppose there is no one in the room who will be able to tell me the ratio of the cubes. I get it out as 2·004. I believe that to be a most remarkable result, and to entitle us to assert that the principle of least action is capable of solving the difficulties of the heart's action, and bringing us to know one more of the many beneficent laws of the all-wise Creator. How it would have rejoiced the heart of the great Kepler had he known this ratio! Divine Geometry! Queen and Mistress and Judge of all Sciences, thy right to rule them shall never be disputed!

This principle of least action applied to the heart consists of simply making every fibre and part of the heart do the entire amount of work of which it is capable. Engineers have admitted the same thing. If you take a fowling-piece, it is of less consequence how the fibres in it are arranged, as it is quite able to resist the explosion for which it is constructed; therefore, no one thinks of asking how its fibres run. But when you come to the construction of 600 lb. guns —such as the Armstrong—then you must calculate your arrangement of the fibres so as to compel every fibre of steel or wrought iron, as the case may be, to bear its own strain. I went down to see this great gun. It consists of eight rings. The first, sixth, and eighth rings were burst, the other five were whole. But a perfect gun would burst so that all the eight rings would give way together, each perishing with its neighbour. And that problem—which engineers have yet failed to solve—is solved in the heart of every person sitting in this room.—*Medical Times and Gazette, June* 10, 1871, *p.* 653.

18.—ON THE ACTION OF THE HEART.

By Dr. J. Milner Fothergill, late Senior Resident Surgeon the Leeds Public Dispensary.

The heart, as has been shown by Pettigrew, consists of several folds of organic fibre-tubing folded on each other; it is thus capable of distension and rhythmical contraction. For this purpose, a singular system of innervation, motor and co-ordinating, is provided. The real motor power of the heart is under the control of minute ganglia, each with a morsel of muscular fibre under its direction, which are alone capable of carrying on the action of the heart, but only in a tumultuous manner, as seen after section of the vagi. As with all other accumulations of organic fibre, the distension of contents leads at length to contraction in a more or less rhythmical manner. The vagus exercises over this a co-ordinating (Von Bezold), or even an inhibitory action; *i. e.*, the application of a stimulus, as electricity, for instance, to the vagus, retards the cardiac contractions, and if the stimulus be powerful enough, arrests the heart's action in diastole. The vagus then normally acts against the first impressions of distension, and only permits contraction when the distension is sufficient to produce uniform contraction, which then goes on in a truly peristaltic manner, but so swiftly as to be easily mistaken for a simultaneous general contraction. Thus, to some extent, distension and the action of the vagus balance each other; any disturbance of that balance, then, would produce irregularity, no matter in what direction the disturbance might lie. There lies, too, in organic muscular fibre, an inherent power of growth to meet demand; thus, if increased strain be thrown upon the heart, increased growth of muscular tissue, *in health,* takes place, and again the balance is restored between the blood to be *driven* and the *power* to drive it. When, however, from any cause, there is a deficiency in the compensatory nutrition, a species of balance is again struck, but of a lower form, by distension of the fibres, or in other words, dilatation. In fact, there is planted in this comparatively simple cardiac innervation, a more complex one of contraction and reduction of the ventricular cavity on the one hand, and a species of normal distension on the other. These actions are regulated by nerves, which have been dissected out in the rabbit, and experimented on by MM. Cyon, Claude Bernard, and others. The one which calls into action an increased action against obstruction is called the *accelerator nerve;* and the other, which admits a normal distension of the walls, is called the *depressor nerve* of the heart. Claude Bernard has even gone so far as to state it as his opinion that, through the action of the latter, a species of distension may so take place in accommodation to existing circumstances, as to convert the cardiac chambers into temporary blood-reservoirs. This their construction as organic fibre would permit; for organic fibre permits great distension without abolition of function. Thus, while carrying on the circulation, by expelling a quantity only of the contained blood off the top of the ventricle, and permitting a large portion to remain on each systole, this accommodation is allowed without bad results. This is undoubtedly no rare occurrence, if clinical observation were exact enough. Thus, between the ordinary balancing powers of the obstacle of the blood to be driven, and the muscular power to drive it, of the stimulating effects of distension in producing contraction, and the controlling action of the vagus, the heart's action ordinarily rocks; but in addition to that, there is a more complex system of accommodating distension on the one hand, and an accelerating contracting action against an obstacle on the other; which exercise a regulating power according to special circumstances. Ordinarily, however, the action lies between the first set of nerves, with the driving power, and the work to be done. With their disturbances of balance, we are now more especially engaged.

Distension.—The first action of disturbance of this balance is engorgement or distension. When much blood has been located in the ventricles, and they are not capable of completely emptying themselves, a portion remains at each systole. At each diastole, however, an equal quantity of blood is again thrown into the ventricle, and thus at the next systole a larger quantity remains unexpelled; and this process goes on until death, or until some compensatory relief is attained. This relief is usually attained by congestion of the veins, and the system suffering. This distension or, if chronic, dilatation is produced in many ways, thus:—1. Pouring in of the blood under increased pressure, as in the enlargement of the left ventricle, which follows in time on mitral regurgitation, and increased action of the right ventricle and thickened pulmonary vessels; 2. Muscular failure from defective nutrition, as in fevers, in coronary atheroma, or pericardial adhesion; 3. Obstruction to the flow of blood forwards, as in deposit of fibrine on the semilunar valves, diseased vessels, &c.; 4. Disorder of innervation, as a disturbance of the balance between the sympathic ganglia and the action of the vagus; 5. Excessive exertion and consequent cardiac exhaustion; 6. Valvular insufficiency. This condition may pass on to permanent dilatation.

Mode of Repair.—The ordinary modes of repair of this condition are two:—first, relief of the condition on which it depends, where practicable; and, second, hypertrophy, when it is due to increased difficulty in the flow forwards, or to valvular insufficiency, by that power of self-increase which is allowed to all muscular fibre, but with which organic muscular fibre is endowed *par excellence.* Restoration of the balance may take place in three modes, of higher and lower grades:—1. The highest, restoration of the cavity to normal size; 2. Hypertrophy, by increase in number of fibres (Forster) and thickening of existing fibres (Bamberger and Rokitansky)—a compromise; 3. Dilatation, a permanent distension—the lowest restoration of balance, and entailing diminished vital capacity to a point proportioned to the heart's lowered power.

The signs of this disturbance of balance, or partial asthenia, are three: palpitation, irregularity, intermittency.

1. *Palpitation.*—The first evidence of failure of power is palpitation. It is undistinguishable from increased action, except in deficiency of result. When there is excited action, as in exertion or excitement, it is perceptible in the bounding pulse, or, with the sphygmograph, in the increased apex-beat. Palpi-

tation is not so accompanied; and though to auscultation and percussion the heart-stroke may appear identical, it is in the results the real difference lies. Palpitation is a laborious heart-stroke, but not a stronger one. It is the evidence of effort, not of capacity. It is barren in result—a laborious stroke in place of a normal one, but not of more effect, not always of so much. It is intimately associated and commonly mixed up with the next form—irregularity. Palpitation may be engrafted on symptoms of chronic insufficiency as a temporary condition; as, for instance, in the palpitation of Bright's disease, or of slight exertion in a dilated heart from valvular insufficiency. Palpitation is a violent effort of the heart-walls to overcome the action of the vagus, *plus* the obstructed flow.

2. *Irregularity.*—Irregularity of the bulk of blood transmitted into the arteries is one thing, and is due to auriculo-ventricular incompetence. Irregularity in time is another. The latter is under consideration here. Irregularity of rhythm is not due to disordered innervation, but to obvious debility. It is an arrest of the commencement of the peristaltic contraction or heart-stroke. The controlling action of the vagus arrests the contraction until such time as the layers of fibres, acting early on the systole, ought normally to have acted; and then a sharp, almost simultaneous contraction takes place, with an increased thud against the chest-walls. This action is homologous with the increased action of the muscular fibres, under the control of the cardiac ganglia, when the systole has been retarded by stimulation of the vagus. Sometimes it appears as if the action of these layers, acting early in the systole, had been lost; and there is a perceptible change of action in the heart, as if the contraction were commencing from a new layer or set of fibres. Richardson has compared this to a change in order of the number of strikers on an anvil. It always reminds the writer of a horse changing its feet when cantering. This is a more serious evidence of over-taxation of the heart, and is often found mixed up with palpitation. It is often the result of over-exertion, and affords strong suspicion of ventricular distension. Chronic irregularity may have engrafted upon it a passing palpitation from an intercurrent additional disturbance of balance. Nervous irregularity and palpitation will be considered in a future section.

3. *Intermittency.*—This is sometimes purely nervous, and is inexplicable. When persistent, it is usually associated with an impaired first sound, defective apex-beat, and other signs of degeneration. The occurrence of palpitation during this condition sometimes clouds the diagnosis, and makes it very difficult. Intermittency of ventricular contraction is not identical with intermittency of radial pulse, though related to it. In some cases of intermittency of the radial pulse, if the stethoscope be applied while the pulse is held, a ventricular contraction will be distinguished when there is no evidence in the pulse; but it is a feeble contraction. In other cases, the ventricle does not perceptibly contract. Whether there is a very abortive contraction, or not, is doubtful. Certain it is, that in some cases the ventricular contraction can be detected when too feeble to produce a pulse wave; in other cases, no ventricular contraction can be perceived. Intimately connected with this condition, is true angina pectoris; and, finally, cessation in diastole. *British Medical Journal, July* 1, 1871, *p.* 6.

19.—THE EFFECTS OF OVER-WORK AND STRAIN ON THE HEART AND GREAT BLOOD-VESSELS.

By Dr. T. Clifford Allbutt, Physician to the Leeds General Infirmary.

[The idea is pretty general in the profession that heart-diseases are due, as a rule, in young people to rheumatism, and in old people to atheroma; also that aneurism of the aorta is due chiefly, if not exclusively, to constitutional antecedents. A very slight reference indeed is made to bodily work, as a cause of heart-disease, either in the current teaching or the current text-books of the day. On settling amongst a heavily-labouring population, Dr. Allbutt found a large number of cases of heart-disease in young well-made subjects, of healthy build, and previously unaffected by constitutional disease. Speaking of the literature of the subject the writer observes:]

Infinite pains are given to the pathological anatomy of the heart, infinite pains to the description and classification of morbid sounds. Little, however, is given to the treatment of heart-disease, and little to the investigation of causes other than constitutional inflammations and degenerations. Curious cases are sometimes recorded in which a sudden shock has ruptured a valve; these cases are, however, given as curiosities, and are not regarded as extreme instances of an agency always at work in some degree. One recent and deservedly eminent English author says, indeed, that although examples of valve-rupture are published, yet his belief derived from post-mortem examination is, that rupture of a healthy valve never happens. It does not occur to him that the diseased valve he has before him may owe its disease to the same causes as those to which it owes its rupture. I have recently been made aware, however, that a few writers have had the mechanical causation of heart disease forced upon their notice in an exceptional way, and these are the medical officers of our army. Their writings are unfortunately hidden away in special reports, unseen by the profession at large; and I am made acquainted with their contents for the first time by the quotations contained in the excellent little book recently published by Mr. Myers, on Diseases of the Heart among Soldiers (Churchill, 1870). This book came into my hands when the present paper was ready for the printer; and it is a peculiar satisfaction to me to find that he uses almost the same descriptions and arguments in explanation of heart-disease in the army which I used at the meeting of the branch of the Association at Bradford a year before to explain its frequency among forge-men, colliers, wharfingers, and other persons exposed to the heavy strain of town labour. That this experience of Mr. Myers is almost confined to those physicians who practise in the army, is evident from the following passage in Sir W. Jenner's address delivered to us at our Leeds meeting in 1869. His words, which struck me at the time as opposed to my experience, are as follows: "We have attained to this practical conclusion, viz. that regarded in a clinical point of view, structural changes in the valves of the heart are referable to one of three classes—imperfection in development, acute endocarditis, degenerative changes; and yet farther advance of clinical knowledge has shown that non-fatal acute endocarditis is almost limited to acute rheumatism, and that degenerative changes, sufficient in degree to interfere with function, do not occur in the valves of the heart till middle life, and rarely till advancing life."

Such is the opinion of this most distinguished physician; an opinion founded upon a long experience of the diseases of the upper classes of society, and of the diseases occurring among the patients of a hospital away from forges, engineering-works, docks, and other places of heavy labour. My experience in Leeds, and in a hospital placed in the midst of such works, is almost precisely the reverse. The heart-diseases due to senile degeneration are, of course, as numerous or more numerous than elsewhere, and seem to come on sooner; but the diseases due to imperfect development are rare; and those due to acute rheumatism are, among young men, fewer than those which I have learned to attribute to over-exertion of the body.

It is my purpose in the present paper to describe first the kind of cases which appear to me to be due to mechanical injuries; taking these not in the order in which I discovered them—for of course I discovered the worst kinds before detecting their insidious beginnings—but in their natural order; secondly, I shall try to show how the agency of mechanical causes accounts for these cases; finally, I shall support my arguments by the experience of other physicians, to which I am now enabled to add the farther evidence collected by Mr. Myers.

It has been interesting to me to assure myself that as I studied these cases of mechanical heart-disease they fell into a very natural class, having well-marked characters among themselves, and presenting unlikenesses to other forms of heart-disease. That a certain hypothesis explains a definite class of facts, and segregates them into a natural group, is but another way of saying that it is true. We have now to examine the facts, and to see how far their evidence goes.

The order in which the chronic morbid changes seem to present themselves for consideration is as follows: 1st, dilatation of the right heart; 2ndly, dilatation of the left heart; 3rdly, in reason, if not in time, hypertrophy of the left ventricle, or of both ventricles; 4thly, chronic inflammation of the aorta, and aortic valves; 5thly, dilatation of the aorta; 6thly, incompetence of the aortic valves; with, 7thly, farther compensatory hypertrophy of the left ventricle: 8thly, loss of compensatory hypertrophy, with consequent rapid failure, and often with consequent mitral regurgitation.

This order of succession may be complete, or may present these variations among others, that after the fourth stage, the inner coats of the aorta may and often do give way, and dilatation of the aorta is complicated with saccular aneurism, or aortic incompetence may take place as a primary or as an earlier event. In the former case the course of events is somewhat variable; but we often find, sooner or later, the sixth and seventh changes in association with it. Sometimes aneurism precedes incompetence or even simple deterioration of the valves; sometimes it appears at the same time; sometimes, but far less often, it occurs subsequently.

I have been fortunate in keeping a large number of these patients upon my books, of watching them week by week for years, and finally of securing post-mortem examinations of their remains. Autopsies, however, in these as in other cases, illustrate the extreme stages for the most part, and we are left to symptoms, to physical signs, and to reason and analogy, for the understanding of the earlier stages.

With regard to the two first stages, simple dialatation and simple hypertrophy, I have little more than the latter kind of evidence to offer. So constantly, however, do I meet with these states of the heart in patients who have been subjected to over-exertion, that I have no hesitation in placing them together as the beginnings of mischief.

Whether dilatation appear alone, or whether the enlargement consist wholly or in part of hypertrophy, depends apparently on several conditions. If the heart over-taxed be a feeble one, we find dilatation of both chambers; if again the patient, though of strong build, be, through carelessness or poverty, deprived of proper nourishment, we find the same thing: if the lungs be small, we find dilatation of the right heart especially; and this, moreover, we find in cases where the lungs, though adequate in size, are hampered in their movements, as in athletes, or soldiers, tightly clothed about the chest, or in labourers whose occupation consists of work such as lifting of weights and the like, which fixes the walls of the chest and prevents its full expansion. But if, on the other hand, we have to deal with the effects of over-exertion under none of these disadvantageous conditions, if the person be well built and well nourished, if his lungs be of adequate size and have free way to expand in a pure atmosphere, we then may find also enlargement of the heart, probably of both ventricles, but we find hypertrophy in proportion to dilatation.

[Prolonged rest is of the greatest service in all these cases—but such hearts seem always to take some time to right themselves.]

In passing on to valvular sufferings we have to inquire in what relation these stand to the dilatation already described. For my own part, I am satisfied that by sheer stretching of the chambers the auriculo-ventricular valves become incompetent. I disbelieve in Dr. King's "safety-valve" hypothesis: I have watched quantities of overworked hearts, and I believe tricuspid regurgitation is as truly a disease as mitral regurgitation, though more common, more easily established, less audible, and, in its early stages, less dangerous. Clinical experience and examination of the dead alike assure me of a fact which many readers I know will doubt—namely, that mitral incompetence may and does result from over-distension of the left ventricle. On the other hand, I have never seen evidence of tricuspid incompetence in hearts assuredly sound, although I have looked for it in scores of labourers and athletes under exertion. Whenever the venous reflux has been distinctly visible, I have been able to recall it quickly *in the same persons* after long intervals of rest;—showing, so far as this symptom has value, that the right auriculo-ventricular orifice is in such persons permanently weakened. Mitral regurgitation is seen less often, because the left ventricle is less frequently over-taxed and is stronger; but out of several cases before me, in which this state of regurgitation was established in the left heart, let me select the following:

Strain on Drill. Dilatation of heart.—John Rogers, aged 23; shoemaker, of slight build; said to be temperate and to have enjoyed good health. He never had rheumatism in any form, nor does he smoke much. Has been on drill as a militiaman four times. Went up to drill in April 1870, when in good health. He found the work much harder than before; his clothes were tight, and the cross-belt and straps of knapsack were very confining. He felt this the more, as the drill took place in part up and down a steep hill-side. Many of the men complained of being overdone. He managed to keep up during exercise; but on the first and every subsequent occasion, on coming home and removing his knapsack, he was seized with a violent and distressing fit of coughing, which lasted an hour or more. He never had a cough before. When his time was over, he felt very short of breath and oppressed in the chest, which symptoms, before unknown to him, have grown worse. On examination in Oct. 1870, the heart-beat is much diffused, rather rapid and excited. The whole heart is much dilated, its dulness extending nearly an inch beyond the sternum, and its sounds are thin and short. There is a systolic murmur heard at the apex, and less loudly over the xiphoid cartilage.

I was struck on hearing this intelligent man's description of the embarrassment caused by his breast-straps, not only because it bore out my own views of the consequences of deficient pulmonary area or of combustive material upon the right chambers of the heart, but also because it reminded me very strongly of a conversation held about a year ago with my friend Prof. Rolleston, who entered with warm interest into my views about heart-work. He especially urged me to remember the provision made in diving animals such as the whale, which animal does not even wear a breast-bone at all, still less a breast-strap. As in diving, however, there is only need for room to hold the venous blood, and no need for excessive combustion (as in drilling upon a hill-side), the absence of a sternum may be useful in allowing the pressure of the water to squeeze the heart, and so prevent the influx of venous blood. The venous reservoirs in the livers of some seals fulfil the same end in protection of the heart. In John Rogers distention of the heart had resulted in mitral and perhaps in tricuspid regurgitation. Dr. Gairdner has likewise noticed this sequence between the dilatation of the left ventricle and mitral incompetence; and Dr. Bristowe has written in support of the same view in the British and Foreign Medico-Chirurgial Review for 1861, p. 215. Dr. Bristowe, in a paper which is too good to be buried in the back volumes of a magazine, proves, I think, satisfactorily, that the incompetence in these cases is not due to a stretching of the orifice so much as to a deterioration in the muscular and tendinous cords of the valves. This explanation clears up some cases of secondary mitral incompetence which were formerly a puzzle to me, as at the autopsies I had not been able to demonstrate mitral dilatation. I had, indeed, presumed, until I read Dr. Bristowe's paper, that the dilatation was "occasionally" produced during functional activity, and that the orifice might recover itself when the ventricle was no longer distended. Certainly it is true, but I think hitherto not formally recognised, that mitral regurgitation, or at least the murmur of it, is not constant in these secondary cases, but may so completely disappear for hours or days, that when I first studied cases of secondary mitral incompetence following aortic disease, I several times accused myself of error in diagnosis. On my following visit, however, I should perhaps find that the murmur was re-established, and that the inconstancy lay in the facts themselves, rather than in the inferences. Dr. Peacock, in his Croonian Lectures (Churchhill, 1865), also accepts, I think, Dr. Bristowe's arguments, and adds some interesting cases of his own to the same effect. Especially he refers to the great frequency of dilatation of the chambers of the heart in certain Cornish miners, who not only have heavy hammer-work during the day, but whose foolish masters *exhaust them farther* by allowing them no power but their own to raise them from the pit-bottom when their day is done. An hour of ladder-climbing is often required of them in an evening.

In such a way is incompetence of the tricuspid and mitral valves brought about. Less frequently is it due to sudden violence.

It is in the aortic region that over-exertion nearly always tells, primary injury to the other valves being rare. As by natural selection it is generally the strong men who betake themselves to heavy work, so also it generally happens that the labouring heart grows *pari passu* with the dilating stress upon it, or nearly so. Again, puddlers, strikers, and other men who bring powerful frames into the market, are usually well paid, and their abundant diet and large lungs secure both sufficiency of fuel and sufficiency of combustion. But the severer their work, and the more powerful their muscles, the more resistance is there to the flow of blood on the systemic side, and the more tendency is there, not to accumulation of blood in the right ventricle, but in the left heart and aorta. Now the left ventricle can meet this by muscular growth; and I have found in a few autopsies of such men, killed by accident or acute diseases, that the ventricles, the left especially, are, like their bicipites, big and red. Such hearts are quite healthy when seen in an early stage, although the hearts may weigh, as in one instance, as much as sixteen ounces, and may often weigh eleven and twelve ounces.

The case is very different, however, with the aorta: it has no power of strengthening itself according to circumstances. Its resistance is great; but its activity is nothing, or rather it is nothing more than the recoil of elastic fibre. Strain of such coats as these, so far from bringing gain of strength, brings loss of elasticity and weakness; and in the hearts to which I have last referred, healthy as they are, and sound as the vessels may seem yet to be, the aorta is even now beginning to lose elasticity, and to dilate. The inner coats of such aortas, when carefully examined by the microscope, showed in two instances many points of endoarteritis, and a good deal of diffuse granular "exudation" among the fibres of the middle coat. From this point, pouching of the aorta, with consequent or concomitant incompetence of the valves, is quickly established; and such cases form the staple of heart-diseases in the younger male patients of our hospital. The accumulation of blood in the aorta distends the vessel, and the heart, hypertrophied by this weight above it, beats with a violent pulse below; so that the two causes together stretch the vessel, bulging it in a manner familiar to all pathologists, and producing the miliary lesions in its tissues which are the starting-points of endoarteritis. During the earlier stages, the aortic valves remain competent, the patient presenting himself at this period with a heaving heart, the apex of which is displaced downwards and outwards, and with a rough systolic murmur over the sternum. The valves will still be heard to close, and the sphygmograph will delineate hypertrophy, aortic roughening, without obstruction and with valvular competence. Let this patient come again under notice, however, after a while, and suppose that he has pursued his work, in spite of some paroxysmal dyspnœa, and in spite of a peculiar anæmic state combined with nervous irritability and depression, which often are the only symptoms at this stage,—and we shall find that there is now incompetence of the aortic valves; either because the orifice has been stretched with the pouching of the vessel, or because the valves have given way as a flooring. Now we shall find two rough murmurs over the sternum, the second probably a rasping murmur; there will be more decided dulness upon the manubrium sterni, and the aorta may be felt to beat behind the episternal notch.

The coronary arteries differ from all others in this, that they are filled during the diastole of the heart. To feed them well, we should have the systole of an elastic aorta, and competent valves below it. If either of these conditions fail, we may suppose that the coronary arteries are imperfectly fed. Part of the blood which should be shot into them is shot back into the left ventricle; and again, if the elasticity of the aorta be lost, the blood receives little or no impulse either way, and tends, therefore, to roll all back again into the ventricle by the mere force of gravitation. Even when the valves are competent, as in the case of J. M., a widely dilated and inelastic aorta ill serves its function as a coronary heart, and the ventricles, which should be nourished by them, fade away. The life which was possible to a heart whose additional strength could make up for the disadvantages under which it worked, is no longer possible when the strength of that heart is gone. With wasting of the ventricles, we have wasting likewise of the papillary muscles; and, according to the degree of their wasting, we have the constant or inconstant mitral regurgitation which precedes dissolution. Several of these hearts, and especially the three now lying before me, were watched by me during the greater part of their course. In one case, that in which the mouths of the coronary were made up, I had watched the heart for five years, almost without a break. The man, a most respectable heavily-labouring forgeman, came to me year by year, at the dispensary and at the infirmary, for the steel and digitalis which helped him on. At last he came into the house to die. When I first saw him he had dyspnœa, hypertrophy of the left ventricle, and a soft systolic murmur; then came more evidence of a dilated aorta; and then a diastolic aortic murmur; finally, he lost compensation, the sphygmographic up-stroke fell from its inordinate height to less than the normal measure, the impulse of the heart became feebler and more diffused, and the water-hammering of the arteries subsided. Inconstant mitral regurgitation was heard shortly before death, and he was taken into hospital, where he died in a few days. During these few days no arterial shock was perceptible, and sphygmographic tracing showed that the steady regular pulse of aortic disease was broken up by the irregularities and intermissions of mitral regurgitation. The kidneys, in all these cases to which I refer, were of course substantially healthy; though no doubt in most of them the organs were congested and beefy.

I have made no allusion to the effects of beer-drinking in these cases. No doubt forgemen and other labourers do drink large quantities of beer; but I cannot satisfy myself that any such changes as I have described occur in the innumerable young beer-drinkers of lighter occupations who frequent the hospital. It is in accordance with experience and with physiology to suppose that great quantities of beer are burnt off harmlessly during heavy labour. Teetotal labourers eat largely of hydrocarbons. For the same reason, I omit any consideration of the effects of tobacco, which I have carefully eliminated in my consideration of the "irritable" hearts; and of syphilis, which does produce aortic disease; cases of which I shall publish separately hereafter.

The other kind of aortic mischief which results from heavy labour, and perhaps as commonly as the kind already described, is primary aortic regurgitation; not consecutive, that is, to pouching of the aorta. If the aorta be uninjured, a slight incompetence of its valves may long remain unnoticed. When the regurgitation, however, becomes excessive, and the ventricle has to grow greatly, its violent impulse becomes annoying; it begins, also, to set up miliary lesion in the aorta, which was constructed for a ventricle of two-thirds the power; the aorta then pouches, and we fall into the series of events already described. Why, in some cases, the valves go first, and the aorta subsequently; and why, in other cases, the valves take the lead in mischief, it is hard to say. I once strongly believed, and now have some belief, that continuous labour, such as hammer-work, was more injurious to the aorta; and that more sudden strains, like the lifting of weights, told rather upon the valves. Lately, however, I have met with cases which disturb this hypothesis; cases, that is, of valve-mischief caused by continuous over-exertion, and of aorta-mischief, caused by sudden efforts. If, however, one sudden effort be the cause of mischief, we never find pouching of the aorta as a consequence; but we find a crack, which crack may be in the floor of the aorta causing regurgitation, or in the side of it, giving rise to saccular aneurism. Thus it is with single efforts; but, when we come to refine upon labour, endeavour to separate it into two categories of sudden and continuous efforts, we find very often that the distinction is almost impossible.

[Dr. Allbutt then proceeds to apply the like reasoning to his experience of aneurism, on which point he observes:]

I am satisfied that mechanical causes play a great part in the causation of aortic as well as *of other aneurisms*. I should have thought it unnecessary to say, that, in claiming attention for the mechanical origin of aneurisms of the aorta, I do not thereby deny a like effect to constitutional degenerations, were it not that such construction was put upon my remarks at the Association meeting at Bradford. So far from denying the constitutional origin of aneurisms, I had two cases of the kind at that time under my care. They were both thoracic aneurisms: the one was in an old man with general arterial disease; the other was in a young man rotten with syphilis. Both cases were at the time constantly pointed out by me as examples of the constitutional origin of aneurism. But what I did say was this,—that we do not pay sufficient attention to the fact, that aortic aneurisms, both thoracic and abdominal, do so frequently occur in men who are young and of healthy tissues. If these patients attribute their disease to a particular strain, the note-taker is as likely as not to set the story aside as immaterial. I contend, that so far from being immaterial, a sudden strain is here not only a cause, but the commonest cause, of aortic aneurism; for in the majority of instances this cause is alleged, it is reasonable, and it stands alone. The inner coat of the aorta is brittle by nature, and on a sudden distension of the walls of the vessel, it may be a trial of strength between this lining membrane and the same membrane on the valves which shall go first. That on the post-mortem the aorta is found to be "atheromatous," is no ground whatever for the complacent assumption that the "atheroma" is antecedent. On the contrary, the endo-arteritis is as probably a consequence of the same kind as the diffuse mischief surrounding an injury to any other tissue. And as a matter of fact, we find this mischief in abundance immediately about the seat of the lesion; and it dies away gradually until it ceases,'no such mischief probably being visible on any other part of the arterial tree. At the same time, as I often discover big hearts, and thick, opaque, but competent aortic valves in heavy labourers who have died of accident or acute disease, and who were not known to suffer from heart-disease; so, under the same circumstances, among our Leeds forgemen, bargees, and the like, I find patches on the inner face of the aorta: sometimes many and large patches; patches, moreover, with unpleasantly-pitted and thin spots in them, which need but little encouragement to give way. One case of this kind in particular I call to mind, where the patient, a powerful bargee, died of pneumonia in the midst of apparent health. Here, I admit, I do more: I claim local degeneration as an antecedent of saccular aneurism, as I have shown it to be an antecedent of pouched aneurism. But that even this mechanical degeneration is not a necessary antecedent I presume from the occurrence of aneurism from accident in persons not previously devoted to heavy muscular work.—*St. George's Hospital Reports*, 1870, *Vol.* v., *p.* 23.

20.—ON DISTRAIN OF THE HEART.

By Dr. Reginald Thompson, Assistant-Physician to St. George's Hospital.

Dilatation of the heart seems to be so generally considered as a chronic disease, requiring some time before its presence is indicated by constitutional disturbances, which are insidious and gradual, that a few instances in which this form of cardiac injury arose suddenly, and from an accidental cause, may be worth recording, if only to call attention to a train of symptoms which at first are very puzzling, and liable to mislead the observer, and of which I have hitherto been unable to find any account.

The fact that a heart will yield to long-continued and excessive exertion is, indeed, popularly recognised; and recently attention has been directed to the results of athletic exercises, which bear closely upon this question.

The form of accidental injury which results from excessive pressure on the valve-strings has been sufficiently described. But is it not possible for a muscular organ like the heart to yield generally and in all directions from the effect of a sudden and undue strain? Or is it the fact that the fibrous coverings of the heart are in all instances a sufficient protection against such an accident? This, I conceive, is not the case. The pericardium, it is well known, can yield to a very great extent. There is a preparation preserved in this Museum which shows the pericardium so distended with fluid as to extend from side to side of the chest, completely masking the lungs when the body was opened. The fibrous investment of muscle can also yield, and that suddenly, as in cases of purpuric hemorrhage into the substance of muscle, which may take place suddenly to such an amount as to form large tumours. It becomes, then, a question as to the time necessary for the production of such dilatation; and as a very short time is required sometimes for the formation of an aneurism of the vessels, so I think the following cases show that a very short time is necessary for the production of an aneurismal or general dilatation of the heart. It is to this form of injury to the heart that I have given the name "distrain," as indicating a special lesion from accidental injury.

The symptoms in these cases are very interesting, as they are likely to draw observation from the injured part to organs remote from the centre of disease; and the patients appear at first to be suffering from intestinal lesion rather than from disease of the heart. The following cases will illustrate this:

A strong well-built labourer, aged 23, was admitted into hospital on the 13th of March 1868. Six days before admission, after work, he was seized with violent pains below the region of the liver and torminus; but notwithstanding these symptoms, he ate a hearty meal and went to work again. From that time he suffered severe pain, and was brought into hospital in a very precarious condition. He was almost collapsed; cold and clammy; the face blue and congested. He was suffering from orthopnœa: the tongue was dry and furred; he had intense pain in the right hypochondrium below the liver; he was vomiting occasionally. The liver was found to be large; he passed no urine during the time he was in the hospital; the pulse was extremely small and rapid; and the heart-sounds were very hurried and extremely indistinct. I was unable to say if there was any murmur. He had all the aspect of a patient collapsed from intestinal obstruction. At one a.m. on the following morning he was seized with severe cramps in the muscles; and he died at four a.m., after a severe attack of cramps.

At the necropsy the following condition was found. The body was well-nourished and well-developed; the lungs were very much congested and full of black blood; the liver and kidneys were congested; and the stomach and intestines were found in the same condition. The heart was very large, quite uncontracted, and contained a quantity of scarcely-coagulated blood. Both ventricles were very much dilated, and the auricular ventricular openings much enlarged. The mitral valve was found slightly atheromatous. The muscular structure was pale and fatty. The weight of the organ was 18 oz.

Incomplete as is the history of the case just detailed, yet the sudden access of symptoms and the general condition of the patient, interpreted and elucidated by the aid of other subsequent cases, leave very little doubt in my mind that the dilatation of the heart, which was apparently the centre of injury, must have occurred suddenly. This view of the case is also borne out by the fact, that the man had been admitted into hospital in the previous January with a slight attack of rheumatism. I had examined him both on admission and on his leaving the hospital, and was unable to detect any disease of the heart.

In the following cases the history of injury is of a more satisfactory kind:

A man, aged 28, was admitted in December with the following symptoms and history. Two weeks before admission he was in good health; but after lifting some heavy weight he suddenly became very ill; and although he did not faint, he sweated profusely, and was seized with acute pain in the region of the heart and down the left arm. Nothwithstanding this, he went to work; and continned at work up to five days before admission, when he became very much worse, suffering from vomiting and cardiac pain. He had spit blood since the injury.

On admission on the 29th of December, he was in a very dangerous condition, *with a look of much anxiety*; in much pain, especially about the abdomen; constantly vomiting and retching, and suffering from intense thirst. The pulse was very irregular and feeble. His chief distress was the abdominal pain. A loud blowing murmur was heard over the base of the heart from the sternum to the line of the left nipple, and more or less over the whole heart's surface; and this organ was evidently very much dilated. A very marked thrill was perceptible between the third and fourth and the fourth and fifth ribs, from the sternum to the left nipple. The apex-beat was between the sixth and seventh ribs. The liver was very large: the belly distended; and the urine was dark and albuminous; pulse 108. He became semi-comatose on the 30th, and made attempts to scratch his heart, as if from pain. He died on the morning of the 31st.

On examination, the organs were found much congested. The heart was much dilated, the ventricles being very much thinned, and the auricles being even transparent in places; the auriculo-ventricular openings were very much enlarged, the left admitting five fingers, the right six. The structure was soft and fatty. This patient had all the appearance of a man suffering from severe intestinal obstruction. The constant vomiting and retching, and the severe abdominal pain, seemed to point to injuries far from the real centre of disease.

A girl, aged 19, was admitted in February, 1869, with the following symptoms and history. On the morning of the 8th of February she walked rapidly from Fulham to Camberwell, having been previously in health. At half-past one in the afternoon of the same day she was seized with acute symptoms: violent pains in the bowels and back, and frequent vomiting. She did not complain of pain in the region of the heart. She was very ill when admitted: her lips were blanched; her cheeks much flushed; pulse very small and running, 136 in the minute. She suffered from intense thirst, and much abdominal pain and tenderness. Inspection of the chest was sufficient to show that the heart was very much increased in size; its action was perceptible from the third rib; a very distinct thrill was felt on placing the fingers below the second rib. A loud roaring bruit (*du diable*), like the roaring in a shell close to the ear, was heard over a space from the sternum to the external border of the chest. These sounds were also heard to a considerable distance beyond the sternum, to the right side; but they were most distinct at the space first mentioned. A double mitral bruit was heard at the apex. The urine was not albuminous. The condition of the patient was evidently one of much danger; but notwithstanding this, she soon recovered, and went out in a month very much relieved.

She was treated at first with opium, and afterwards with hyoscyamus and belladonna. Under this judicious treatment, and complete rest, the pulse became quiet. On the 16th of February, four days after admission, it was reduced to 120 per minute; and on the 17th still farther to 88. The sphygmograph was applied, and a rapid but regular tracing was obtained. The up-stroke corresponding with the systole was at first very short, but subsequently increased considerably.

In this case the actual damage to the heart was not verified by examination; but the case so closely resembles in symptoms and physical signs those of the two first cases, that I think the same condition of things would have been found.

A few more cases of the same kind have come under my notice, making a total of seven. Of the whole number, three made a good recovery, the constitutional disturbances passing away, although the heart remained permanently injured. Still the organ was able to do its work in such a manner that the patient considered that health was restored.

As regards treatment, perfect rest is the most important remedial agent; and if I may draw conclusions from the very small number of cases which recovered, it has seemed to me that those narcotics which quiet the action of heart, and tend to reduce the number of pulsations—namely, conium, henbane, and belladonna—are more useful than digitalis, which seemed to me to be of little service.—*St. George's Hospital Reports*, 1870, *Vol.* v., *p.* 119.

21.—ON VALVULAR MURMURS.

By Dr. Henry W. Fuller, Physician to St. George's Hospital.

It is not long since the doctrine was universally taught, that a valvular murmur indicates disease or organic change of an irremediable character in the valvular apparatus of the heart. After a time this lesson was considerably modified, and we were led to discriminate between murmurs dependent on organic changes in the valves themselves and those produced by mere functional causes—by an altered condition or impoverishment of the blood, or by irregular contraction of the vessels. The term functional was still confined to certain soft systolic murmurs at the base of the heart, which could be traced more readily along the pulmonary artery than along the track of the aorta. Of late years observation has led to the belief that the range of the term functional must be greatly extended, and may be strictly applied to murmurs at the apex of the heart, as well as to those already referred to at the base. In many instances of chorea, for instance, a temporary systolic mitral murmur is frequently met with; and the constant concurrence of such murmurs with a disorder characterised by irregular and spasmodic muscular action, led me to suggest that their existence is attributable to some irregular or imperfect action of the valvular apparatus—a view, I believe, which is now very generally adopted. But I am satisfied that the term functional, as applied to valvular murmurs, requires still farther extension, and that cases of functional disturbance of the heart's action sometimes occur accompanied by intense murmur, which in the closest manner simulates serious organic valvular mischief.

Many of you will remember a case in point which occurred in the Queen's ward about two years ago. The patient, a young woman, Mary Ann W., æt. 23, was admitted on March 12, 1868, complaining of severe and incessant palpitation, with consequent dyspnœa. The heart's action was turbulent; its impulse was greatly increased; a loud rough systolic murmur was audible at the apex, and a systolic and slight regurgitant murmur at the base. The impulse of the heart was so violent, its action was so turbulent, and the murmurs were so intense, that if much febrile disturbance had existed, it would have been difficult to avoid the conclusion that the symptoms were referable to acute endocarditis. For some days the symptoms continued unabated, and the girl's suffering from palpitation was very great; but under the influence of the tincture of the veratrum viride, the palpitation ultimately subsided, the murmurs ceased, the heart's action became quite tranquil and natural, and she left the hospital. On the 22nd of July, 1868, she was re-admitted under the care of my colleague, Dr. Wadham, not this time suffering from her heart. Indeed, the heart's action was quite normal, and the sounds were free from murmur; so that Dr. Wadham could hardly believe that only a few weeks previously the heart's action had been turbulent, its impulse excessive, and its sounds accompanied by loud systolic and diastolic murmurs. However, before she had been long in the hospital, her former heart-symptoms recurred, and with them the same loud systolic and diastolic murmurs, which, as on the former occasion, entirely disappeared before she left the hospital. In private practice I have met with two similar cases, and I suspect they are more common than is usually supposed. Their true character is often overlooked, and doubtless their diagnosis is somewhat difficult; nevertheless, it may be arrived at with tolerable certainty, if all the circumstances of the case are taken into consideration. It has been thought that the position of the murmur itself might afford a clue to its functional origin, for in some instances the murmur has been louder and more distinct over the ventricle of the heart than it has at its apex. But this sometimes holds good of organic murmurs, and therefore is not to be relied on. More certain evidence is to be derived from the position of the heart, and the general history of the case. Though the action of the heart is so forcible and turbulent, its apex beats in its natural position, there is an absence of any history of former cardiac disease, and there is little or no febrile disturbance. These are the characteristic and diagnostic features of the disorder. If the murmurs were attributable to old-standing valvular mischief, the heart would be enlarged, and its apex would be felt pulsating lower than usual in the chest; if it was due to mischief of only a few weeks' or a few months' standing, the apex-beat might not be greatly lower than natural, but there would probably be a history of antecedent febrile disturbance, with dyspnœa, palpitation, and præcordial pain; whilst if occasioned by existing acute disease, the usual train of febrile symptoms would be strongly marked. It is possible to conceive a combination of circumstances which would render a correct diagnosis almost impossible; but ordinarily the features already referred to would at least suffice to excite suspicion as to the functional nature of the disorder, and should lead to caution in giving an unfavourable diagnosis.

There is another point to which I would refer in relation to valvular murmurs. I allude to the pathological significance of murmurs undoubtedly of organic origin. Briefly, I would have you clearly understand that it is simply impossible from one examination to judge of the pathological significance of an existing murmur. The importance of any murmur is strictly proportioned to the amount of obstruction which the mischief in which it originates offers to the onward current of the blood, and of this no single examination will enable you to judge. The turbulence or irregularity of the heart's action, the force of its impulse, the loudness, harshness, or roughness of any murmur, and its position, whether at the base or apex of the heart, do not, even when viewed together, afford a certain criterion as to the existence of organic valvular disease, and still less do they justify any positive diagnosis as to the extent of that mischief, or any prognosis as to the rapidity with which it is likely to run its course. These are points which can only be judged of by repeated examinations conducted at long intervals. A minute bead of fibrin may be so placed at the edge or on the ventricular surface of the valve, as to give rise to a sharp eddy productive of a loud, rough, and persistent murmur, and yet may not offer any serious impediment to the onward flow of blood; whereas in another instance the valvular apparatus may be damaged in such a way as to cause very little eddy in the current of the blood, and consequently to produce very slight murmur, and yet may seriously obstruct the onward flow of blood, and thus may lead rapidly to hypertrophy and dilatation. Be careful, then, not to be over-hasty in forming a conclusion as to the significance even of organic valvular murmur, and be still more so in expressing any prognosis in the case. Content yourselves by stating that organic valvular mischief exists, but that its real practical significance can only be ascertained by repeated examinations conducted at long intervals. If at the expiration of a twelvemonth from the date of the first examination, you find the heart's action quiet and regular, and the apex-beat in its normal position, depend on it there is no serious impediment to the circulation, however loud and rough the murmur, and your prognosis need not be very unfavourable. This would hold good even more fully if the parts remained in the same condition after the lapse of another six or twelve months. If, on the other hand, the heart were to show signs of enlargement, if its impulse had become more forcible, and its apex-beat lower, your prognosis, however slight and soft and apparently unimportant the murmur, could not be otherwise than unfavourable. Several of the former class of cases have come under my observation, in which a loud murmur has existed for periods varying from ten to twenty years, and in which even now very slight hypertrophy or dilatation of the heart has occurred.

There is yet another point in relation to valvular murmur to which I would beg your earnest attention. I mean, the degree to which the lesions on which these murmurs depend will sometimes admit of repair, when the patient is placed under favourable circumstances. I press this upon you the more earnestly, because, from the very nature of the case, you are unable here in hospital practice to observe the fact for yourselves, and you must take it therefore on my authority. Reparative action, in these forms of disease, takes place very slowly, and hospital patients do not remain long enough under inspection to enable you to trace its various steps. But it does take place to a remarkable degree—to a degree far greater than is commonly supposed. Let me give you one example. *In the year 1865 I was* consulted about a young man, æt. 17, who had suffered from rheumatic fever at the age of 15, and again a twelvemonth afterwards; on which last occasion he was reported to have had inflammation of the heart. When first brought to me, he was suffering severely from palpitation, with a heaving impulse of the heart visible through his clothes, and an extremely loud and rough systolic mitral murmur. His father was aware of his son's condition; for his medical attendant had told him that the heart was irretrievably damaged, and that dropsy was the inevitable result. I so far confirmed this gentleman's opinion as to tell the father that the mitral valve was damaged, and that unless the mischief were to subside under treatment, the case would probably terminate fatally at no distant date. But having said thus much, I told him that as the apex of the heart was beating almost in its natural position, there were fair grounds for hoping that the mischief was not so serious as the physical signs appeared to indicate, and that treatment might be productive of real benefit; and I instanced several cases of partial or complete repair which I had met with in practice. Farther, I entreated the father not to regard the case as hopeless, but to do all that was necessary to give his son a chance of recovery. I explained that complete rest, with freedom from excitement, for two or three years, was absolutely essential, as was also a long-continued course of iron, together with cardiac sedatives; and that throughout the period of treatment the utmost care would be needed to regulate the secretions, and avoid a recurrence of rheumatism. My injunctions were faithfully carried out both by father and son, and my patient was brought to me every three months, that I might caution or encourage him, if encouragement or warning were necessary, and that I might also note the progress of events. Before the end of a twelvemonth considerable improvement had taken place; and when he came to me last March for a certificate of health, with a view to a public appointment, the heart's action was so quiet and regular, the apex beat so nearly in its normal position, and the murmur so faintly audible, that I verily believe the disease would have escaped detection except under the closest examination.

Let me beg of you, then, not to be over-hasty in hazarding a prognosis when you are called to a patient suffering from valvular disease of the heart. Several examinations, conducted at considerable intervals, will often be needed to determine with certainty whether a murmur is organic or functional; and even when a decision on this point has been arrived at, the true bearing of the murmur, and the practical importance of the lesion which the murmur denotes—the degree to which it interferes with the circulation, and the rapidity, therefore, with which it will induce hypertrophy and dilatation, and lead to dropsy, dyspnœa, and death—are only to be ascertained even approximately by carefully noting the condition of the heart as regards its sounds, its impulse, and the position of its apex-beat, at several examinations conducted at intervals of three or four months. If, after some months of observation, the impulse of the heart becomes more forcible, and the apex-beat lower in the chest, the prognosis ought to be more unfavourable than the mere character of the murmur may have seemed at first to warrant; whereas if an opposite tendency is observed—if at each successive examination the turbulence of the heart's action and the force of its impulse are found to be lessening, and the loudness and roughness of the murmur to be diminishing; and if, farther, the apex continues to pulsate almost in its natural position—the opinion may be given that the obstruction to the circulation is not great, and probably will not tend, rapidly at least, to a fatal issue; nay more, that if due caution be taken, repair may possibly be effected to a very great extent, and that the patient may live through a long series of years in the enjoyment of very tolerable health.—*St. George's Hospital Reports*, 1870, *Vol.* v. *p.* 1.

22.—SUBCUTANEOUS INJECTION OF ERGOTINE IN HEMOPTYSIS.

By Dr. William Allan Jamieson, Berwick-on-Tweed.

In the July number of the *Edinburgh Medical Journal* for last year, Dr. George W. Balfour recommended the hypodermic injection of ergotine in hemorrhages *of various kinds;* and I determined to try it in the next case of hemoptysis which occurred in my practice. The result in the following is, to say the least, remarkable.

A. B., aged 41, a tobacco-pipe maker, a fresh-looking man, and not intemperate, fifteen years ago, about the New-year, fell asleep, when rather tipsy, out of doors, one wet night. He caught cold then, and had a cough ever since, which had not, however, interfered with his pursuit of his trade. On November 20th, 1870, he had to walk quickly up a steep hill, and then stood, when overheated, exposed to more or less of a draught. On the morning of the 21st, while at work, and not using any unusual exertion, he brought up a mouthful of florid blood. This recurred at intervals during the day, and in the evening he called for me. I did not then strip him for examination, as he was somewhat overheated, but at once injected five grains of ergotine (procured from Messrs. T. and H. Smith, of Edinburgh), dissolved in ten minims of distilled water, into the cellular tissue of the arm.

On November 22nd, no more florid blood had come up after the injection, only one or two dark coagula. At the base of the left lung, both anteriorly and posteriorly, there was fine crepitation audible at the close of inspiration, but no dulness on percussion. He felt well, and returned to work, an aperient pill being given to avoid constipation. On the 23rd there was slight return of the hemoptysis; and on the 24th it was again considerable. I therefore repeated the injection of ergotine, enjoining strict rest in bed. The result of the injection was the same; the expectoration of florid blood immediately ceased, only a dark clot or two coming away. Rest was continued till December 1st, when he resumed his employment, quite well, with the exception of the moist sounds at the left base.

He continued well during the severe winter of 1870–71, taking, from time to time, a little cod-liver oil, until May 7th, 1871, when he came to me in great trepidation, the hemoptysis having recurred. While in my house he spat up some bright blood; and I therefore had recourse again to the ergotine. The expectoration of bright blood at once ceased, and a tickling cough which preceded it disappeared also. Next day I found him well, only one or two brownish clots having come up. The crepitation was still audible, though less clearly, at the left base; and though there was no appreciable dulness, there was slight flattening of the chest-wall. I ought to have mentioned that I examined the expectoration in November, in the manner recommended by Dr. Fenwick, without finding any fragments of elastic tissue.

Should the result of this case be borne out by further experience, we shall have to thank Dr. Balfour for introducing a remedial agent sadly wanted, an efficient and rapidly-acting styptic in hemoptysis. No local effect, save slight irritation of the skin, lasting a few hours; and no influence on the pulse, which remained at 72, resulted from the injection.—*British Medical Journal, June* 3, 1871, *p.* 587.

DISEASES OF THE URINARY ORGANS.

23.—ON DIABETES.

By Dr. W. R. Basham, Physician to the Westminster Hospital.

Though physiology has obtained considerable insight into the source and formation of sugar or glucose in the organism, yet the pathology of diabetes is still far from settled. No final or undeniable explanation has been offered which satisfactorily accounts for the large amount of sugar daily formed and passing through the kidneys in a case of diabetes. It is not the error or disorder of a single organ or of any one function which will explain this deviation or exaggeration from the processes of health. The vice or error is probably in many organs and many functions. It may be instructive to draw your attention to the leading hypotheses which seek to account for the excessive amount of sugar formed.

In studying the symptoms of diabetes you must not overlook the physiological fact that sugar or glucose is in small proportion a natural production in the organism; and that it exists not only in the liver in health, but that it is formed out of every article of farinaceous and amylaceous food we eat, and that a small quantity exists even in our nitrogenous food. So that in health sugar is always being formed; but it is only in a transitional state; passing out from the liver by the hepatic veins, it enters the respiratory circulation, where it is rapidly and completely oxidised, and converted into carbonic acid and water; and thus adding to the sources of vital heat, it disappears from the organism almost as soon as formed.

As far as our present knowledge enables us to judge, diabetes is an error of function rather than an organic disease. The organic diseases with which it is occasionally associated are accidental complications rather than essentials to its existence. Tubercle, and cataract, and carbuncle, are far from being present in every case, although not unfrequently accompanying it.

The excess of sugar passing through the kidneys in diabetes may be accounted for:—1st, either by supposing an exaggeration of the gluco-genetic function of the liver; or 2ndly, by a defect in the gastric digestion by which the farina of the food, having been converted into glucose by the salivary and buccal and pancreatic secretions, suffers no further change in the ultimate processes of assimilation, but is excreted with the urine as sugar. These views lead up naturally to the supposition of an error in the respiratory function by which the glucose circulating through the lungs is not further oxidised and converted into carbonic acid and water. But it may be that the sugar is formed in such abundance, or in such excess, that the capacity of the respiratory apparatus is not equal to the task of the conversion of so large a supply as is cast upon it.

There are five predominant theories which endeavour to trace or account for the excess of sugar formed in diabetes.

First.—Bouchardat's theory. The amylaceous or starch elements of our food are converted into sugar by several animal products. The saliva, the pancreatic secretion, the gastric fluid rendered alkaline (having lost its property of dissolving flesh), continues capable of converting starch into glucose. He considers the sugar of diabetes to be principally, if not entirely, formed from the farinaceous series of foods. He thinks diabetic persons digest these articles of food differently from healthy persons.

Secondly.—Claude Bernard's discovery of the sugar-forming function of the liver has led to the opinion that diabetes consists in an over-activity of this function, and there are many who, following these views, consider the liver as the chief, if not the only, organ at fault. But this over-activity of the gluco-genetic function of the liver is supposed to arise from some undue excitation of the great sympathetic system of nerves.

Thirdly.—A general and irritable state of the nervous system being a frequent, if not a constant, symptom in diabetes, and as Claude Bernard records that irritation of the ganglionic centre in the medulla oblongata, exercises a special influence over the functions and actions of the liver, and, moreover, as Flourens has shown, that irritation of the floor of the fourth ventricle led to the presence of sugar in the urine of the animal operated on, it has been conceived that the nervous centres are remotely the cause of this singular disturbance in the functions of the organism.

Fourthly.—It has been held by some that a defective respiratory process, by not consuming the elements of sugar in the lungs, might account for the accumulation of the sugar in the blood and its presence in the urine. M. Reynoso affirms that in almost all cases in which the function of respiration is impeded, sugar appears in the urine. Patients under the influence of ether or chloroform, in the advanced stage of tubercle, in pleurisy, in asthma, hysteria, and epilepsy, in experiments on animals prevented from breathing freely, sugar was universally present in the urine.

Fifthly.—The importance, the absolute necessity for the presence of certain alkaline salts in the blood, without which the further metamorphosis of many excrementitious substances would be incomplete, is well known.

> M. Mialhe has proved, by the analysis of the blood in diabetic patients, that it is deficient in these alkaline salts; he states that the farina of our food is converted with equal activity into sugar both in health and in diabetes. But in health the sugar is decomposed or burnt off by the presence of these alkaline salts by the agency of the oxygen in the lungs; but in diabetes the decomposition does not take place, owing to the deficiency of these important constituents, and the sugar undergoing no further metamorphosis, passes off as such through the kidneys.

Such are the prevailing theories or doctrines explaining the cause or tracing the source of the sugar in diabetes.

It is absolutely necessary that you should be familiar with these doctrines, for without such knowledge the treatment of diabetes will be incomprehensible to you. The several plans of treatment are based on one or other of these views; and the object for which, or the therapeutic value of any remedy employed, must be referred to one or other of the theories just enumerated.

It is practically of importance in the management of a case of diabetes to ascertain what is the relative amount of sugar derived from the food, and what formed irrespective of the ingesta. You should know that the amount of sugar is always augmented after a meal. The sugar then should be estimated by an analysis of the urine passed from one to two hours after the principal meal; and also by an analysis of the morning urine, or that passed after fasting from eight to ten hours. From the first may be approximately estimated the sugar derived directly from the food; from the second analysis, the amount of sugar will represent the exaggeration of the liver function. The practical deduction from which will be, that diet, or the abstinence from farinaceous foods, will, in a great degree, moderate the proportion of sugar after a meal, but will exercise a very limited influence over the amount representing the sugar derived from the liver, to control or moderate which will require the use of medicinal agents, the action of which will be explained hereafter.

The case which I select to illustrate these general remarks on the disease is that of a woman, S. D., 64 years of age, the mother of eight children, six of whom, however, were still-born. Two are grown up and living. She stated that her general health, up to within the last twelvemonth, had always been good. The first indications of failing health appear to have been a general physical debility or incapacity for bodily exertion, associated with a very rapid loss of flesh, intense thirst, and a desire for food, which no quantity seemed to satisfy. On her admission the following symptoms were most worthy of note:—There was a great amount of emaciation, the skin being everywhere harsh and dry, and puckered; not a trace of subcutaneous fat could anywhere be felt. The tongue was moist. The bowels were very sluggish, the dejections being hard, dry, and pill-like. The breath was aromatic. The pulse was small and frequent, with occasional intermissions. The perceptions of sense were typical. The sight was occasionally obscured by musci volitantes; there were no indications of cataract. The sense of smell was not affected. That of taste was, however, disordered by the sense of constant sweetness, which provoked thirst, and tainted everything she ate. The cutaneous sensibility was the seat of irritation about the vulva and meatus urinæ. The chest was free from any symptom of tubercular disease. The urine was passed in great abundance, from fourteen to sixteen pints daily. It was nearly colourless, but, notwithstanding, had a specific gravity of 1·034. The copper test gave marked evidence of sugar, and the fermentation test proved that it contained nearly thirty grains of sugar to the ounce of urine. She was at once placed on the diabetic diet, excluding, as far as possible, farinaceous articles, and supplying vegetables from the cruciferous family, supplemented with a large amount of animal food.

The medicinal treatment should in the commencement be governed a good deal by the complications which may attend the case. If there be much irritability and restlessness in the nervous system the treatment by opium in large and repeated doses should be at once commenced. If pulmonary symptoms give ground for the suspicion of tubercle, cod-liver oil and ferruginous preparations should be given. In the case under consideration the symptoms of chief note were those derived from the abundant formation of sugar—the emaciation, the thirst, and the craving appetite, the essentials to the diabetic state. It was a case which seemed to promise benefit from what is sometimes called the alkaline plan of treatment, but which might be more correctly designated as the ammonio-saline system. It is based on the views already explained of the existence of a deficiency of those salts in the blood. The saline mixture is composed of carbonate of ammonia, ten grains; phosphate of ammonia, ten grains; the bicarbonate of soda, ten grains. These salts, dissolved in an ounce of water with a few drops of the tincture of ginger, are given three times a day, in a state of effervescence, with a table-spoonful or more of fresh lemon juice. Such a medicine is very grateful to the patient; it relieves thirst, and usually, in a short time, mitigates the morbid appetite. The clinical report states, ten days after this treatment had been continued, that the thirst and appetite were much diminished. The tongue was moist. The glutinous sticky feeling of the mouth was gone. She stated she felt more energy in her; less of that physical feebleness which she formerly suffered. By the end of the third week the urine was reduced in quantity, about eight pints in the twenty-four hours.

The remedy was continued for a further period, and at the end of the sixth week, the amount of sugar per ounce was reduced to six grains. The daily amount of urine was estimated as from six to eight pints. The tongue was moist, the thirst by no means urgent. The dejections were no longer hard and pellet-like, but soft and pultaceous, and, moreover the patient had more than once felt some moisture on the skin, which had not occurred for many months. The report to the middle of March continued very favourable; in the third week of that month the thirst and morbid appetite was stated to be much diminished; the urine had decreased to no more than four pints in the twenty-four hours, but the amount of sugar remained the same. I think you may trace all these favourable symptoms to the agency of the ammoniacal saline which has been taken without intermission from her admission into the hospital. It is true the disease has not been cured, but the disorder has been brought within such limits that the more distressing symptoms have disappeared. Before she leaves the hospital the remedy she has taken so long will be discontinued, and after a few days interval, the urine well examined for quantity of sugar present, and observations made to ascertain if the effects of the treatment have been permanent or temporary.

[Dr. Basham determined to test the effect of the ammonio-saline treatment of diabetes by discontinuing it for a few days in this case. No test could be fairer.]

She discontinued the medicines on the 5th April; the diet was strictly continued. On the fourth day of the interval she complained of missing the medicine. She stated that the clammy, thirsty feeling in the mouth had returned, that her food did not satisfy her as when taking the saline. The amount of urine was daily increasing; its specific gravity had become 1·038, and the amount of sugar proportionately augmented. There was an evident return of all the leading conditions of the diabetic state on the discontinuance of the ammonio-saline. After an interval of ten days, to be certain that this recurrence of the more prominent and characteristic symptoms was not accidental, the carbonate and phosphate of ammonia were resumed in the form described in the last lecture. If, again, a mitigation of symptoms followed, if the urine and sugar decreased and the thirst became modified a few days after the operation of the remedies, it could not be reasonably asserted that these agents were inactive, or of no therapeutic value. Five days after the resumption of the medicine, the specific gravity of the urine had gone down to 1·027, and the proportion of sugar to the ounce had decreased to ten grains. Other symptoms had again disappeared; there was little or no thirst, the tongue was not clammy, the breath was inodorous, and the appetite nothing inordinate. You must recollect, however, that it is by no means an unusual occurrence in diabetes in the later periods of life, for the symptoms to oscillate between great extremes, irrespective of whatever remedy may be employed, so that in the view of some this decline of the more distressing conditions might be considered as the natural law of the disease, apart from any therapeutic agency. It is doubtless true that in what has been termed senile diabetes these variations are frequent. But it is not a little singular that the abatement of the symptoms always correspond in this case to the operation of the salines, and the aggravation of them to the interval in which they were discontinued. The same results have so constantly been observed by me in the treatment of diabetes in another class of practice, that I have no hesitation in placing the greatest reliance for the mitigation of the more distressing symptoms, particularly that of thirst, to the beneficial action of the ammonio-saline treatment. It must be admitted that there are cases of diabetes in the treatment of which all that can be hoped for is mitigation, not cure. Reference to the preliminary remarks I made in a former lecture will prove that it is not difficult to diagnose such cases, to determine the fact at the commencement of the treatment how far it will probably be curative, how far only alleviative. This can and ought in every case to be determined by an analysis of the urine, collected at definite periods of the day, having reference to the time of taking food. If the morning urine, passed after an interval of some eight or ten hours' fasting, contains but little sugar, perhaps only a trace; if the urine passed some hour or two after breakfast or dinner, no rigid abstention of diet having been observed, present evidence of a well-marked notable amount of sugar, the inference should be that the sugar was derived from the farinaceous elements of food, that it was therefore an error of digestion in some of the primary stages, rather than implicating such organs as the liver, lungs, or nervous system as its causes. On the other hand, where the evidence of sugar in the morning urine (*urina sanguinis*) is as great as that of the urine of digestion (*urina cibi*), the disease has a more remote origin, the digestive function is in error, but so is probably that of liver, lungs, or ganglionic system. In the first type of cases the more distressing symptoms of diabetes—unsatisfied appetite, emaciation, restlessness, are absent, or only present faintly, and not distressingly; in the second type of case all of these, with others, are present in aggravated form. Diet and medicinal treatment will cure the first, but diet, however strictly regulated, or remedies however appropriate, will do no more than alleviate the second, for under no circumstances is the disease in this form otherwise than incurable. You will easily perceive, therefore, how desirable, not on scientific grounds only, but on purely practical grounds, how imperatively necessary it must be in each case to determine to which of these two types of the disease the case under your treatment may belong. The mitigation of the more urgent symptoms on each occasion, a few days after resuming the remedies, left no reasonable doubt of the therapeutic influence of the ammonio-saline plan of treatment. Under these agents the patient is increasing in weight, is gaining week by week an increase of bodily vigour, and losing all the prominent and more distressing symptoms of the diabetic state. You will find it convenient in practice in private life to keep, in a tabular form, a list of the articles of diet, which, in the dietetic treatment of diabetes may be allowed, and those which are forbidden; a list also of the wines and beverages which may be taken or avoided is also useful. The following list fulfils these objects:—

DIET TABLE IN DIABETES.

General rules to be observed by those suffering from Diabetes, who should earnestly recollect that an accurately observed diet is more effective in relieving the disease than any other known means.

VEGETABLE FOOD.

Articles of Food Forbidden.	*Articles of Food Allowed.*
Every form of vegetable food containing Flour (Farina), Starch, Dextrine, or Sugar.	All the Cabbage Tribe (*Cruciferæ*). Cabbage, Cauliflower
All articles made from Wheat Flour—	Broccoli, Borecole
Bread, Biscuits, Pastry	Scotch Kale, Brussels Sprouts
Oatmeal	Sea Kale, Cardoons

Peas, Beans, Haricots, Lentils
Rice, Sago, Tapioca, Arrow-root
Semolina, Revalenta
Potatoes, Yams
Carrots, Turnips, Radishes
Parsnip
Macaroni, except with Cheese
Vermicelli
Cocoa, Chocolate
All Fruits rich in Sugar

Couve Tronchuda (Portugal Cabbage)
Spinach, French Beans
Artichokes
Cucumber, with Oil
Asparagus
Onions, Leeks
Mushrooms, Truffles
Lettuce, Endive, and all varieties of Cress, Italian or Corn Salad, American, and Water Cress
All kinds of Acid Fruits
As a substitute for Wheaten Bread—
- Gluten Bread
- Bran Bread
- Bran Biscuits
- Brown Bread, cut thin and toasted, may be allowed

ANIMAL FOOD.

Articles of Food Forbidden.

None.

No article of food derived from the Animal Kingdom is forbidden in Diabetes.

In every kind of made dish prepared from Animal food, the sauce or gravy, if needed, must be thickened with either Gluten Flour or Egg Powder.

Articles of Food Allowed.

The Food of the Diabetic Patient should be as much as possible selected from the Animal Kingdom.
All kinds of Meat
All kinds of Game
All kinds of Poultry
All kinds of Fish and Crustaceæ
Eggs of every variety that are edible
Cheese in every form and variety
The Curds of Milk.

DRINK.

Forbidden.

All varieties of Beer—
- Ale, Stout, Porter
- Sweet Cyder

All Home-made or Sweet Wines
All Wines containing a per centage of Sugar or Dextrine—
- Port,
- Sherry, Brown or Golden
- Madeira

Champagne, if sweet
Sparkling Moselle
Sparkling Hock
Sparkling Burgundy

Allowed.

All the Bordeaux Wines
- St. Julien
- Medoc
- Lafitte
- Sauterne, &c.

All the Rhine Wines
Every variety of Hock
- Marcobrunner
- Rudesheimer
- Steinberger

Red Assmannshauser
Spirits—Brandy (Cognac)
- Whiskey, Gin unsweetened
- Rum, Hollands
  - Tea, Coffee, without Sugar, but with Cream *ad libitum*

Soda, Seltzer, Vichy, and similar Waters.

The whole surface of the body should be clothed with flannel winter and summer.—*Medical Press and Circular, May* 3 *and June* 21, 1871, *pp.* 373, 523.

24.—ON THE RELATIVE INFLUENCE OF BREAD, HONEY, AND SUGAR UPON THE AMOUNT OF UREA AND SUGAR EXCRETED IN DIABETES.

By Dr. W. **Wadham**, Physician to St. George's Hospital.

[The following observations upon the amount of sugar and urea excreted by diabetic patients during the consumption of regulated quantities of bread, honey, and white sugar, were made upon three patients who were under Dr. Wadham's care in the hospital, in the year 1870. The facts which appear to have been proved by the first experiment, made upon the urine of a man 50 years of age, were]

1. That, irrespective of any change of diet, the amount of urine, urea, and sugar excreted in diabetes varies very greatly from day to day.

2. That the excess in either of these constituents does not appear to be accompanied by a decrease in the other.

3. That the addition of honey to the diet causes an immediate rise in the quantity of urine, urea, and sugar excreted, the rise in all these becoming greater as the honey is continued.

4. That only about half of the sugar given in this form appears to be eliminated by the kidneys in the form of sugar, the remainder being probably burnt off in the lungs or assimilated to the system.

5. That whatever truth there may be in the asserted benefit derived from the dietetic use of honey in diabetes, in this case it certainly did not act beneficially by diminishing the amount of urea; for not only was the amount of urine, urea, and sugar greatly increased during its consumption, but after it was omitted all these remained higher than they had previously been, the increase being especially noticeable in the amount of urea.

[The second experiment was made upon an unmarried woman 26 years of age who was known to have been suffering from diabetes during the eight preceding months.]

The effect of administering 42 grammes of starch was apparently to convert a case which was daily improving into one which, judging by the amount of sugar and urea excreted, became rapidly and persistently worse. The facts which these experiments seem to prove are the following:

1. That, as was shown by the former cases, the amount of urine, urea, and sugar varies greatly from day to day in diabetes, irrespective of any alteration of diet; and that the excess in either the sugar or urea is not compensated by a decrease in the other.

2. That, contrary to that which happened in the former case, a large quantity of honey given during two consecutive days was followed at first by a considerable reduction in the amount of urea and sugar excreted; but that as the honey was continued, an increase in both these constituents took place, and was almost equally evident in the urine collected during the twenty-four hours immediately following the omission of the honey from the diet. The absolute amount of sugar and urea passed during these three days, however, very slightly exceeded that passed during the three days previous to the honey being given.

3. That twenty-four hours after this consumption of honey had ceased, the amount of sugar excreted began to diminish and gradually become less; and although during the first day some diminution occurred in the amount of urea excreted, this was not permanent, as for two days after the honey was omitted it increased enormously—apparently from some transitory effect produced upon the system. The gradual diminution which afterwards took place in both the amount of urea and sugar seemed to show that this experiment had done no permanent harm.

4. That the effect of a small addition of bread to the diet caused an immediate and considerable increase in the amount of sugar excreted; but that the injurious effect of the bread was not fully established until the second day of its administration, after which it seemed to produce a steady increase in the amount of sugar.

5. That the additional amount of sugar excreted when the bread was taken

was greatly in excess of that which could have been formed by the mere chemical conversion of the starch contained in the bread into sugar; and was therefore probably the result of some injurious effect produced by the bread upon the system.

6. That the administration of bread caused an immediate increase in the amount of urea excreted, but that this increase was not, like that of the sugar, progressive, or afterwards very marked. Finally, honey appeared to produce a favourable effect upon the progress of this woman's disease, bread a decidedly injurious one.

[The third series of determinations were made upon the urine of a man 25 years of age, who had been the subject of diabetes four months.]

The principal additional fact which the experiments in this case seemed to prove was, that the increased excretion of urea following the addition of bread to the diet may be delayed for twenty-four hours; and that when this is omitted from the diet, a similar delay may occur before any diminution takes place in the amount of urea excreted.

The fourth series of experiments were made upon Thomas Jones after his return from Wimbledon. Whilst in the Convalescent Hospital there, he was placed under no treatment, was allowed the ordinary diet supplied to the inmates of that institution, and took exercise freely in the open air. The results of this were, an apparent improvement of his health, a slight increase in his weight, but a decided exaggeration of sugar excretion. So much was this the case, that on the first day after his readmission the amount of sugar passed amounted to 334·55 grammes in the twenty-four hours, or to nearly three times the quantity he was passing when he left the Hospital five weeks previously.

For three days after his readmission he was allowed twelve ounces of bread daily, but no other amylaceous food, no saccharine matter, and no beer. Even under this diet the amount of sugar was rapidly diminishing, when for the next three days he was entirely deprived of all amylaceous food, and then made the subject of experiment. During this his second residence in the Hospital he received no treatment beyond being strictly dieted in the intervals of the experiments. He, however, improved very much in health, and increased steadily in weight, during the whole time, gaining about one pound each week. The time he complained of feeling most unwell was when for a few days he was taking bread; and although after this the amount of sugar in the urine never again fell so low as it had previously been, the average amount of it for several days before he finally left the Hospital was about one-half of that he was passing when previously discharged, and less than a fifth the amount he passed the day following his return from Wimbledon.

The experiments were three in number, and were made with a view of ascertaining the relative influence of equal weights of honey, bread, and white sugar upon the amount of urea and sugar excreted, and the amount of each of these materials given was eight ounces. It was originally intended to give each of them for a week in succession, and to allow a week to elapse between each experiment, in the hope that by so doing the urine during this interval would return to its previous standard, and lose all trace of the disturbance to its composition caused by the preceding experiment. This plan, however, was not strictly adhered to, and the bread was only continued for four days. The man almost immediately he took it complained of the distressing thirst it caused him; and on this account and because it inconveniently increased the amount of urine he was passing, he, unlike diabetics in general, was very unwilling to take it. It was therefore thought advisable not to continue this experiment; partly from a conviction that the bread was really doing him harm, and also from a fear that he might employ deceit and not eat it, though professing to do so, and thus destroy the value of the experiment.

[From a fourth series of experiments the conclusions were:]

The first fact worthy of notice is that scarcely any diminution of sugar-excretion occurred during the first twenty-four hours following the total withdrawal of all amylaceous and saccharine materials from the diet, and that very little increase of it occurred from the addition of honey to it, either at once or when this was continued, *as it was, for* some days. Similar circumstances had been noticed during the experiments formerly made upon this man; for both, when additional bread was given him, and when this was afterwards withdrawn, no change in the amount of excreted sugar took place during the first twenty-four hours. If we calculate the daily average of sugar passed by this man during the week he was taking for two days 142, and for five days 284 grammes of honey, we shall find that it varied so little from that which was afterwards excreted when the honey was no longer taken, that we can only conclude that this latter had in this case little or no influence over the excretion of sugar, the amounts being respectively 195·61 and 184·01 grammes.

Even the addition of 284 grammes of white sugar, which was continued for the next five days, appears to have had but little influence in increasing the amount of sugar, though it slightly diminished the amount of urea excreted, the average increase of sugar being only 45·47 grammes, and the diminution of urea 2·58 grammes. Thus, in a case of confirmed diabetes, in so advanced a stage that the man died within a month of the experiment, we have the somewhat extraordinary fact, that the addition for five days of 284 grammes of pure white sugar to the diet only caused an increase of 45·49 grammes in the amount of sugar excreted with the urine.

After the white sugar was withdrawn from this man's diet, the amount of excreted sugar diminished gradually, but slightly, for three days. Diarrhœa then occurred and this was followed for some days by a much-diminished excretion of sugar. At this period the experiment was made of giving 284 grammes of bread with the diet. This caused an immediate and large increase of sugar-excretion, which continued the next three days, and was also accompanied by an excess in the excretion of urea. Directly he commenced this diet his appetite failed, he lost flesh rapidly, began to feel weak and exhausted, and was evidently rapidly sinking when his friends removed him from the hospital. During the time he remained under observation, determinations were still regularly made of the amount of sugar and urea passed, the latter of which was much increased in quantity. These may be seen by reference to the table, but I have not attempted to draw any conclusions from them. At the time it was thought that this man's death was hastened by the administration of bread, and that until this was given there was no reason for supposing that the case was progressing unfavourably. Whether this was the real cause of his rapid change for the worse it is of course impossible to say; but this, and all the other experiments in which bread was given, certainly impressed me strongly with an idea of its injurious influence in cases of diabetes.

I can only regard the results of these experiments as affording us so many unconnected facts from which it would be hazardous to draw any positive conclusions; and I am quite prepared to see many of them reversed by a repetition of the same experiments upon other individuals. It is, then, without any wish of attaching to them greater importance than they merit, that I offer them as a contribution towards our knowledge of the dietetic treatment of diabetes.

If they prove anything, these experiments certainly seem to show that the amount of sugar excreted in the urine is far larger after the consumption of a given weight of bread than it would be after the same amount of honey, and after this latter than after pure white sugar.

Whenever bread was eaten, it always gave rise to a large increase in the amount of excreted sugar; and in one case, or it may be at one stage of the disease, this increase was absolutely in excess of that which could have arisen from the mere conversion of the starch contained in the bread into sugar. In some experiments this sugar formation was delayed for twenty-four hours after the bread was eaten; and when this was removed from the diet, a similar interval elapsed before, by diminished excretion of sugar, the system gave evidence of its having been relieved from the effect produced upon it by the bread; though, in similar experiments repeated upon the same individual, this result was not constant. The addition of bread to the diet seemed also invariably to increase the amount of urea excreted.

In no instance when honey was given did it lead to a greater excess in the sugar excretion than would represent one-half the weight of the sugar given in the form of honey. In two experiments it was even difficult to detect any very conclusive evidence of the excreted sugar having been increased by the addition of honey to the diet. In other instances in which an increased excretion had taken place, this seemed to continue to quite as great an extent for two or three days after the honey had been entirely omitted from the diet. In some instances, honey appeared to increase the amount of urea excreted; but more generally it diminished this slightly so long as it was continued in the diet. Whenever honey was given, and then omitted, an excess in the excretion of urea followed, or continued with oscillations for several days. This had the appearance of being the normal excretion, which, having been checked by the presence of the honey, made its appearance in the urine immediately the honey was withdrawn from the diet. The most unexpected results, however, were those which followed the addition of sugar in large quantities to the diet. These results were similar in both instances in which the experiment was made, though one was made on a comparatively slight case, progressing favourably; and the other upon a severe case, rapidly tending towards a fatal termination.

In the first case, the addition of 284 grammes of white sugar to the diet raised the amount of excreted sugar 46·54 grammes, and diminished the urea 5·94 grammes. In the second case, the same weight of sugar raised the amount of excreted sugar 45·47 grammes, and diminished the excretion of urea 2·58 grammes. One of these experiments was continued for six and the other for five days; and in the former, when sugar was no longer given in the diet, an increased excretion of urea took place, and was still rising when the determination was omitted, on account of the mercury solution being exhausted.

The practical conclusions which the facts above related, if subsequently verified, would seem to justify, are:

1. That in all cases and in every stage of diabetes, bread, and probably all other amylaceous food, should be strictly excluded from the diet; for, if given, it will largely increase the amount of urine, urea, and sugar excreted, and in every way aggravate the symptoms of the disease. It is, however, probable that its injurious effect is less felt by an individual who is at the same time taking exercise and much in the open air.

2. That honey may often be advantageously used as an article of diet, because in some cases, or possibly in some stages of diabetes, a large amount of it may be eaten without materially increasing the weight of urea or sugar excreted; and because, although in other cases an increase of the sugar may occur, this is accompanied by a diminished excretion of urea, and is often very much less in amount than would be represented by the sugar consumed in the form of honey.

3. That pure white sugar may be added to the diet in diabetes with every prospect of a beneficial result; for its use is accompanied by a diminution in the amount of urea excreted, and, when given in large quantities, less than one-sixth of the amount escapes as sugar in the urine, the remainder being either burnt off, or otherwise appropriated to the uses of the system.

As regards the various theories respecting the nature of diabetes, these experiments seem to prove, that it does not at any stage depend either upon a simple arrest at the stage of sugar in the chemical changes which starch undergoes in the system, or upon sugar as it passes through the body escaping those chemical transformations by which, in health, it is converted, first into vegetable acid, and then into carbonic acid and water.

The facts that an interval of twenty-four hours may sometimes elapse before any increased excretion of sugar follows the increased consumption of starch, and that this may at other times give rise to an increased excretion in excess of that which would represent the conversion of the starch into sugar, and especially that a very large amount of pure sugar may be taken in the diet, of which only a very small proportion subsequently appears in the urine, are all circumstances which seem to make it far more probable that the disease depends upon some derangement of the liver, leading to an excessive secretion of sugar by that organ. Upon this theory we can explain the injurious effects of starch, by supposing it to act in some manner directly upon the liver, and are able at the same time to understand why both honey and sugar are always less injurious, or even positively beneficial, when contained in the diet of those suffering from diabetes.—*St. George's Hospital Reports, Vol.* v, 1870, p. 193.

25.—THE CHEMICAL RELATIONS OF URATES AND PHOSPHATES.

By Dr. A. W. **Barclay**, Physician to St. George's Hospital.

[There are many considerations involved in the deposits of urates and phosphates, which are of great service in practice. They guide us in the selection of remedies, and not unfrequently point more or less distinctly to conditions in distant organs, of which the functional disorder of the kidney is only an index and a consequence.]

The rule for the discrimination of the urates and the phosphates is so well known, so universally admitted, and so simple, that an exception might be supposed impossible. The urates are dissolved by heat, the phosphates by acid. A precipitate in acid urine soluble by heat would consist of some compound of uric acid; a precipitate in alkaline urine insoluble by heat would be an earthy salt, and probably a phosphate of lime. Conversely, a precipitate of urates ought not to exist in hot urine; a precipitate of phosphates should not be found in acid urine.

It is very many years since, as a student at St. George's Hospital, I submitted to Dr. Bence Jones, so well known as a chemist, who was at that time one of our teachers, the puzzle, which I cannot say is even now quite intelligible to me, how a phosphatic deposit could be found in decidedly acid urine. But until recently I never met with an instance of urates insoluble by heat. On May 10th, T. S., aged 17, a porter, was admitted under my care at St. George's Hospital, in a state of considerable depression, with a quick pulse, a chapped and furred tongue, and persistent diarrhœa of some days' standing, and the suspicion of enteric fever seemed rather confirmed by slight elevation of temperature. To my own mind, the history was opposed to this idea. It seemed rather to have been a case of severe diarrhœa, in which the prostration was a mild form of collapse, and the further progress of the case bore out this diagnosis. The various secretions were naturally watched, and the urine was exhibited to me with a turbid, flocculent deposit, which my clerk pronounced to be phosphates, because the urine was nearly neutral, and the precipitate was insoluble by heat, but dissolved on the addition of acid. Its appearance was certainly more like urates than phosphates, and I found, on examining it for myself, that the acid only dissolved the precipitate with heat, but had no action on the cold urine. It was consequently placed under the microscope, and was found to consist almost entirely of globular urates, with high refractive powers, mixed with some crystals of oxalate of lime. Their presence in nearly neutral urine was readily explained by the scanty amount of secretion consequent on the previous severe diarrhœa, but their insolubility by heat was a phenomenon for which I was not prepared. On the following day the urine was more decidedly acid, but there was no precipitate. On boiling, however, a copious precipitate was thrown down, which was immediately dissolved on the addition of a few drops of acid. I have already alluded to the difficulty of explaining the precipitation of phosphates in the presence of acid; when the deposit only occurs after the application of heat the change is still more difficult of explanation, because the urine after being boiled becomes rather more acid than before, as I ascertained distinctly in this case, and the granular precipitate does not dissolve readily with acid after it is allowed to cool.

Formerly we used to be taught that the presence of urates was only due to acidity, of phosphates to alkalescence of urine; but that is a very limited view. It was probably only put forward in this broad form as antagonistic to the common expression of those days, that one or other deposit showed an excess of uric or phosphoric acid, the "lithic-acid diathesis" and the "phosphatic diathesis" of Dr. Prout. It is certain that such a conclusion is very likely to be erroneous, although, perhaps, it contains some element of truth. All that can be said with certainty at present in regard to the uric-acid salts is, that when they are precipitated there must be a certain excess of acidity, and if not an excess of the salt, at least a disproportion between it and the water as ingredients of the urine.

With reference to the presence of phosphates, I conceive that we must assume *the same disproportion* to exist between the water and the earthy salts whenever the precipitate is abundant, especially when the urine is not decidedly alkaline. It is not an uncommon practice in the present day to maintain a certain degree of alkalescence of the urine both in rheumatism and gout. In such cases it is quite remarkable how the amount of earthy deposit varies while the urine is freely alkaline, but I have long learned by experience that a copious deposit is an indication of depression, and is sure to be followed, if not accompanied, by other symptoms of a corresponding kind. At one time I used immediately to diminish the dose of the alkali, but I have recently observed that, without making any other change in treatment, the addition of a few grains of quinine daily will have the effect of at once diminishing the amount of deposit, and will enable the patient to persevere with such a dose of alkali as he could not otherwise have taken with impunity. Believing as I do that in both forms of disease it is desirable to neutralise as far as possible excessive acidity, the effect of the quinine is to me a point of great importance in the treatment of these disorders.—*Lancet, July* 22, 1871, *p.* 117.

26.—QUERIES RESPECTING SO-CALLED URÆMIA.

By Dr. H. W. Fuller, Physician to St. George's Hospital.

In acute congestion of the kidneys, epileptiform seizures and other symptoms of so-called uræmia are not unfrequent; and the same holds good respecting chronic Bright's disease. In such cases the amount of urea excreted by the kidneys is far below the normal standard, but nevertheless is often considerable. If the non-excretion of urea is the true and only cause of so-called uræmic symptoms, how is it that coma and convulsions are not present in numberless cases of dwindled granular kidneys, in which the specific gravity of the urine often ranges for years together between 1001 and 1003, and in which therefore the excretion of urea by the kidneys is reduced to a minimum? Again: how is it that persons in whom total suppression of urine occurs, and in whom therefore the excretion of urea through its natural channel is absolutely arrested, not unfrequently go on for a week or ten days without the slightest cerebral disturbance? Such instances are not to be explained by the draining off of the urea by the bowels, for ordinarily the secretion from the bowels is not excessive, or in any respect abnormal; neither is it to be explained by vicarious action of the skin, for very commonly the skin is dry and inactive. Within the last two months I have seen a case with Dr. Playne of Maidenhead, in which a child of fifteen, suffering from scarlatina, had complete suppression of urine for eight days; and yet, with a dry hot skin and only a natural action of the bowels, she not only did not exhibit any symptoms of uræmic coma, or convulsion, but retained full possession of her mental faculties to the last.

Again; if so-called uræmic symptoms were due solely to the presence of urea in the blood, how does it happen that a person ever recovers who has once manifested symptoms of uræmic poisoning, including convulsions and coma? During the fit and subsequent insensibility, the secretions, if not extremely scanty, are certainly not more profuse than before the attack; and presumably, therefore, the quantity of urea in the blood must be as large at the termination of the seizure as it was before the fits commenced. Nevertheless, patients emerge from a long succession of these epileptiform seizures, and remain for months, or even years, without any farther attack, although the secretions from the skin, the bowels, and the kidneys, continue in precisely the same condition as before the seizure.

Let us, then, look the difficulty in the face, and endeavour to explain it. It is obvious that the non-excretion of urea is in some way connected with the symptoms of so-called uræmic poisoning, inasmuch as they occur only when the secreting power of the kidneys is interfered with; but it is equally obvious that some other factor is necessary to their production, and this is what requires investigation. Until it is clearly understood and taught that so-called uræmic symptoms are not due solely to the presence of urea in the blood, there is little chance of any investigation which is calculated to shed light upon this dark corner of pathology.—*St. George's Hospital Reports*, *Vol.* v, 1870, p. 11.

SURGERY.

AFFECTIONS OF THE BONES AND JOINTS—FRACTURES, DISLOCATIONS, &c.

27.—ON ANTISEPTIC SURGERY.

By Joseph Lister, Esq., F.R.S., Regius Professor of Clinical Surgery in the University of Edinburgh.

[Professor Tyndall in one of his experiments showed that a mass of cotton-wool has the power of filtering the air passed through it of its suspended particles. He blew against a beam of condensed light entering a dark room, with a pair of bellows having a mass of cotton-wool tied over the nozzle; the result being, that the beam, elsewhere white from illuminated dust, became perfectly black at the part on which the current was directed through the cotton filter; hence the idea has naturally suggested itself that cotton-wool might be used with advantage as an antiseptic dressing.]

It would be useless to apply ordinary cotton without special precautions, for, according to the germ-theory, putrefactive particles must exist among the fibres and lie scattered over the wool. But if the cotton were impregnated with some volatile material capable of destroying the vitality of the septic organisms, and then placed upon the wound after washing it with a lotion containing the same substance in solution, the result ought to be, supposing the theory true, that, after the volatile antiseptic had become dissipated by diffusion from the dressing and from the wound, the cotton-wool, though destitute of any chemically antiseptic properties, should effectually prevent, by its filtering property, the access of any putrefactive agents, and keep the wound sweet, while in itself a perfectly bland and unstimulating application. Accordingly I prepared four samples of cotton-wool by diffusing through each one of the following substances—chlorine gas, sulphurous acid gas, carbolic acid vapour, and the vapour of benzine—four materials very dissimilar in chemical properties, but having a common hostility to low forms of life. Chlorine, sulphurous acid, and carbolic acid, are well known to have such a property; and, knowing that benzine is used by the entomologist for killing insects, and having ascertained by experiment the potency of its vapour for the destruction of pediculi, I thought it probable that it would also answer our purpose. I then dressed with these four kinds of prepared cotton-wool various suppurating sores, excoriations, and contused wounds, after washing the surface with the corresponding lotion, or in the case of benzine, with the undiluted material. The results in every instance corresponded exactly with theory. After about twenty-four hours' exposure at the temperature of the body, the cotton-wool was found to have lost the odour of the antiseptic, yet the blood, serum, or pus, as the case might be, remained perfectly sweet for an indefinite period, while healing advanced in the satisfactory manner that might be anticipated from the absence of all irritating quality in the dressings. There was, however, one circumstance, highly instructive in itself, which interfered sadly with the utility of this application; namely, that, if the discharge happened to be sufficiently copious to soak through the cotton-wool and appear at its external surface, putrefaction occurred throughout the entire mass of the moistened part down to the wound, even within the first twenty-four hours after the dressing, if the fluid were sufficiently copious to penetrate within that period. It is only when dry that cotton-wool can arrest the progress of microscopic organisms, which have ample room to develope among its meshes when filled with a putrescible liquid.

And now, allow me to endeavour to bring home to you a little more closely the inference that is to be drawn from these facts. But, first, let me describe in detail the manner in which the dressing with carbolated cotton-wool was practised. The cotton-wool having been impregnated with about a two-hundredth part of its weight of the acid in the form of vapour, the surface of a granulating sore or abrasion was washed, together with a portion of the surrounding skin, with a solution of the acid in about forty parts of water. A piece of oiled silk of the size of the sore was then applied, to prevent the dressings from sticking through dryness. Over this was placed a piece of folded linen rag, rather larger than the oiled silk, and impregnated with the carbolic acid vapour in the same manner as the cotton-wool; the object of the rag being to absorb the discharge and prevent it from trickling down, as it was otherwise apt to do, below the slightly absorbent cotton, involving its early appearance at the surface and consequent spread of putrefaction to the wound. Lastly, a well overlapping mass of the carbolised cotton-wool was securely fixed by a bandage. The result, as before stated, was that, though all chemical antiseptic virtue left the dressing within a day or two, putrefaction was excluded by the cotton-wool for any length of time, provided the discharge did not penetrate to the exterior of the mass. Consider, now, the circumstances of the serum or pus that oozed from beneath the edges of the oiled silk into the folded rag—let us suppose a week after the application of the dressing, when all traces of the volatile antiseptic had certainly disappeared. Here was a highly putrescible liquid, not subjected to boiling, as in the flask experiment, or acted on by any chemical agent whatever, yet remaining free from putrefaction in a rag moistened with it at the temperature of the human body, simply because it was covered over with pure dry cotton-wool. How, then, did this cotton-wool exclude the causes of putrefaction in the atmosphere? It certainly did not keep out any of the atmospheric gases. The same cause that led to the escape of the volatile antiseptic necessarily occasioned a perpetual intermingling between the external air and that between the meshes of the fabric, as any one acquainted with Graham's beautiful researches into the laws of gaseous diffusion must at once admit. The only constituent of the atmosphere which the cotton-wool could possibly exclude is its dust; and this we know, from Tyndall's experiment, it did exclude. Here, then, we have another inevitable inference from fact, another truth, and that in itself all-sufficient, with reference to the antiseptic system of treatment; the truth, namely, that pus, blood, and the dead tissues in contused wounds do not putrefy through the influence of the atmospheric gases, but through the operation of particles of dust, which may be permanently deprived of septic energy by the vapour of an agent like carbolic acid. I do not ask you to believe that the septic particles are organisms. That they are self-propagating, like living beings, and that their energy is extinguished by precisely the same agencies as extinguish vitality, such as heat and the various chemical substances to which I have referred, is certain, and is of the utmost practical importance. But if any one, in spite of these facts, and in spite of the strong analogy of the yeast-plant, and the various kinds of fungi which we term mould, prefer to believe that the septic particles are not alive, and to regard the vibrios invariably present in putrefying pus or sloughs as mere accidental concomitants of putrefaction, or the results, not the causes, of the change, with such an one I, as a practical surgeon, do not wish to quarrel. Nor do I enter upon the question whether spontaneous generation can take place at the present day upon the surface of our globe. To do this, would be to engage in doubtful disputations which I promised to avoid.

But I do venture earnestly to beg of all of you who are engaged in surgical practice, that you will give these simple facts your careful consideration; and if you think the interpretation I have given a sound one, do not let any statements, whether in books or in journals, shake your belief in the truth that putrefaction, under atmospheric influence, as it occurs in surgical practice, is due to particles of dust ever present in the atmosphere that surrounds our patients, and endowed with wonderful chemical energy and power of self-propagation, yet happily readily deprived of energy by various agents which may be employed for the purpose without inflicting serious injury upon the human tissues. With this as your guiding principle, you will find yourselves successful with the antiseptic system of treatment; but without it, whatever theory you adopt, you will ever be walking *in the dark, and* therefore ever liable to stumble.

And now I proceed to the second division of my subject—the exhibition of our principal means and methods of treatment. For preventing the access of putrefactive fermentation, the agent which we now commonly use is what we have termed the antiseptic gauze, of which these are samples—being a loose cotton fabric, the fibres of which are impregnated with carbolic acid securely lodged in insoluble resin, which holds the carbolic acid with remarkable tenacity, while at the same time a little paraffin is added to prevent the adhesiveness which the mixture of carbolic acid and resin would otherwise possess. The interstices between the fibres are kept free from these ingredients, so that the fabric, being porous, may be fitted for absorbing discharges. The carbolic acid is in considerable quantity in the gauze; but it is held so tenaciously by the resin that, on the one hand, when first applied it is unirritating to the human skin, and, on the other hand, unless discharge be very copious, it will retain its virtues for upwards of a week at the temperature of the human body. Now supposing I were going to use this gauze for dressing any case in which a copious discharge was expected—as, for example, a large psoas abscess immediately after it had been opened—I should take a considerable quantity of the gauze (about as much as one can conveniently hold between the extended hands) and fold it three times so as to make it eight layers. But there would be no use in my having the folded gauze of this extent, if I did not adopt some means for compelling the discharge to pass throughout the length and breadth of the dressing; and for this purpose some impermeable tissue must be interposed between it and the external air. That which we have found the most convenient is a cheap and light form of mackintosh, termed "hat-lining" by the India-rubber dealers. I cut a piece of this, nearly as large as the folded gauze, and then place it beneath the layer that is intended to be outward. The discharge then coming from the wound, situated opposite the middle of the gauze, instead of passing directly outwards through it, is compelled to traverse all the extent of the antiseptic dressing: and in that way, by using a sufficiently large piece, and with this arrangement of the mackintosh, you may be perfectly certain that, if you leave no putrefactive mischief in a wound or abscess, none will enter it, however profuse the discharge may be during the first twenty-four hours. That is one very important point gained. As the discharge diminishes, the intervals between the dressings are extended; and when it amounts to only a minim or two in twenty-four hours, the application may be left undisturbed for a week. The gauze is also extremely convenient in the form of bandage—an antiseptic bandage—which is put on to hold the main dressing in position; and instead of being a nidus for putrefaction, as a cotton-bandage would be, it increases at every turn the antiseptic efficacy of the dressing. Besides this, the bandage having a degree of stickiness, its turns do not tend to slip as those of a cotton-bandage do, which is an additional advantage.

Such, then, are the means by which, in ordinary cases, we ensure that putrefactive fermentation does not extend from without into the wound or abscess. Of course it would be of no use to apply such an external dressing if putrefactive particles in an active state were left within a wound. If a wound be presented for treatment, having been inflicted by another than the surgeon, some dust is sure to have been introduced; and we must first destroy its septic energy by washing the raw surface thoroughly with some liquid trustworthy for the purpose, such as chlorine-water, or sulphurous acid lotion, or a strong solution of carbolic acid, or of chloride of aluminium,for there are various preparations which may be used with efficiency. But when the surgeon operates on a previously unbroken integument, he has the opportunity of preventing the septic particles from entering in an active state at all, by operating in an antiseptic atmosphere. This is readily provided for in small operations by using a watery solution of carbolic acid with Richardson's apparatus for local anæsthesia. For making the spray more fine, I have found it convenient to have the lower end of the water-tube almost entirely stopped up, leaving only very minute apertures. The result is, as you see, an exceedingly satisfactory spray. For any small operation this answers the purpose perfectly well, provided always that you take the precaution of having the liquid filtered through a cambric handkerchief or some *similar fabric, in* order to exclude the grosser particles of dust, which otherwise would have the effect of blocking up the fine orifice at the nozzle of the apparatus—an occurrence which, under some circumstances, might be disastrous in its effect.

We have lately found that the strength of the solution employed for producing the spray may be considerably reduced. We have ascertained that it may be used as weak as one part of carbolic acid to a hundred parts of water; and that a spray made with such a lotion is thoroughly trustworthy as an antiseptic atmosphere.

The reduction of the strength of the spray is a matter of great importance. In the first place, it is a great comfort to the surgeon, as I can testify from experience. When we used a solution as strong as one part of carbolic acid to forty parts of water, my hands were constantly in a rough and uncomfortable state; but when the proportion is reduced to one to a hundred for the production of the spray, the hands experience no inconvenience whatever, and one can even breathe with comfort in such an atmosphere.

In the second place, it is equally advantageous for the patient, because the weaker the antiseptic application, of whatever sort it is, the less irritation do we occasion to the tissues of the part treated with it. The antiseptic is always injurious in its own action; a necessary evil, incurred to attain a greater good. To suppose that it is useful by its own operation in some specific manner unknown to us, is an entire mistake. I know that, not only from theory, but as a matter of experience. At one time, I used the undiluted acid; and, in doing this, I could not avoid producing not merely irritation, but a certain amount of sloughing. Then I used a strong solution of carbolic acid in oil; then a rather strong solution in water; then a weaker watery lotion; and now we employ a solution as weak as that which I have described—one part of carbolic acid in a hundred of water—and that applied only in the form of spray, avoiding absolute drenching of the tissues at all, and avoiding also the injection of the wound by a syringe, as we used to do after the operation was completed, in order to destroy the organisms introduced; and, in direct proportion to the weakness of the solution used and to the smallness of its opportunity of acting on the tissues of the part, is the satisfactoriness of the results obtained, provided that the essential object of avoiding putrefaction is secured.

And now, supposing that I were single-handed, about to change the dressing in the case to which I have alluded—a large psoas abscess—the spray is of extreme value. I wish that the spray shall play upon the surface of the body, in the angle between the dressing and the skin, as I lift the gauze. It would be very inconvenient if it were necessary for this purpose always to have an assistant to work the spray; but, by a little management, the spray can be worked perfectly well, as you see, by the surgeon himself. [This is done by placing the bottle of Richardson's apparatus against the ball of the thumb, and holding the India-rubber bulb to be compressed between the opposite side of the bottle and the fingers of the same hand.] Supposing this were the site of the incision in a case of psoas abscess, as long as I choose I can perfectly protect it with the antiseptic atmosphere, and then put on what we have called, for the sake of distinction, a "guard" —a piece of rag dipped in the one to one hundred watery solution of carbolic acid, after which the spray can be removed with security; the surrounding parts having then been cleansed from any discharge there may be, the spray is once more made to play on the part during the exposure of the wound until the permanent antiseptic dressing is re-applied.

But though such a spray-producer is perfectly efficacious for a small operation, it does not make a cloud of sufficient volume for a large one, such as an amputation of the thigh or at the hip-joint. Therefore, with the object of securing the same result in such cases, I have had this apparatus prepared, which, I confess, is in a cumbrous and heavy form; but I hope it will be improved in that respect before long. Meanwhile, it is much better than nothing. Let me say a word or two, in the first place, as to the principle on which it is constructed. It appears that the best kind of spray which can be produced, is that which is formed on the principle of the atmospheric odorator, by having one tube set at right angles to another, the air-tube being larger than the water-tube, and the opening of the water-tube being exactly opposite the middle of the orifice of the air-tube. This makes the finest and best of all sprays. But, with a heavy apparatus like this, it would never do to have to move it about along with the nozzle, as is absolutely necessary in the instruments of ordinary construction on this principle. We must have tubes to convey the air and the water to a considerable distance; and this is very easily done by not merely having the liquid ejected by the force of the air blown over the orifice of the water-tube, but by having it driven through the tube by the force of the same pump that propels the air, the quantity of the water being regulated by a stop-cock. Then it was necessary to provide some ready means of clearing the fine end of the water-tube, in case of its obstruction by particles of dust. This is done by having the water-tube straight for a short distance from the nozzle, and then bent at a right angle, with a little milled cap to screw on at the angle, so that, in case of obstruction, the cap is screwed off, and the orifice of the water-tube is cleared at once with a needle or a bit of fine wire. I have used this apparatus in various operations of late, among which I may mention my last two amputations, one in the thigh, the other in the arm, in both cases using nothing stronger than the one to a hundred solution for the spray, and the same for the sponges; except only, what I believe to be a wise precaution, that, when a sponge has become soaked with blood, it should be washed first with pure water, then dipped for a moment in a strong solution (one to forty), and then squeezed out of a solution of one to a hundred to give it the necessary blandness; and in both these cases putrefaction was entirely avoided. [The apparatus exhibited had two nozzles, attached to independent caoutchouc tubes, furnishing large clouds of spray, that could be directed, if necessary, to opposite sides of the part operated on. Two of Richardson's spray-producers, worked by two assistants, will answer the same purpose, though less efficiently.]

The antiseptic catgut-ligature is used for securing the arteries while the spray still plays over the wound. It is absolutely necessary that it should be properly prepared. I must not enter into the method of preparation, further than to say that catgut undergoes a remarkable change in its physical constitution when steeped for a long time in an emulsion of water and oil, so that it becomes quite transparent, and no longer liable to become soft and slippery when placed in water or in a watery discharge. But for this cicumstance, the animal ligature would be an impossibility; but, if you use it properly prepared, you will, I believe, see good reason to be satisfied with it. That which I now show is extremely fine, much finer than any silk commonly employed; and yet with a piece like this I should not hesitate to tie the femoral artery in a stump. If you choose to use it thicker for a large vessel, you can do so. It is conveniently carried on a little winder, in a capsule appended to a caustic case. The catgut, as tied in the ordinary reef-knot with the ends cut short, seems to me to be a perfect hæmostatic. It has all the simplicity and universal applicability of the ligature, with, at the same time, the virtual absence of any foreign body from the wound. If putrefaction be avoided, it is rapidly absorbed, and you may reckon as certainly on the absence of any interference with primary union on the part of such ligatures, as if there were no ligatures at all. Should putrefaction occur, I was at first uneasy lest the prepared catgut might soften and permit hemorrhage. I was, therefore, at the pains to test some of the prepared catgut in the following manner. I tied some pieces of it at intervals round a cylinder of India-rubber, so as to pinch the India-rubber to a considerable degree of constriction, and then introduced it into putrid serum of blood, and kept it for a week at a temperature of about 90 deg. At the end of this period, the India-rubber was still constricted, shewing that the catgut had retained its hold in the putrid liquid, in spite of the constant strain of the elastic material upon the knots. No doubt, in such parts of a wound as actually putrefy, the little bits of catgut must come away like shreds or sloughs of cellular tissue; but I am bound to add that this is only a matter of presumption: for, although I have used nothing but this ligature for securing vessels in wounds for more than two years, excepting torsion, which I comparatively rarely resort to, and though in certain classes of cases putrefaction cannot be avoided, in no instance have I seen the catgut knot come away, nor have I ever known secondary hemorrhage or abscess caused by its use.

I have spoken of the injury that the stimulating carbolic acid lotion inflicts on the tissues by irritation. The great disadvantage of this is, that it causes an unusually large flow of serum during the first twenty-four hours or more; and you must provide a special exit for the serum, else you will have inconvenience from tension, which will lead to suppuration, though not of the putrefactive kind. For the purpose of guarding against this, I introduce, at the most dependent part of the wound, a strip of lint steeped in a solution of carbolic acid in about ten parts of olive oil, to serve as a "drain." This is drawn out under the spray in twenty-four or forty-eight hours. If you drew it out without providing an antiseptic atmosphere, you would certainly have putrefaction. In some cases, a fine drainage-tube is convenient for this purpose, if well steeped in solution of carbolic acid. For, as India-rubber happily absorbs carbolic acid, the drainage-tube is antiseptic when introduced.

There is yet one other point to which I must allude, which is that carbolic acid interferes with the cicatrisation of a wound, if it act directly on it. This agent operates with special energy on the epidermis. Sometimes this is a convenience. For example, if we dip the forefinger into a carbolic acid lotion, and hold it there for a second or two, we may be certain that the epidermis is so imbued with the carbolic acid, that it is for the time antiseptic, and therefore may be introduced into the cavity of an abscess or any other part which we wish to explore; and very valuable an antiseptic forefinger often is in that way. But this action of the acid on the epidermis makes it interfere with cicatrisation; and even the gauze, though generally perfectly free from irritating influence upon the sound skin or an old scar, will frequently, if applied directly to a wound, entirely arrest new epidermic formation, and sometimes excoriate a tender young cicatrix. Something, therefore, must be interposed to protect the wound from this effect of the antiseptic. What we have generally used hitherto for this purpose is what we have called the "oiled silk protective," consisting of oiled silk varnished with copal varnish, which makes it much less permeable to the carbolic acid. But unfortunately, this is not a perfect protective. It acts admirably until it becomes moistened; but afterwards the water that penetrates the substance conveys the carbolic acid in. I have striven in various ways to get something perfect in that way; and I have lately been engaged in a manner which, though not yet completely successful, may be mentioned on account of its interest otherwise. Some time since, I tried the effect of an oil-paint on oiled silk, in the hope that the particles of pigment, closely packed, might serve considerably to intercept the carbolic acid, though the oily material that cements the particles is permeable to it. The result was such as I had hoped, except that the material proved too stiff for convenient use. A few weeks since, however, I happened to be going through an India-rubber factory, and there I saw, among other things, the process of mixing various pigments with caoutchouc; and it occurred to me, might not India-rubber, blended with some pigment, answer as a protective? The India-rubber is permeable to the carbolic acid; but with the pigment it might not be so. I first tried a coloured rubber that had been vulcanised, and then came out a most curious and interesting circumstance. The sulphur in the vulcanised India-rubber acting chemically on the discharge, the result was a stench like rotten eggs, presenting an excellent example of decomposition without putrefaction; for there was no putrefactive fermentation—no spread of the decomposition into the interior of the wound or abscess. It was limited to the exterior, and was simply the result of the chemical action of the nascent sulphur upon the discharges. And if under such circumstances we resumed the oiled silk protective, we again had perfect absence of unpleasant smell.

The necessity for avoiding any sulphur in the material was a great cause of embarrassment; for, as a general rule, the admixture of any foreign ingredient with caoutchouc causes a most inconvenient softness and adhesiveness of the product—evils which vulcanising completely corrects. Magnesia forms an exception to this rule, producing with the pure rubber a very satisfactory substance as regards its physical properties. But then we found that, in the case of a sensitive skin, this magnesia caoutchouc produced intolerable itching and redness, for a reason which I do not quite understand. At length it occurred to me that perhaps shell-lac, which seems quite unirritating, might be mixed with the caoutchouc; and that this might answer the purpose. For though shell-lac, when once mixed with carbolic acid, holds it very tenaciously, as is seen in the lac plaster with which some of you are familiar, yet the acid does not readily penetrate into unmixed lac. When I suggested this to the managers of the India-rubber works, they told me that they had previously ascertained that shell-lac could be perfectly blended with caoutchouc; the product being the beautiful article you now see, sufficiently tough, yet pliant, transparent, and with no unpleasant odour, and, as I ascertained by experiment, practically impermeable to carbolic acid. Here, then, I thought I had attained the object at which I had been aiming for years; and already we were getting results of a kind we had never got before: we had reached more nearly than ever before the conditions which we know must occur subcutaneously. I had never witnessed the healing of ulcers proceed so rapidly as I have seen it under this protective, covered with overlapping gauze; but to my extreme chagrin I have learnt, within the last few days, that, in two patients with very sensitive skins, even this material produces a trifling irritation. Still I cannot but believe that we are on the verge of getting what we want in a protective—viz., a tissue perfectly bland and unstimulating in its own substance, and also quite impermeable to the antiseptic.—*British Medical Journal, Aug.* 26, 1871, *p.* 226.

28.—ON ANKYLOSIS.

By Bernard E. Brodhurst, Esq., Orthopædic Surgeon to St. George's Hospital.

[On determining on the treatment to be adopted in a case of ankylosis of a joint, we consider less the position of the limb than the character of the adhesions. Soft or fibrous adhesions we call false ankylosis; and hard or bony adhesions we call true ankylosis.]

False, fibrous, or partial ankylosis, then, is occasioned by the deposition of lymph within or around a joint, through which adhesions are formed, which interfere with motion; while true, complete, or bony ankylosis, or synostosis, is that condition in which the soft structures of the joint have been destroyed, and bony union has taken place between the adjacent and bony surfaces.

False ankylosis may be *intra-capsular;* which signifies a junction through membranous bands or fibrous tissue of the contiguous surfaces of the bones which enter into the articulation; or it may be *extra-capsular*, in which case lymph is deposited external to the capsule, either in the shape of membranous bands, or there may be produced thickening of the capsule, or of a part of the capsular and ligamentory tissues around the joint.

In many instances, however, the structures both within and beyond the capsule become inflamed and thickened; lymph is poured out into the cellular tissue and about the sheaths of the tendons and muscles, and the various structures become more or less matted together, whether in an extended or a flexed position; and lymph is also deposited upon the synovial membrane, through which adhesions form and motion is hindered. In some cases the adhesions which have formed are slender, but they may, notwithstanding, prevent useful motion; or again, they may be more extensive, and yet admit of treatment to restore motion. Inflammation may extend from without inwards, or the reverse may take place. Thus, of the first description, we have inflammation arising from mechanical injuries, phlegmonous erysipelas, and burns; while of the latter, acute synovitis, as from over-exercise, and strumous, rheumatic, syphilitic, and gonorrhœal inflammations are the chief forms.

Each of these forms of inflammation presents characters peculiar to itself in the ankylosis which results; so that it may for the most part be predicated what form of inflammation gave rise to the joint affection. It is of importance, *in the treatment* of ankylosis, to distinguish the various results of these different *forms of inflammation*—to recognise in the result the amount of force that may be necessary to overcome the rigidity of the joint; for as the sensation which is communicated to the hand in bony ankylosis is altogether different from that which is presented by fibrous ankylosis, so is there scarcely less difference between the firm fixed condition produced by gonorrhœal rheumatism, and the soft adhesions which result from scrofulous inflammation.

The *treatment* of fibrous ankylosis may be divided into, 1st, gradual extension with or without tenotomy; and, 2nd, immediate flexion of the limb with or without tenotomy, and subsequent gradual extension.

In all cases of partial ankylosis there exists some muscular rigidity; in some cases, also, cicatrices are found, resulting from loss of substance. Where adhesions are recent, contraction of a limb may probably be overcome by continued extension—such extension, namely, as is made by means of a well-adjusted instrument for the purpose; but, except in cases of recent adhesions, it is generally necessary to commence the treatment by dividing the tendons of rigid muscles, and by dividing cicatrices subcutaneously. It is better to proceed at once to these subcutaneous sections, rather than to prolong the treatment by extension unnecessarily; for, unless the adhesions are recent, simple extension is seldom of itself, and uncombined with subcutaneous section, sufficient to remove the contracted condition of a limb. It is important to remember this principle of treatment; for partial displacement of the articular surfaces is easily induced by continued extension of the limb, if the tendons have not been previously divided. Indeed, it is not uncommon to see this displacement take place at the knee when extension is long continued, and where the tendons have not been divided. Whenever, therefore, it is desired to remove contraction, it is the rule first to divide the tendons of rigid muscles, and to divide cicatrices subcutaneously, and subsequently to proceed gradually to extend the limb.

But if such be the law of treatment where the articular surfaces occupy their normal positions, it is even more to be insisted on when any displacement has taken place. Extension should then, without fail, be preceded by the subcutaneous section of such tendons, fasciæ, and cicatrices, as might interfere with the readjustment of the articular surfaces.

These obstacles to extension, then, having been removed, a well-adapted instrument is to be applied to the limb, and extension is to be made slowly. The instrument should support the limb efficiently; and it should always, in the first instance, be applied at that angle, whatever it may be, at which the limb was held before the subcutaneous sections were made. So soon, then, as the punctures have healed, extension may commence, and be carried on gradually, without producing pain, and without occasioning displacement.

Numberless cases, however, exist in which the means above mentioned are useless to restore to the limb either the normal position of its parts or to restore motion; cases, for instance, in which the adhesions are so firm that they do not yield to gradual extension. The pressure produced by continued extension may occasion destruction of the integument, or it may induce displacements, partial or complete, of the articular surfaces; but the adhesions, whether intra- or extra-capsular, will not yield to such force. Injury alone, but no useful result, can accrue from gradual extension in these cases. Before chloroform was introduced, these were among the *opprobia* of surgery. Then, gradual extension of such limbs was continued for months and years, without any advantage being derived.

I know an instance where an advocate of the gradual extension system, and gradual extension only, continued to apply his means to a case of ankylosis of the knee for the space of fourteen years. The extending apparatus was removed by another surgeon, and the knee was excised, because bony ankylosis had taken place at an inconvenient angle. Probably, if left to himself, the first operator might have continued his efforts at extension, in the firm hope of ultimate success, after another period of fourteen years.

It is necessary, therefore, before proceeding to the treatment of a case of this kind, to form a correct diagnosis—to determine whether complete ankylosis has taken place, or whether the adhesions are fibrous; and, if fibrous, whether they will or *will not yield* to gradual extension. If these several points cannot be otherwise determined, chloroform should be fully administered; so that, when muscular relaxation has been obtained, both the character of the adhesions and the amount of motion may be ascertained.

When bony union has taken place, a sense of solidity and continuity of structure is communicated to the hands on grasping the limb above and below the articulation; but when fibrous adhesions have formed, either slight motion may be felt at the articulation, or at least a sense of elasticity is communicated, on endeavouring to flex the limb. And if the adhesions are of such a character, so firm and unyielding, that the normal position of the limb can only be gained by force suddenly applied to rupture the adhesions, the force should be so applied that it is used mainly, if not entirely, in the flexion of the limb.

It was only when tenotomy and anæsthesia were combined that the operation of forcible flexion could be looked upon as free from danger. I have performed 267 operations of this nature, and I have never known any *contretemps* whatever—neither fracture, nor dislocation, nor pyæmia, nor inflammation.

Any tendons which are rigid should first be divided; and the punctures having healed, and chloroform having been fully administered, the limb to be operated on should be so firmly fixed that all motion is prevented, except that which the operator is about to impart to the limb. Thus, for instance, if the hip-joint is to be operated on, the pelvis must be fixed; if the knee, the thigh must be securely held; and so on. When the limbs are thus firmly secured, the adhesions are to be instantaneously ruptured by force applied in the direction of the flexion. I say that the adhesions are instantaneously ruptured, when the patient is properly prepared, and the force is rightly adjusted. The limb is then to be bandaged; and especially the affected joint is to be firmly bandaged, and confined either in a gutta percha splint or on a flexible splint. This bandage should on no account be removed until tenderness has ceased. The articulation should be firmly encased in the bandage; and should this become loose, another should be placed tightly over it, and be allowed to remain so long as any tenderness continues. It is rare, indeed, that the bandage requires to be moistened.

I know of no danger whatever from the use of force thus applied. Indeed, when the influence of the muscles is perfectly removed, the adhesions themselves usually offer very little resistance; and if the power to be applied is sufficient for the purpose, the result is instantaneous. In a small number of instances, the hand alone is insufficient to rupture the adhesions readily; and in these I make use of an instrument to flex the limb. Not only is there no danger connected with this operation, but with moderate care it would seem to be impossible to set up unhealthy action. It is sometimes said that in these operations fracture is not uncommon, and that inflammation is not unfrequently excited. Let it be sufficient for me to say, that I have never seen a fracture produced, nor have I known inflammation to occur, nor any other ill whatever to follow an operation of this nature; and that when disaster ensues, it is from abuse of the operation. Doubtless this operation is capable of abuse, just as is any other operation; but when it is performed as I have endeavoured to describe it, I do not know an operation more successful than this in the whole range of surgery, nor one more free from danger.

When the joint retains its normal external form, the adhesions are easily broken down by the hand, after the limb has been properly placed in position and the full effect of chloroform has been obtained. There was lately a case under my care in the hospital, where the patient, having suffered from rheumatic inflammation, was admitted with partial ankylosis of the knee and of the ankle. The tendo Achillis was, in the first instance, divided; and after the puncture had healed, the adhesions at the knee joint were ruptured by flexing the foot upon the leg. On a subsequent occasion the hamstrings were divided subcutaneously: and, the punctures having healed, the adhesions at the knee-joint were ruptured by flexing the leg upon the thigh. This patient walked well when she left the hospital, and without lameness; and the movements at the knee and ankle-joints of the affected limb were as free as those of the sound limb.

It is a point to remember, that after dividing the tendons, and before the punctures have healed, the adhesions should not be ruptured; or they should be ruptured only with great care, lest the puncture should be extended into a rent. This extension of the puncture is much easier to effect than might be supposed; and it is, therefore, safer to allow the punctures to heal before any force is employed.

When the position of the limb is perfectly restored, then passive motion should commence. At first it may be necessary to administer chloroform, for motion is painful; but as motion increases, passive movements excite less pain.

True Ankylosis.—Complete, bony, or true ankylosis, is rare. When it has taken place, a sensation of solidity is communicated on grasping the limb above and below the articulation, such as can only be occasioned by continuity of bony structure. In fibrous ankylosis this sensation is never experienced. Fibrous ankylosis may, however, allow of so little motion, that with rigid muscles it may be inappreciable until chloroform has been inhaled. Therefore, as motion is thus masked, and as bony ankylosis is rare, it is safer not to express an opinion in favour of bony ankylosis until chloroform has been exhibited. When the patient is under the influence of chloroform, no doubt can exist as to the nature of the adhesions.

Bony ankylosis is the result of inflammation and suppuration within the joint, together with the destruction of the articular cartilages. Ankylosis may then result between the exposed surfaces of the bones, if the inflammation be of a reparative nature, causing the deposition of new bone. When destructive inflammation ensues, causing necrosis of the epiphyses, bony union is impossible. Diseased action must cease before repair can commence; and bony union is repair. This repair, however, may be of such a character as to be useless, and even worse—it may be detrimental. Such cases then admit of treatment.

Ankylosis is occasionally observed as a congenital affection: the articular apparatus is then entirely absent. In these cases an inconvenient angle is never found, and the reparative process is as complete as nature can make it.

In bony ankylosis the articular extremities are either bound together in the course of the ligaments, or the bones are united in their entire thickness; two becoming fused into one. It has occured to me once to see a living person without a single movable articulation—every joint was ankylosed.

Bony ankylosis is, however, rare; but union may take place at such an angle as to be in the highest degree inconvenient. Under these circumstances, this repair, which is intended to be permanent and useful, may fairly be made the subject of surgical interference.

Treatment.—There are four operations which may, under certain circumstances, be done to restore motion or to improve the position of the limb—viz., 1st, to remove a wedge of bone; 2nd, to break through the ankylosis, after drilling through the new bony formation; 3rd, to make a false joint; 4th, to divide the bone subcutaneously, and restore the position of the limb.

[Mr. Brodhurst then gives an account of these operations as performed by Dr. Barton, of Philadelphia; Professor Brainard, of Chicago; and himself, which we advise the reader to peruse in these valuable reports of St. George's Hospital.]—*St. George's Hospital Reports, Vol.* v. 1871, *p.* 149.

29.—ON A NEW APPARATUS FOR THE TREATMENT OF FRACTURES.

By Dr. Anthony H. Corley, Surgeon to Jervis-street Hospital, and Lecturer, Carmichael School of Medicine, Dublin.

[In describing and bringing under the notice of the profession a new way of using plaster of Paris in the treatment of fractures, Dr. Corley lays no claim to originality. He has found the plan both convenient and efficient, and it appears to us worthy of trial.]

We are accustomed to summarize the treatment of fractures of the extremities in the four words: *Extension, Counter-extension, Co-aptation*, and Retention.

Every practical surgeon knows and appreciates the difficulty of fulfilling the last of these indications. The variety and multiplicity of splints, bandages and other means of retaining the fractured ends in apposition, sufficiently testify to the importance of the object to be secured and the attention bestowed on it. In Jervis-street Hospital, where patients with fractured limbs are admitted almost daily, it is a matter of necessity to practise some method of treatment which will do full justice to the sufferer, whilst involving the least trouble and loss of time to the surgeon. Since the introduction of starch and plaster of Paris bandages, the benefits conferred by these have been freely taken advantage of, but their use in the ordinary way is attended with very serious objections. The first of these is the trouble and time spent in their application; and the next, a still stronger argument against them, is the difficulty of *taking them off.* Even with the facilities afforded by Seutin's Pliers, and other resources, the removal of the rigid apparatus is a work of much difficulty. This makes the surgeon unwilling to apply it until the fracture is so far consolidated as to no longer require frequent inspection. In the institution named, neither starch nor gypsum is ever used until about three weeks from the date of the accident, and in most cases during that entire period the patient is confined to one position. Starch bandages, &c., have the great disadvantage of requiring some days to harden, and unless the fractured bone be tolerably well united, or the most constant attention be paid during the time of drying, the ultimate removal of the apparatus may disclose a deformity which is for the remainder of the patient's life a practical illustration of careless surgery. Gypsum hardens at once, but has the same defect in difficulty of removal, and cannot therefore be applied until the period before referred to. The irksomeness, and even danger, attendant on confinement to a constrained position can scarcely be over-rated, especially in old people, and the chief advantages of the apparatus which I am about to describe are, that it can be used at a much earlier period, and that when applied the patient may lie in almost any position.

In the appendix to the *Army Medical Report* for 1869 (just published), it is described with an illustration, by Staff-assistant Surgeon Moffitt, who states that it is used by the Bavarian Ambulance Corps. Two pieces of flannel, suited to the length of the limb, are cut sufficiently wide to overlap slightly in front. When so prepared they resemble the leg of a stocking cut vertically. One is now laid over the other, and they are stitched together from top to bottom, down the mesial line, like two sheets of note paper stitched at the fold. They must now be spread out under the injured limb, so that the line of stitching corresponds to the back of the calf. The two inner leaves, so to speak, are now brought together over the shin, and fastened by long pins, the heads of which are bent. The leg being held firmly, an assistant mixes the plaster with about an equal bulk of water, and rapidly applies it, partly with a spoon and partly pouring over the outer surface of flannel *covering the limb.* The two portions of the second layer are then quickly brought over, so as to meet, and the inequalities in the distribution of the plaster are removed before it hardens, by smoothing with the hand. In about three minutes the gypsum sets, and the limb is encased in a strong, rigid covering, which gives uniform pressure and support to every part. The edges of the flannel in front can now be trimmed, and the pins withdrawn from the inner layer, by seizing their bent heads. A couple of straps, or a few turns of a roller, make all secure. In order to take the apparatus off, it is only necessary to remove the straps and separate the edges of the flannel, when the two sides will fall asunder, the line of stitching behind acting as a hinge.

The application takes less than ten minutes, the removal about two. Thus, from day to day if necessary, the limb can be inspected, and the splints (for they are no less) re-applied. In cases of compound fracture, an opening suitable to the wound may easily be made. In most cases it is desirable to make a number of perforations with a gimlet, so as to prevent unnecessary heat. The following cases, treated within the last month, or still under treatment, illustrate its efficiency:—

Case 1.—Simple fracture of fibula, immediately above the malleolus. In this *case the patient*, a restless, uneasy, middle-aged man, could not be induced to keep his leg in a box or splints, but when the plaster and flannel were applied he expressed the greatest satisfaction. As he was a tailor by trade, and not requiring to stand or walk, I allowed him to leave the hospital at the end of a fortnight, with instructions to wear the plaster for a month.

Case 2.—Fracture of fibula, about its centre, in a young boy. Apparatus applied at the end of a week, and in a few days he was able to walk with crutches.

Case 3.—Oblique fracture of both bones of leg in lower third, much shortening, rotatory, deformity, ecchymosis and swelling. This last sign was so marked that it was not deemed judicious to apply the Bavarian apparatus until the fifteenth day. The patient, a man of about forty years of age, experienced much relief, could keep the limb in any position and move it so freely that he complained of the weight of the apparatus interfering with his movements,

Case 4.—Fracture of both bones of leg immediately above ankle-joint, with much twisting inwards of the foot. For the same reason as in the last case, the plaster was not applied until the twelfth day. While the plaster was setting the foot was kept turned in the requisite direction, and in hardening the correct position was retained. The patient, a young girl, felt quite comfortable in a few minutes.

Two of the above cases, with others since admitted, are still under treatment. In conclusion, I believe it will be found that the thanks of the profession are due to Dr. Moffitt for introducing such a valuable adjunct to surgical appliances.—*Dublin Quarterly Journal*, *Aug.* 1871, *p.* 62.

30.—ON THE TREATMENT OF HIP-JOINT DISEASE.

By Dr. Lewis A. Sayre, Surgeon to the Bellevue Hospital, New York; and Lecturer on Clinical Surgery.

[Dr. Sayre considers extension the proper basis of treatment of hip-joint disease. He doubts very much whether disease ever commences in the cartilage of a joint, though this subsequently becomes involved in it. The diagnosis whether the disease has commenced in the bone, or rather in the blood-vessels at the extremity of the bone, or in the synovial membrane, or in the ligamentum teres, is sometimes difficult to make out, for the one soon runs into the other.]

Taking it for granted that I am speaking, not to medical students but to medical men who are perfectly familiar with the physiological condition of the joint, I will go at once to the pathological changes that take place.

There is, first, a synovial inflammation, which is always followed by effusion. Then there is a wrench or tear of the ligamentum teres, which is almost always followed by destructive inflammation of the bones of the joint, particularly if a concussion have been at the same time associated with the wrench or tear. Synovial inflammation is almost always the result of exposure. Skating, racing, jumping, playing, football, and other movements that over-exercise the joint, followed by a sudden exposure and change of temperature, will produce synovial inflammation in the hip-joint as well as any other joint. When boys are wrestling, going through violent gymnastics, twisting their legs in various positions, putting the ligamentum teres upon the stretch will produce a tear—probably not tearing it entirely off, otherwise attention would be called to it, and they would be put under treatment; but generally the smaller the rip the greater the danger, because it is neglected; and it is sometimes months before the disease is seriously developed, and frequently by this time the origin of the difficulty has been forgotten.

A concussion, a blow, a jump from a great height, would cause a pressure upon this little network of blood-vessels which I described as existing between the intra-articular cartilage and the head of the bone; this would produce a "blood-blister," *or extravasation* of blood at that point, which would be the nidus or starting point of the disease ; and, if the injury were detected at the time of its infliction, *rest alone* for a sufficient length of time would probably result in a favourable termination of the difficulty in great majority of instances. But, the injury not being detected, and in many instances not even suspected, the *rest* is not *insisted* upon at the proper time, and thus the disease is slowly developed, and frequently is not distinctly pronounced until so long a time after the accident that caused it, that the trifling injury which has been the origin of so much trouble has been entirely forgotten.

A pinch of the skin producing a "blood-blister," or slight extravasation in the cellular tissue, is of common occurrence, and of no great importance. If left alone it will soon be absorbed, or at most, if you let out the fluid and do not irritate the wound, it will soon get well : but, suppose even in this most trifling injury that, instead of giving it rest and time to heal, you constantly scratch it with a rusty nail, you will produce a sore that will last as long as the irritation is continued. This is a parallel case to a joint being exercised after a concussion or wrench producing an extravasation in the tufts of blood-vessels already referred to.

The accidents to which I have referred are the three general causes of the disease which will of course be more or less modified in its symptoms according as it is developed in one or the other of the tissues referred to. If there be effusion within the joint it is always accompanied by a peculiar distortion. That distortion necessarily arises from the fact that the capsular ligament can only hold a very small amount of fluid without being distorted. You will remember the ilio-femoral ligament that lies upon the front of the capsular ligament, running from the anterior inferior spinous process of the ilium to the trochanter minor ; it is folded over the capsule, and causes it to lie close to the neck of the bone. There is a very small amount of fluid in the capsule normally, and you cannot increase that amount without increasing its capacity by *unfolding* it. As long as the limb is in the normal position, the capsule rings round the head of the bone so tightly that no additional fluid can be placed there ; but if inflammation take place, and there be increased secretion, accommodation is made by the unfolding of the capsule, and the flexing of the limb when the capsule becomes loose, so as to enable it to hold this increased amount of fluid. This is the reason why the leg always becomes flexed, abducted and rotated outwards. I am particular about these changes, because they materially aid you in making a diagnosis ; and the thing is to be able to diagnosticate the disease in its early stages, when a great deal can be done towards its relief.

If the effusion have become very great, the limb is more flexed, more abducted, and more rotated outward, and at the same time more fixed, apparently ankylosed, as if it had been solidified by plaster of Paris; yet it is not a bony ankylosis, but simply arises from distension by effusion and from muscular rigidity.

One of the things that is sure to take place in any inflamed joint of long standing—the hip, knee, or any other joint—is atrophy of the muscles above and below it ; and at the same time reflex irritation causes muscular tension or muscular contraction. The muscles that move the bone become irritated and contracted, so as to bring pressure against the diseased surfaces, which aggravates and complicates the disease from the beginning to the end. This is one of the great sources of pain and trouble which we have to overcome in these diseases. So in diseases of the hip-joint, the muscles contract and flex the limb, and would adduct it, but it cannot be adducted on account of the anatomical distension of the capsular ligament necessarily forcing the limb outward, rotating it outward and *ab*ducting it. Now, the adductor muscles (as all who have seen this disease must have observed) become, like catgut, intensely contracted, and feel like a strong cord. The desire to bring the limb in is on account of this reflex irritation of the adductor muscles, and the absolute impossibility of doing so is because of the capsular distension of which I have already spoken. If you take a dead body and flex the joints so as to break up the *rigor mortis*, and place it on the back so that both limbs are in their natural position ; if you now bore a small hole through the ilium into the acetabulum and forcibly inje [illegible] ount of quicksilver, it will cause the limb to flex, abduct, and rotate outward at the hip-joint. By driving into the hole a small piece of wood to retain this increased fluid within the joint, you will find it impossible to extend, adduct, and rotate the foot inward, without rupturing the capsule; and the attempt to do so, if the capsule do not rupture, will force out your plug like a pellet from a pop-gun.

It is the struggle between the irritated muscles to adduct the limb, and the impossibility of the limb yielding to their traction on account of the over-distended capsule, that cause the pain to be so great in the *second* stage of the disease; and it is made manifest on the inner and lower portion of the thigh near the knee, on account of a little filament of the obturator nerve which runs off there to the ligamentum teres, and a sensitive branch from it running down to the adductor muscle. This over-distension can only be relieved by puncturing the joint and letting out the superfluous fluid, or by cutting the tendon, so as to accommodate the one to the other. The treatment will of course depend upon the condition in which you find the patient; you must exercise your own judgment as to which method is to be preferred, according to the condition in each case. In this stage of the disease the pain is agonising, torturing, with loss of sleep and loss of appetite. The least attempt at movement in any way causes the most agonising shrieks; and at this period ulceration takes place in the cartilage of incrustation. The patients begin to have what are called night-spasms—the peculiarly characteristic pains that occur in the night depending, I suppose, upon the fact that the child becomes completely worn out, loses its hold on the muscles, and drops off to sleep for a second; then there is a spasmodic contraction bringing this increased pressure and movement of the diseased joint, and the child wakes up with a shrill shriek. The mother runs upstairs, and finds, perhaps, that the child has dropped off to sleep again. You hardly ever ascertain this fact unless you live in a hospital, or stay for several nights in a house where there is a child with disease of the hip-joint, when you will appreciate this thing very accurately indeed. The tension of the muscles that the child is undergoing all the time keeps the joint from movement, and makes him comparatively comfortable; but that is done at the expense of vitality. The constant effort of the child to hold the muscle still to prevent movement wears him out, and by and bye he drops off to sleep from sheer exhaustion, and then, when there is the least movement, he screams as though he were pierced with a red-hot iron; instantly the muscles are on guard again, and the child is relieved. That goes on over and over again. After a while, ulceration takes place, and the capsule ruptures; then this imprisoned fluid escapes from the expanded capsule into the cellular tissue, and the child is comparatively easy. But the disease has still further progressed. We often think we have gained a great deal because the child is so much more comfortable, whereas the disease has only gone on to the *third* stage, in which effusion takes place into the cellular tissue. Almost immediately there is a marked change of attitude, or character of the deformity. The leg is immediately adducted, drawn close to the other; the toes are turned inward, and the limb is apparently shortened. If this rupture that takes place through the capsule be very slight, so that the pus oozes out by slow degrees, the change takes place very slowly; but if (as is often the case) there be a big rent in the capsule, you may find the child distorted in one way to-night and to-morrow the very reverse. The fluid has suddenly escaped; and that has always been heretofore described as the luxation which occurs in the third stage of hip-disease. I looked through your museum the other day and saw a great many specimens, but none are luxated. The theory of luxation in the third stage was proved by Dr. Alden March of Albany, New York, to be a mistake. The head of the bone is absorbed; the acetabulum is absorbed and increased in size; and the capsular ligament which surrounds the acetabulum slips up, and up, and up, and so it remains—the periosteal development, giving an additional growth of bone, makes a new attachment above, so that the capsule is still attached, and the head of the bone embraced in it only slipped up. You may call it, then, a displacement of the acetabulum, not a luxation of the head of the bone. The absorption taking place in the acetabulum and in the head of the bone, causes the limb of course to be very much shortened; and it being adducted, and the neck having been nearly destroyed (in many instances entirely), the idea of luxation has arisen; but I draw a distinction between a destroyed and changed position of the acetabulum, and a luxation of the head of the bone. I call it a luxation when the head of the bone is outside the capsular ligament; but if the ligament have been displaced or moved in its position, still embracing the head, it is a displacement, not a luxation.

So much for the pathological changes that take place; next as to the causes of this disease. My impression is, that the great error we have always committed has arisen from looking at this disease as necessarily one of constitutional dyscrasia, a blood disease, a constitutional poison; that this is simply a local manifestation of a general disease. Believing this, we treat the system, we give internal remedies, and apply our medicines for the purpose of changing the character of the blood, instead of paying attention to the local disease which is the great source of trouble. All authors, as far as I know, particularly the older ones, have looked upon this as a scrofulous or tuberculous disease of the bone, necessarily connected with some hereditary taint or some peculiar scrofulous condition of the system, and therefore they believed that the disease must necessarily go on to destruction, and that there was no necessity for any local treatment except counter-irritation, blisters, setons, and so forth. Internal remedies were the principal things relied upon. The result was, with all that kind of treatment with which we are familiar, always the same—viz., shortening of the leg by absorption of bone, and, finally, the consolidation of it in more or less deformed positions; and then the patient frequently came into a condition of robust health. I do not suppose there is any person in this room that cannot call to mind some old fellow with a shortened hip perfectly ankylosed, yet with a ruddy face, a good healthy complexion—a vigorous robust old man. If he had had scrofula in his blood, if he had had tuberculosis in his system anywhere, it would have remained there; and when the hip recovered, the man would have been a miserable old fellow after all. The fact of his being a vigorous robust man after going through all this trouble, proves, in my judgment, that the disease is not of constitutional origin. The fact that in so many cases the joint has been exsected when the patients have been apparently at the point of death, and that they have become, when the source of trouble has been removed, vigorous strong persons, is another evidence that the disease is not constitutional. Then there is the still stronger fact that, by making application to the local disease, and treating it irrespectively of any constitutional taint, you produce perfect results, so that the patients recover without the slightest deformity, and with perfect motion; that is the best proof in the world of the disease being local. Follow up the history of these cases, and you will find that it is a local disease. Out of several hundred cases that I have accurately observed, and taken the trouble to trace their history, the immense majority, I may say ninety per cent., occur in the most vigorous, robust, wild, haram-scarum children—boys that take their chances of danger, that run races, that climb over fences, jump out of apple-trees, kick their playmates down stairs, ride down the banisters, are reckless and careless. The sickly, poor, scrofulous child, who clings to his mother's apron, does not run the change of getting hurt. I do not say that scrofula is a preventive against disease of the hip-joint—by no means. All things considered, a smaller amount of injury would produce the disease in one of these miserable young ones sooner than in a healthy robust child. But such children take very little chance of being hurt, and consequently the majority of cases occur in the active and robust. The injury that starts the disease is sometimes very trifling, and not observed at the time. I might illustrate this by my own experience. In getting out of one of your cabs, I have nearly torn off my tendo Achillis. You have no kerb-stone as we have, and no steps to your Hansoms; and the result was that, expecting to step down only eight or ten inches to reach the stone, I found I had to go eighteen or twenty inches, and thus I strained the tendo Achillis. I fI had torn it entirely off it would have done no damage, except loss of time, because I should have been compelled to lie by; but I hurt it so little that I paid but little attention to it; and I have been walking about for a week or ten days, until I can scarcely walk any more. It is now bad enough for me to begin to take care of it. If I had hurt it a little less, it would have been more dangerous, because it would have gone on by degrees producing those conditions that terminate in a destruction of the part.

In the first stage of the disease it is necessary to examine the child by taking off every stitch of clothes. Then, going behind, you will observe the peculiar form of the nates. The diseased side will have the gluteo-femoral fold a little dropped down; the two buttocks are not exactly alike; and you will observe, if you watch carefully, that the toe has a tendency to turn out, and the child bends his knee and hip. The sound leg is made into a solid column to receive the concussion and bear the weight of the body; the child is careful not to concuss the other at all. By careful measurement you will find that, even in the first stage, the thigh is already smaller than the other. Of course you want to examine very carefully in this stage, and it will take time to detect it; but, after examining some little time, you will see the gluteo-femoral fold dropped lower, the nates flattened, the line from the centre of the sacrum to the trochanter major a trifle greater than on the other side, and the leg having a tendency to be slightly abducted, and flexed at the knee. You then lay the child perfectly flat on the table. The case before us is a very illustrative case. You see the perfectly cautious manner in which the child moves; she sees everything, and keeps herself constantly on guard. There is now no movement at all at the hip; she is locked up, and she keeps herself as quiet as possible. The case is one that has gone on for some time [the mother stated that the child had been suffering for four years; that she had a slight fall at first, and did not complain of it at the time], and it is so conspicuous that anybody could find it out across the street. It is in the incipient stage, such as this child was in three years ago, that you want accuracy of diagnosis. This case has gone on to the third stage of the disease.

It is essential to a proper diagnosis of these cases to have a normal standpoint to go from, and it is this. The body of a child in a healthy state can be laid upon a flat surface, the spine lying upon the floor or table; and a line drawn from the centre of the sternum over the umbilicus to the centre of the symphysis pubis, and another drawn from the anterior superior spinous process of the ilium of one side to that of the other, cross each other at a *right* angle, when the trunk and pelvis are in a normal state. In that state, if the hip-joints be perfect, the legs can be brought so that the popliteal spaces can be made to touch the floor or table. Now, whatever position the other leg has to be kept in, in order to get these two lines at a right angle with each other, that is the characteristic deformity of the leg. A sofa, bed, or lounge will not do for this examination—it must be done on a *solid* foundation. In this case, you see, in order to get these lines right, I am compelled to hold the leg flexed, but it is simply adducted. That is a very important point. It is an evidence that the capsule has become ruptured. If there were effusion in the capsule, the leg would be *ab*ducted and rooted outward. The fact that it is adducted, and inside the central line, as you see, is evidence that the disease has gone to the third stage, and that the capsule has become ruptured.

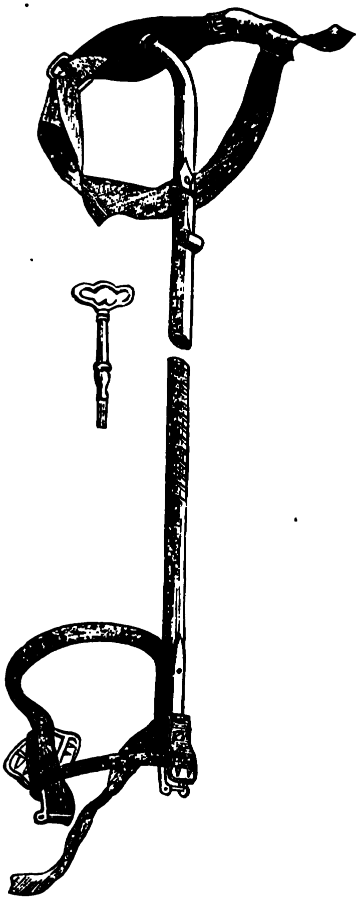
You see that the child, when held in the manner in which I am now holding her, has no pain. Putting my hand upon the pelvis, I make movements, and you see that the joint is not ankylosed. Seizing the pelvis with one hand, and passing the other under the knee, and making moderate extension in the direction at which the deformity has arrived, I make movements, and the child has no pain whatever. I let her go for a second, and then, on making the slightest movement, you see what pain she suffers. Now, I have ankylosed that joint; there is ankylosis perfect and complete to all appearance; it is muscular rigidity that has produced it. You see here the disease aggravated and continued by pressure. You could not have a better illustration of the practical working of the principle of extension.

You perceive that the child can be moved about without any difficulty if she be handled properly, and the limb slightly extended. A person accustomed to it can take up a child with apparently the greatest carelessness, and will *seem* to be acting with perfect brutality, without giving any pain whatever. He knows how to apply his force; and yet in a single instant he can produce the most agonising torture. See the way in which the child locks herself u makes herself perfectly rigid the instant I omit my extension.

Pressure causes absorption. By constant pressure of the head of th against the acetabulum, there is absorption of the head and absorption acetabulum; destruction takes place, and terminates in an abscess whicl an outlet somewhere; or else it goes on to ankylosis.

FIG. 1.

Instrument of extension.



Many persons apply a plaster of splint in these cases. This is go far as it keeps the part still; bu lacks the power of extension, i not accomplish all that is desired. better to have the limb recovei motion than ankylosed.

If I take hold of the pelvis, anc moderate extension, the child is fre pain; if I put it down straight her, for I put the psoas magnus a iliacus internus on the stretch. straightening is to be done by sl grees. You see the result in the the other child, whose leg was lik but is now much better, from tl that extension has been applied fo days. It is best to put these pati bed and apply the night ext You put the extending force diseased limb very nearly in the a deformity as you find it; lowerin extension by slow degrees, until, course of a week or ten days, or three weeks, you get the limb as s as in the case of this other child.

This is really the proper time tc the instrument which I propose on this child in order to enable it out-of-door exercise. If you tak the early stage of the disease you want to do that; but, this child undergone this deformity, we mu her as we find her.

Mode of applying Extension take two strips of adhesive plaist or three inches wide (according to of the child), and extend them fro above the ankle on each side, an enough to reach two or three above the knee, first having had sewed fast to the lower extremity plaister for the purpose of puttin buckle for night extension. These are secured by a roller, as you see. I always put on the plaister without l it. When heated, plaister is very apt to slide from itself; it moves off fr web or cloth on which it is spread. If you heat it, it burns the skin, or away from itself; but if it be put on with gentle pressure, this does not I am careful not to make the upper end of the plaister stick before reacl If you put on the plaister with a fold and get a pucker in it, the effect same as pulling your boot on with a pucker in your stocking: it does n you much till after dinner; before bed-time it is unbearable. The plais to be worn many months, and it should be applied carefully. I mentio *little* matters as the result of my own experience and observation. Y *only learn by making* blunders: and having made many myself, I try t *others to avoid them.* It is remarkable how easily the plaister may be w months if it is smoothly and comfortably adjusted. The skin should always be thoroughly washed, and carefully *wiped* before applying the plaister. Having gone above the knee for an inch and a half or so, I reverse the plaister over the roller. At first I make the roller hold the plaister until I get above the knee; that is, simply to catch a bite, as it were, on the condyles, so that extension shall be made upon the hip, and not upon the lateral ligaments of the knee-joint. I then reverse the plaister and bring the roller back over it; and now the plaister is made to hold the roller in place so that it will not slide. This can now be worn for three or four months. If the bandage become dirty you can take it off without removing the plaisters; or you can put a long stocking over it.

This is now ready for night extension. We next put on the plaister to apply an instrument for day extension, so that the child can run out of doors. This is done by taking two fan-shaped pieces of plaister, and sewing on to the pointed end a piece of webbing just wide enough to fit the jaws of the instrument. The instrument—(Fig. 1)—is made of two pieces of steel, one sliding into the other, as you see, and made to lock by a key. This rod terminates in two branches that go over the thigh, and come on to the inner portion of it. I formerly made the instrument to go down the whole length of the leg; but finding that it only stiffened the knee-joint, I prefer now to make it go over only the thigh. By means of a cross branch you take the extension from the inside of the thigh. The rod should be made to extend from an inch and a half to two inches above the condyles of the femur up to the crest of the ilium. It ends in a little roller, over which the webbing is to play, and it is capable of being elongated and shortened at will by the key. The upper portion terminates in a ball and-socket joint in a little steel band which runs round just under the crest of the ilium. At each end of this band is attached a perineal pad, which runs under the perineum and is buckled; and counter-extension is made on the perineum so that the child walks on the perineal pad instead of on the hip-joint. This was a desideratum which I was always trying, for a great many years, to obtain, but did not succeed. A man named Davis was the first to make an instrument accomplishing this object. It was a piece of steel running down the whole length of the leg to the ankle. Extension was made by a joint and a buckle, but it could not be gauged, regulated, or controlled with ease or accuracy; still the principle of keeping up extension, and at the same time permitting motion, was made first by this man Davis. Had he remained an honourable man, true to his profession, he would have deserved to have his name passed down to posterity with respect; but having taken out a patent for his instrument, he has incurred the reprobation of every honest and honourable man. We will, however, give the devil his due, and admit that this man was the first who made an instrument accomplishing this object. But his instrument has been so modified and so immensely improved, that his patent is of no avail.

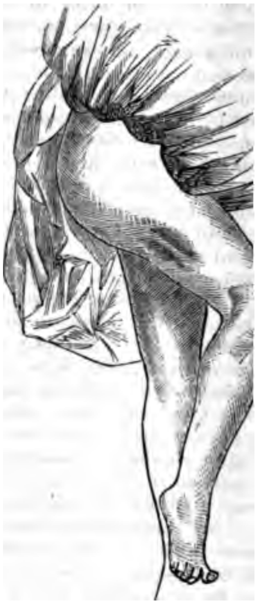
You lay the instrument over the thigh. If it do not fit exactly, you can bend it with the fingers; if it be a little too tight, you can spring it apart. You put your thumb and finger opposite where the two lower jaws come, and you will know the place to commence the application of your plaister for the extension of the thigh. Sometimes the pain is so intense that it is necessary to give chloroform. The plaister is put on and bandaged round, as you see, working in and out basket fashion, so as to cover any inequalities of the skin. After a few applications, you learn how to do it snugly and smoothly. By having the plaister cut in slits at the upper ends, it can fall in pieces or fall together more readily, and thus be applied more smoothly.

I used to put the bandage over the pelvis, in the form of the figure 8, but I prefer the plan of splitting it in strips and reversing it over the roller, as you now see. Remember that it is not requisite to pass above the trochanter major. It is hardly worth while to draw the anatomist's attention to that; but I have seen the best surgeons thoughtlessly apply the plaister clear above the joint, and of course it only pulled the pelvis instead of opening the joint.

If this instrument had been put on the child before she became so much distorted, you can well understand how much greater the advantage would have been; or had she been kept in bed two or three weeks, and the limb pulled down straight; but we will take the worst kind of case, and see w will do any good or not. As I am compelled to put on the instrume diately, I press the plaister on, rather than apply it by heat; and by the limb extended, I can press the plaister home without hurting t But it is better to leave it for a few hours to get an attachment. [The accompanying sketches (Figs. 2 and 3) show the child before and after the instrument was applied. In a few minutes after its application, the child walked around the room in perfect comfort, sat down on a chair, and got up several times to show the perfect freedom of motion in the joint.]

FIG. 2.

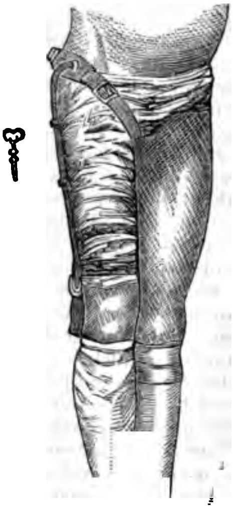
Child before the application of in



This treatment may be required for months and months; and when the patient is well I make him wear the instrument for weeks or even months afterwards, as a means of protection or guard. If the child be small, no crutches are required; but, of course, if the weight of the body be too great to be sustained by the plaister, crutches must be added. The advantage is, that the ligaments are kept in movement. The objection to the *fixed* treatment is, that the ligaments which are not involved in the disease become diseased by rest. By giving them motion they are kept in a normal state. A piece of old leather, if you do not handle it and keep it supple, is good for nothing.

FIG. 3.

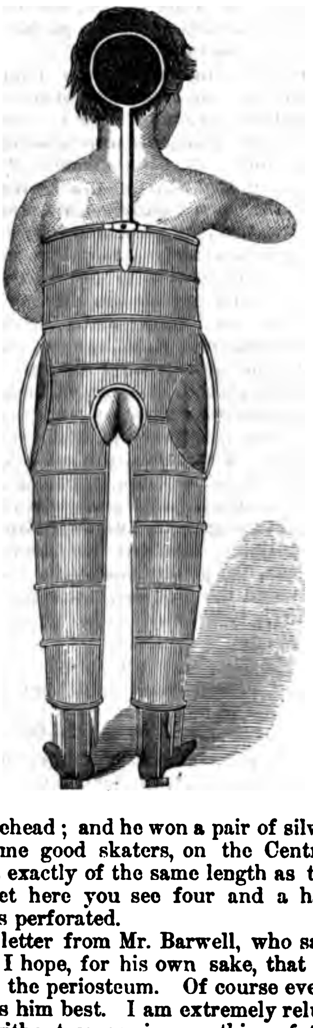
Child after the application of the in



When the disease has gone on to another stage when sinuses have occurred, and discharge pus, when a probe leads down to dead bone, there is nothing to be done but to exsect it by making a small ncision above the trochanter major, midway between it and the crest of the ilium, over the top of the acetabulum—a semiunar incision, the belly of the D covering the posterior part of the trochanter major, going straight down to the bone, *through* the periosteum. You then pull the soft tissues on one side, and, taking a small but strong curved bistoury, go as far around the bone on each side as you can reach, at right angles to your first incision, so as to divide the periosteum completely; you then take a strong firm periosteal elevator, with a large handle, and the end slightly curved, and go into this little triangle; you *peel* off the periosteum, and, as a matter of course, all the muscles with it; by opening the joint thoroughly, and turning the head of the bone out, the periosteum is peeled off from the inner portion; you then saw off the bone above the trochanter minor. I believe that this is better than cutting through the neck. If you go through the neck, the trochanter major comes over the hole and prevents *discharges*; whereas, by cutting off the *trochanter major* along with the neck of *the bone, you* leave a perfectly free opening for the discharges from the diseased joint; and by peeling off the periosteum in the way which I have described, you carry with it all the muscles that move the joint; and if you then keep the leg pulled out to its proper length, by putting on a pair of wire breeches, you can send the patient out into the air the very next day (Fig. 4).

FIG. 4.

Wire-breeches.



Here is a photograph showing the result of a case that has been exsected. Three inches and a half of bone have been taken away, and the head of the bone united. Here is another, in which four inches and a half were removed, and the man recovered with a moveable joint. This which I now show you was the first case performed in America. I operated on the patient in 1852. I invited many surgeons of New York to come and see the operation; but no one attended, as at that time it was thought to be unjustifiable.

The patient is, as you see, a handsome woman, and no one would observe any defect in her. Here, however, is the most successful case, in which four inches of bone were removed. The whole acetabulum was destroyed; the head and neck nearly absorbed. There were several sinuses leading down to dead bone. I removed the head and neck, cutting it off just above the trochanter minor; but, finding the disease extending further down, I pushed the femur up, and sawed it off again. By keeping the limb extended, if you will peel off the periosteum in the manner which I have described, the bone will be regenerated, and be as good as ever. By extending it sufficiently, you make the limb as long as before. When you have got the bone regenerated, and before consolidation takes place, you commence passive movements, and in that way obtain an artificial joint, as in the case of an ununited fracture. By constant movements you prevent ankylosis; and, as the muscles have all been carried off with their insertion at the periosteum, you have certainly got the normal position from them to be moved again. The proof of it is in this boy. He bends his leg at right angles; he can kick higher than his forehead; and he won a pair of silver skates two years ago, in contest with some good skaters, on the Central Park Pond in New York. The leg is almost exactly of the same length as the other, and the motions are perfect; and yet here you see four and a half inches of his femur, and the acetabulum was perforated.

I have been rather surprised to receive a letter from Mr. Barwell, who said he was going to publish it in the *Lancet*. I hope, for his own sake, that he will not. He dislikes my plan of removing the periosteum. Of course every one has the right to select any plan that suits him best. I am extremely reluctant to be dragged into controversy; and, without answering anything of the kind, I will draw your attention to the answer which you can make yourself. If you take an orange and cut a slit down through the skin, I should like to see any of you with a sharp penknife peel the orange out of the skin without splitting the skin or sticking the knife into the orange; or if you take a straight broad knife, like a scalpel, and take the orange out without damaging the skin or making an occasional hole in the orange, you are more dexterous than I am. But if you will take a knife and cut across the skin, you can take the handle of a silver spoon, slip it in, and peel round under the skin, and separate the orange from its covering without tearing it. So it is in regard to the femur. In *dissecting* off, you tear the periosteum into shreds, destroy its vitality, and prevent it doing the good which you want it to do. By commencing *above*, and "prying" it off, without first limiting its extent by the circular incision above referred to around the femur, you are in great danger of loosening its attachment to the bone below the point where it is to be sawed off—where you do not want to loosen it. You want the periosteum left attached to the femur. Therefore I prefer cutting down over the trochanter major, through the periosteum, down to the bone, as far as opposite a line a little above the trochanter minor. Taking a small bistoury, and going at right angles to this first incision, half-way round on each side, I thus limit the line of removal of the periosteum to the place where section of the bone is to be made; and below that point the periosteum remains attached to the bone. Since adopting this plan of operation, and always using the saw to divide the bone, instead of the pliers or bone-forceps, I have not had exfoliation follow my operations, as in former instances. To prove that this is the better way, I may mention that I have operated forty times, and thirty of my patients are walking about with excellent movement of the joint; and you here see daguerreotypes of them. Eight or ten of them, I am confident, might be walked into this room and not one gentleman here would be able to say which leg was operated on. These are cases in which I have operated in the way I have described. I did not learn to do this the first time I tried; and some of the cases, therefore, did not get on so well. I have had some short legs; and I have had cases where there was a good deal of exfoliation, because I did not peel the periosteum properly; and I have had exfoliations because I cut off the femur with a forceps, which in my judgment ought not to be done.

Let me, before I conclude, say a word on operative surgery and anatomical surgery. You sometimes see beautiful dissections. Men will hold the knife like a pen, lay out the various tissues, and give a very pretty dissecting-room demonstration. Every surgeon should go through this anatomical surgery in the dissecting-room, and do it thoroughly and frequently; but when he comes to apply his knowledge to the living human being, let him be an *operating* surgeon, and go straight to the place which he wants to reach, without splitting hairs and dividing endless series of fasciæ.—*British Medical Journal, July* 22, 1871, *p.* 85.

31.—ON EXCISION OF THE HIP-JOINT FOR DISEASE.

By Frederick James Gant, Esq., Surgeon to the Royal Free Hospital.

What are the changes which the joint and constitution undergo in the course of natural cure by anchylosis? In the joint there is a twofold process of destruction and reparation. A piecemeal or molecular excision, so to speak, is constantly progressing, apparently by absorption, and certainly by the draining away of débris of bone in the discharge until two healthy opposed surfaces are reached, so that union may at length be effected. This natural cure of joint disease entails a protracted period of recovery, extending even to many years, as compared with that of weeks or months required for recovery after excision. During this ordeal the constitutional vigour is reduced, subsequently leaving the patient stamped with the aspect of suffering in former years. Occurring also as it often does during the growing period of life, the reserve power, which should have been gained to meet the exigencies of after-life, is used up prematurely by incessant demand in the long process of reparative anchylosis. Persons who have undergone the natural cure of diseased hip-joint, for example, may be seen hobbling about the streets, being easily recognised by the characteristic gait of old-standing hip disease, and by their sallow and prematurely aged appearance. This constitutional decrepitude may possibly be averted by a remarkable acceleration of the excisional part of the process. In a case represented by specimen No. 7 of the hip-joint series (exhibited before the Society), an eminent surgeon differed with myself and others respecting its diagnosis; nature subsequently undertook the operation of excision *en masse*, for she severed and discharged the greater portion of the head of the femur through one of the fistulous tracks. This natural excision of a joint—one of the only two, I believe, on record—will be singularly suggestive to operative excisionists, and it should be equally admonitory to those surgeons who blindly oppose the operation.

The conditions of hip-joint disease appropriate for excision.—It was formerly held, and may still be maintained by some surgeons, that excision of the hip-joint for disease should be resorted to only in the following conditions, constitutional and local:—

(1) Only in the last stage of the diseased or of constitutional endurance.

(2) Only when the extent of disease is limited, the acetabulum being free from disease and the amount of pelvic disease trivial.

(3) Only when the head of the femur is dislocated.

The reverse of these rules, or nearly so, may be partly gathered from my cases, and can, I believe, be justified by accumulated experience, drawn from the results of a large but varying number of cases, with regard to each such rule in question.

Of the three conditions laid down respecting excision of the joints in general, the first only applies to the hip-joint.

(1) Destruction of the articular cartilages, without the supervention of anchylosis, will always justify operative interference by excision. The constitutional condition will probably *not* then have advanced to hectic and emaciation. But the state of the general health should primarily determine the necessity for excision in all cases, and not any arbitrary consideration of the period of the disease and the condition of the joint. Whenever, therefore, the general health is manifestly failing, whatever may be the stage of the hip-joint disease, excision should be resorted to, and without further delay. This guiding rule was strongly urged and clearly illustrated by Mr. Hancock in his recent lectures at the Royal College of Surgeons. On the other hand, the most extreme state of constitutional exhaustion, previous to the operation of excision, may be followed by recovery after removal of the diseased bone—as the successful results in my own series of hip-joint cases, 1, 2, 4, and 6, clearly show.

(2) Osseous anchylosis with malposition of the limb will *not* justify the peril of attempted excision. Section of the neck of the femur is practicable, whereby the limb can be brought down to a straight position. This principle of operation was lately practised in a case by Mr. W. Adams, and successfully.

(3) The *extent of bone* diseased may be considerable, and involve both the femur and acetabulum. In the *femur* the diseased portion may include the head, neck, great trochanter, and shaft, entering even into the medullary canal. In the *acetabulum* the diseased portion may include the whole floor of this cavity, and even extend to adjoining portions of the ilium, pubes and ischium. Neither of these conditions of extensive osseous disease prohibits excision; but the acetabulum not unfrequently recovers itself when the diseased head of the femur has been removed from further contact and attrition.

(4) Dislocation is unfavourable for excision, as implying an advanced stage of the disease constitutionally. The significance of this local condition will therefore diminish in proportion to the absence of marked hectic and emaciation.

Operation.—Excision of the hip-joint was originally proposed by White, of Manchester, in 1769; but the operation was first performed by Schmalz, of Pirnie, Saxony, in 1816, and first performed in this country, and for the second time in Europe, by Anthony White, of the Westminster Hospital, in 1821. It was repeated by Hewson, of Dublin, in 1823, after which period the operation fell into disuse until its revival by Sir William Fergusson in 1845. Since that period it has been performed by many surgeons, and in a large number of cases.

The hip-joint, deeply placed owing to the neck of the femur, is reached most conveniently by a T-shaped incision; the vertical line perhaps slightly curved, being made from just above the great trochanter downwards on the shaft to about three inches or less in extent, and the transverse line about half that extent on the summit of the longitudinal incision. The very limited extent of this latter incision avoids the femoral vessels anteriorly, and the crural nerve posteriorly. In disease of the joint, with perhaps consequent dislocation backwards on the dorsum ilii, and wasting as the result of long-standing disease, these incisions seem to be almost invited, so prominently does the trochanteric portion of the femur abut under the integument. By detaching the integument on either side of the vertical incision, keeping the knife turned towards the femur, especially on its inner side, the subjacent portion of femoral shaft is exposed; then, sinking the knife vertically in the transverse incision, just above the trochanter, the attachment of muscles thereto is divided, so that the finger can be readily passed down to the joint and its state ascertained. The capsular ligament will generally have given way or entirely disappeared. To turn out the remnant head of the femur for excision it may be necessary to adduct and evert the limb, when, with a touch of the knife on the bone, the round ligament yields and the head starts from its socket. Or this ligament also may have disappeared, and the head and neck of the femur be so reduced, and the acetabulum so patulous, from more advanced disease, that the bone can be readily dislodged and hooked out with the finger. In a third class of cases dislocation backwards has taken place. In any case, however, adduction of the limb across the opposite thigh presents the bone for application of the saw; and then the diseased portion is removed by one or more successive slices, the integument on either side being protected by a curved spatula. The chain-saw may be used by those who prefer it. A gouge may be used to finish off the femoral excision, instead of unnecessarily removing any healthy portion of the trochanter, if that be left, or of the adjoining shaft. The acetabulum should be scraped rather than gouged, to remove any carious or denuded portion; or more extensive pelvic excision may be necessary, and has proved successful. But superficial caries, acetabular or pelvic, will often recover itself, the former having been maintained by constant attrition of the femoral head. Any hemorrhage is easily arrested by torsion. I have never had occasion to apply a single ligature in any hip-joint excision.

Excision of the trochanter major may occasionally prove sufficient, caries of this portion of the femur existing without disease of the hip-joint. I have had one such case, and with a successful result.

The *after-treatment* of excision, whether of the hip-joint or of the great trochanter alone, is very simple. The limb may be laid straight in bed, and retained in position only by a small side pillow or roller sand bag, without absolutely fixing the thigh. Or a long splint may be applied, extension being made from the opposite thigh as recommended by Sir W. Fergusson. Of these two modes of after-treatment I prefer the former, especially for the joint operation. The section-end of the femur is drawn up by muscular action, and hitches just above the acetabulum, which, having been generally more or less superficially carious, is thus left to recover itself, undisturbed by any attrition of the femoral end of the bone; while a new and firmly fibrous movable joint forms where the end of bone rests above the acetabulum. There is little or no tendency to displacement after hip-joint excision, and the slight extra shortening which results from thus leaving the limb to itself is unimportant compared with the advantages in regard to the acetabulum, and the formation of the best kind of new joint requisite for the functional use of the limb in progression, as well as for support. All my cases were treated in this way, and with perfect success.

Results.—(1) In relation to life or mortality. In 111 cases collected by Dr. Hodges of *unrecorded conditions* of operation, 56 recovered, 53 died from the *combined effects* of the operation and the previous disease, and in the remaining 2 cases amputation was resorted to. Thus about 1 in 2 died—a very high mortality. But Mr. Hancock presents the following very interesting results as to the mortality with reference to certain *guiding conditions* of disease for operation:—The *acetabulum*, in a healthy state, gave a mortality of 6 in 18 cases, or 33 per cent. On the other hand, acetabular *disease* has had more favourable results of operation. Of the 10 cases in which perforation had taken place, 6, or 60 per cent., recovered; 2 only, or 20 per cent., died. Of the 4, in which not only perforation existed, but abscess also within the pelvis, 2 recovered, 2 died,—50 per cent either way. Of the 3 in which the acetabulum was trephined for the evacuation of matter from the pelvis, 2, or 66 per cent., recovered; 1, or 33 per cent., died. Therefore, in the whole 20 of these apparently most unfavourable cases for excision, the mortality was only 5, 1 in 4, or 25 per cent. *Dislocation* of the head of the femur having taken place, the per-centage of recoveries was actually 46, against 23 where it had remained in its socket; the total number of cases compared being 143. Of my own 8 cases of hip-joint excision, in 4 there was dislocation, and they all recovered equally with the 4 in which dislocation had not occurred.

Another equally large series of cases—112, British and foreign—has been collected by Dr. R. R. Good, late surgeon in the Confederate American army. This series is the more complete as it embraces the most *essential particulars* respecting excision of the hip-joint for disease, both in regard to its mortality and the state of the limb. We are thus enabled to take a commanding view of the whole subject.—*Lancet*, *July* 15, 1871, *p.* 77.

32.—THE APPROPRIATE SPLINTS FOR USE AFTER EXCISION OF THE KNEE-JOINT.

By Jonathan Hutchinson, Esq., Surgeon to the London Hospital.

At the London Hospital, for some time past, stout hatter's felt (as used for the hats of the Cornish miners) has been largely employed for these splints. It is a substance very readily cut and moulded to the limb after soaking in hot water, and capable of easy adjustment with iron brackets, etc., where such appliances are required. We saw several such splints in the wards, and they seemed to be excellently adapted for their purposes, being quite firm and yet light, and comfortably moulded to the joints supported by them. Mr. Hutchinson mentioned that, in his last three cases of knee-excision, he had modified the operation somewhat by peeling up a skin flap, after making the customary horse-shoe incision, and so getting into the joint above the patella, and removing all the synovial membrane and ligamentous tissues in front of the bones; and in these cases the wounds had healed very favourably.—*Medical Times and Gazette*, *July* 1, 1871, *p.* 8.

33.—ON FRACTURE OF THE PATELLA.

By Jonathan Hutchinson, Esq., Surgeon to the London Hospital, the Blackfriars' Hospital for Skin Diseases, and the Royal London Ophthalmic Hospital.

In most patients suffering from transverse fracture of the patella, if we examine the bone several hours, or a day or two after the accident, we find the fragments separated from one another by a considerable interval, from a quarter of an inch to an inch. We also find, most likely, considerable effusion into the joint. At first thought we shall, perhaps, be inclined to ascribe the separation of the fragments to contraction of the rectus muscle; but that it is really due to quite another cause will be apparent when we examine a little further. We shall probably find, in the first place, that the rectus muscle is not in a state of contraction, or that it becomes so only under the pressure of the fingers or at the patient's will. It is true that muscles possess a certain small amount of *elasticity* in addition to their *contractility*, and this elasticity, when unopposed, will *tend to draw the fragments* apart; this force is, however, comparatively unimportant, and for practical purposes it may be safely neglected. We must bear in mind that the patella is firmly attached, by its entire circumference, to tendinous or ligamentous structures of great strength. Now, when the bone is broken across the fibrous attachments on each side, the tendons of the vasti remain entire, and they are amply sufficient to hold the portions of the patella in place and prevent their separation, so long as there is no distending force exerting pressure from within. Soon however, fluid is effused by the synovial membrane, the joint is distended, and what was before a mere fissure through a bony part of the joint-capsule becomes a wide gap. This is the state in which we generally find the knee-joints of patients admitted with fractured patella. In a few cases, however, scarcely any effusion takes place, and it is precisely in these rare cases that the broken portions of the patella never become separated to any appreciable extent. Those cases in which the joint does not become distended, and the fragments are not separated, are the most favourable of any as regards good union ; the case will be unfavourable just in proportion to the *amount* and *duration* of the effusion—*i.e.*, the extent and duration of the separation of the fragments. The further the fragments are apart the more difficult will it be to bring them together ; and the longer they remain separated, the less likely are they to unite by bone.

You will readily see, then, that the principles of treatment are to prevent effusion if it has not occurred, to favour absorption of fluid which has been poured out, and to bring the fragments of the patella into close and permanent apposition.

It is unnecessary that I should go into detail on any but the method of carrying out the last named principle.

The plan which I invariably adopt, and which has been carried out in the case before us, is to put the limb on a straight back-splint, and by that means bring the lower fragment as high as possible, and by pieces of cross-strapping maintain it there ; then to bring down the upper fragment, by strapping and firm bandaging, as near as may be to the lower fragment, and keep it there, by readjusting the strapping as often as it gets at all loose. Many surgeons, after doing all this, proceed further to elevate the whole limb into the air, with the intention of shortening the distance between the origin and insertion of the rectus muscle. I never adopt this practice, because I believe it to be quite useless, and very uncomfortable to the patient. The advocates of the elevation plan defend it by appealing to a constant state of partial contraction of the muscles which is supposed to be mainly instrumental in causing the separation of the fragments, and they assert that by lessening the distance between the ends of the muscle they diminish the consequence of this contraction.

I do not for a moment deny that the origin and insertion of the rectus femoris muscle are brought nearer together by elevating the thigh, or by raising the body ; I assert, however, that the muscle when left to itself is not in a state of constant contraction ; but, on the contrary, that it very soon relaxes completely ; and that, therefore, any arrangement for shortening the distance between its attachments is uncalled for. I repeat, also, what I have already mentioned, that it is not to contraction of the rectus, but to synovial effusion, that the separation of the srrctured portions os bone is due, and that it is, therefore, useless to make any special provision for insuring the relaxation of the muscle. I am even of opinion that elevation of the limb may be injurious, for we place it in a constrained position, and I think that muscles when in positions of discomfort are more likely to take on irregular and violent action than when allowed to rest in their ordinary postures.

A few words as to the mode of repair in transverse fracture of the patella. I am inclined to think that bony union is not so rare as it is supposed to be. I have dissected one specimen of union by bone, and I have seen several cases in which I had not the slightest doubt that bony union had taken place. The last case that I discharged from the hospital was, I believe, one of this kind. I have quite lately seen a gentleman who was treated by his brother, a surgeon, twenty years ago, for fracture of the patella, and whose limb has during that time been in every respect as good as it was before the accident. There is in him a groove across the patella marking the seat of fracture, but, on examination, I felt no doubt whatever as to the repair having been by bone. The most favourable cases, as I have before mentioned, are those in which no effusion occurs, or in which the effusion is comparatively slight, and disappears rapidly, and which are treated by prolonged rest in bed, these being the cases in which there is little, if any, separation of the fragments. When the fragments of the patella are separated widely, and for a long time, then bony union is very unlikely to occur.—*Medical Times and Gazette, August* 19, 1871, *p.* 218.

34.—ON EXCISION OF THE ANKLE-JOINT FOR DISEASE.

By Frederick James Gant, Esq., Surgeon to the Royal Free Hospital.

[By excision of the ankle-joint, the writer means the removal of the articular surfaces of the bones which enter into the formation of this joint—namely, the lower articular ends of the tibia and fibula, and the upper portion of the astragalus.]

This operation, first performed for *injury* by Hippocrates, and revived by Hey, of Leeds, in 1766, was first performed for *disease* by Moreau, senior, in 1792; then by Moreau, junior, in 1796; next by Mulder, in 1810; and probably by Champion, in 1813. But the credit of introducing the operation into British surgery is due to Mr. Hancock, who excised the ankle-joint for disease, in February, 1851. The operation has since been resorted to by Professor Humphry, of Cambridge, in four cases, and by other surgeons, including myself.

Conditions appropriate for excision.—Disease of the ankle-joint, according to Mr. Hancock, frequently commences in the synovial membrane, and extends to the articular surfaces, thus, secondarily, involving the bones. But it may also commence, as scrofulous disease, in the cancellous tissue of the long bones, tibia and fibula, or of the astragalus, or of all three bones. In the former condition, the affected portion of the articular surfaces may be removed with safety and advantage. In the latter condition, excision is not inappropriate.

Other and different opinions are held by some surgeons of repute, as by Mr. Furneaux Jordan, of Birmingham, both with regard to the origin of disease of the ankle-joint and the propriety of excision. That disease rarely extends from the synovial membrane, but begins in the cancellous tissue, either of the extremities of the tibia and fibula, or in that of the astragalus; and, it is said, necessarily affects the whole of that bone. In the one condition, excision of the extremities of the long bones, as the local source of the disease, is said to be inadmissible; while, in the other condition, the whole bone—astragalus—must be excised. The former contra-injunction is plainly at variance with the established practice of excision in disease of the knee-joint, under strictly analogous circumstances—when the end of the femur, or head of the tibia, is the seat of disease. Excision of the extremity of either of these long bones is not inadmissible, but only, perhaps, less favourable in scrofulous disease of the cancellous tissue.

The fair inferences from this diversity of opinion respecting the diseased conditions of the ankle-joint for which excision is appropriate, would appear to be:—

(1.) When disease commencing in the synovial membrane has extended to and destroyed the articular surfaces of the tibia and fibula, that of the astragalus, or of both opposed surfaces.

(2.) When disease, having the same articular consequences, commenced in the cancellated tissue either of the long bones or of the astragalus, provided it be limited to part of this bone—its upper articular portion.

(3.) Whether the disease originated in the synovial membrane or in the articular cancellated tissue, resulting in destruction of the cartilages without anchylosis, excision should be resorted to *before* the supervention of constitutional exhaustion.

Operation.—Hancock's description of excision of the ankle-joint, as first performed by him in England (1851), is as follows:—"I commenced an incision behind, and about two inches above the external malleolus, carrying it forwards beneath that process, across the front of the joint, and terminating about two inches above and behind the inner malleolus. This incision included the skin, without implicating the tendons or their sheaths. The flap thus formed was dissected up, and the peronei tendons were detached from the groove behind the fibula, and cut through, as was the external lateral ligament close to the fibula, with a pair of bone-nippers. I next divided the fibula about an inch and a half above its inferior extremity, and, cutting through the inferior tibio-fibular ligaments, detached the external malleolus. Turning the leg on to its outer side, I cut through the internal lateral ligament, carefully keeping the knife close to the end of the tibia, to avoid the posterior tibial artery. The tendons of the tibialis posticus and flexor communis were then detached from the groove behind the internal malleolus, and, taking the foot in both hands, Mr. Avery holding the leg, I dislocated the foot outwards, thus bringing the end of the tibia with the internal malleolus prominently through the wound. These were removed with a common amputating saw, applied half an inch above the horizontal articulating surface of the tibia, the soft parts being protected by a spatula; the upper articulating surface of the astragalus was also removed by a metacarpal saw, held horizontally. The foot was then restored to its proper position, the cut surface of the astragalus being adapted to the cut surface of the tibia; and the wound having been closed by sutures, except on the outer side, left open for the free escape of discharge, the leg was placed on an external splint, having an opening corresponding to the wound."

The parts divided by this operation were—the skin, peronei tendons, internal and external, lateral and inferior tibio fibular ligaments, and the articular surfaces of the bones. In subsequent operations the tendons were preserved entire. In no instance have the tibial arteries, anterior or posterior, been wounded; and never has it been necessary to apply a single ligature.

Lateral incisions alone, without the anterior communication, have since sufficed. This was my mode of operation in two cases.

The *after-treatment* of ankle-joint excision should be conducted in accordance with the principles laid down respecting the excision of other joints, regard being had to the kind of anchylosis necessary for the use of the limb. The fixed position of the foot must be maintained during the period requisite to secure a firm anchylosis of the osseous surfaces made by excision—the lower ends of the tibia and fibula, and the upper surface of the astragalus; thus to adapt the foot for support and progression. The wounds, one on either side of the ankle, are dressed from day to day, but the splint should be removed and reapplied *very seldom*. In my own excisions of the ankle-joint I have fixed the leg on a flat back-splint, provided with a foot-piece; thus secured, displacement can scarcely occur, and the side wounds are freely accessible for cleansing and dressing. Eventually a starched bandage may be applied; and the foot being slung from the neck, the patient gets about on crutches as the foot is gradually brought into use.

Results.—Excision of the ankle-joint for disease presents some most interesting results, both in relation to the mortality of this operation and the state of the foot, with the probability of secondary amputation; both these aspects of the operation being considered also as compared with amputation of the leg, and with Syme's and Pirogoff's operations of amputation at the ankle-joint, severally, for disease.

For certain valuable statistics bearing on these important questions I am indebted to Mr. Hancock, who liberally placed at my disposal the manuscript of his lectures at the Royal College of Surgeons.

In relation to Mortality.—The results of 32 cases of excision of the ankle-joint for *disease* have been collected by Mr. Hancock; all that he could find recorded in the practice of British surgeons. Of these 32 cases, 7 died—about 1 in 5, or a fraction above 21 per cent. But of the 7 deaths, 4 are reported to have died of consumption, one suffering from that disease at the time of operation; while another died of secondary syphilis. This reduces the average mortality to 1 in 16, or about 6 per cent. Of [illegible] 2 cases, both lived.—Lancet, Aug. 5, 1871.

35.—ON EXCISION OF THE TARSAL BONES FOR DISEASE.

By Frederick James Gant, Esq., Surgeon to the Royal Free Hospital.

Excision of the tarsal bones comprises certain recognised operative procedures which correspond to the lines of the osseous articulations; as removal of the astragalus, or of the os calcis. But the plan and performance of excisional operations on the foot should not be restricted by these anatomical limitations. The modification of these operations on the foot, according to the kind and extent of the disease, well illustrates the guiding principle of excisional surgery; and this application of the general principle I have laid down is specially advocated by Mr. Hancock, in the lectures already referred to.

The *conditions of disease* for which excisional operations on the foot may become appropriate are similar to those which affect other bones and their articulations; namely, the destructive result of inflammation, and especially in the form of caries. According to the extent of such disease, partial or complete excision of any one or more tarsal bones may be necessary. I proceed to describe the various operations, and to estimate their value by their results.

1. *Excision of astragalus.*—*Partial* excision of the astragalus, for disease, was first attempted by Severin in 1646, and in England by Ramsay in 1792. Since that time it has been performed by other surgeons, but in a comparatively few authentic cases. *Complete* excision, for disease, was first performed by Mr. Busk in 1850. Three other cases only have since been recorded in England; one each by Mr. Erichsen, Mr. Holmes, and the late Mr. Statham. An incision along the outer and anterior aspect of the ankle will expose the bone; its neck should then be severed with strong cutting pliers; and, some space having been thus made, the bone may be drawn out of its bed by the lion forceps, the knife being used to detach its ligamentous connexions, but applied cautiously towards the inner side of the joint in proximity to the plantar arteries. It may be necessary to extirpate the bone piecemeal when its substance breaks down in a carious state; it must then be gouged out.

Some of the *anterior* tarsal bones may have to be removed *with* the astragalus. Liston's operation on the foot—as the excision thus extended, in a noted case, might be named—consisted in the removal of the astragalus, scaphoid, and two cuneiform bones. The case is reported in the *Edinburgh Medical and Surgical Journal* of January, 1821.

Results.—Of *partial* excision of the astragalus, in 27 cases recorded, 8 were operations for disease (caries); 5 terminated well; 1 ended in anchylosis, and in 2 the result was not stated. *Complete* excision: In 109 cases of complete excision of the astragalus, 14 were operations for *disease*, 13 being for caries, and 1 for necrosis. Of the 13 cases 1 died, 8 recovered with good and useful limbs, 2 underwent secondary amputation two years after excision, and both recovered; in 2 the results were doubtful. The case of excision for necrosis did well.

(2) *Excision of os calcis.*—*Partial* excision seems to have been first performed for injury, in the removal of a musket-ball from the os calcis, by Formius, as long since as the year 1669.

Complete excision was first performed, and for disease (necrosis), by Mr. Robert, of Prague, in 1837. The second operation, and the first in this country, was by Mr. Hancock, in May, 1848. The first successful case in England was by Mr. Greenhow, of Newcastle, in the same year, 1848; and two equally successful cases of complete excision, in the hands of that surgeon, followed also in that year. Since then the operation has been practised, with varying success, by other surgeons, principally by Sir William Fergusson, Teale (Leeds), Humphry (Cambridge), Cann (Hereford), Pemberton (Birmingham), Potter, (Newcastle), Mr. Holmes, Mr. Erichsen, and by myself.

The operation is thus performed:—The patient lying on his face, with the sole of the foot uppermost, an incision with a stout bistoury may be commenced at the calcaneo-cuboid articulation, just above the sole of the foot, and carried backwards round the heel forwards to an equal point on the inner side. This *sole-flap* of integument is raised forwards from the under surface of the os calcis, *and in its whole thickness down to* the bone, so as to form a good cushion; a slight perpendicular incision is made opposite the tendo Achillis into this horizontal incision, and with a little reflection of the integument to either side the tendon is severed from its insertion. The knife is then entered posteriorly over the upper surface of the os calcis, and the strong interosseous ligament connecting it with the astragalus is divided, somewhat as an oyster is opened. The bone can then be raised, and its lateral attachments being cleared by a gentle application of the knife, the calcaneo-cuboid articulation is opened, and the bone completely detached.

Or, an incision may be made, as for Syme's amputation at the ankle-joint, and a *heel-flap* having been formed, the os calcis is exposed, lateral incisions are continued, on either side, along the sole of the foot to the line of the calcaneo-cuboid articulation, whereby a short *sole*-flap is reflected forwards, and the operation completed as before. The objection to this plan of excision is the liability to sloughing of the heel-flap in disease of the os calcis; fistulous openings frequently having formed posteriorly, where the flap is attached. The advantage of a heel-flap is that the excision can be readily converted into Syme's amputation in the event of the astragalus being found to be extensively involved. This method, therefore, was recommended by Mr. Teale, and practised by Mr. Page, as precautionary proceeding in excision of the os calcis.

Apart from the contingency of sloughing, I prefer the sole-flap method of excision.

3. *Excision of the os calcis and astragalus.*—Wakley's operation is performed much in the same manner as the second method of excision of the os calcis alone. The only differences are, an *additional* incision between the malleoli posteriorly, curving down to the insertion of the tendo Achillis, so as to expose the astragalus on raising this flap of integument, and that the integument of the heel between this incision and the incision across the heel, from malleolus to malleolus, forming the heel-flap in the other operation, is here removed. The bones are excised through the gap thus made posteriorly, by dividing the tendon and lateral ligaments, lateral incisions being continued along the foot to the line of the calcaneo-cuboid and astragalo-scaphoid articulations, so as to give access to these articulations. The malleoli are removed with bone-nippers. The posterior tibial artery must be ligatured, but the anterior tibial is avoided by very cautiously using the knife in removing the astragalus in front of the tibia.

This excision would probably meet the difficulty of disease extending *up* to the ankle-joint, and in such case prove a substitute for Syme's amputation at the ankle-joint. The operation was performed by Mr. Wakley at the Royal Free Hospital, in December, 1847, and the case is reported in the *Lancet* of July, 1848.

Results.—Of *partial* excision of os calcis, in 42 cases collected by Mr. Hancock, 38 were operations for *disease*—25 caries, 12 necrosis with sequestra, and 1 necrosis. Of the 25, 1 died the day after operation of diarrhœa, 14 recovered at periods varying from six weeks to six months, 2 required a second operation, but recovered, and 1 underwent secondary amputation, while of the remaining 7 the result is not stated. Of the 12 cases 3 recovered, 1 required a secondary amputation, and of 7 the result is not stated.

Complete excision: Of 18 cases (the particulars of which are authenticated,) in 14 the disease was scrofulous; and in 11 of these this condition was entirely constitutional, in 1 it is said to have originated from a nail having been run into the heel, and in 2 to have followed sprains. Of the 11 cases, 1 died of diphtheria, 7 recovered, and with perfect use of the limb, while 3 suffered secondary amputation, 2 from recurrence of disease in the remaining tarsal bones, and 1 owing to erysipelas. Of the 3 cases induced by injury, 2 recovered completely, and the result of the third was doubtful.

The *general* results of complete excision of the os calcis are thus represented in 34 authenticated cases: 1 died of diphtheria, 25 recovered completely, 4 underwent secondary amputation, and of 4 the results are not given.—*Lancet, Aug.* 5, 1871, *p.* 185.

36.—ON EXCISION OF THE ELBOW-JOINT.

By Frederick James Gant, Esq., Surgeon to the Royal Free Hospital.

[The elbow-joint appears to be not unfrequently amenable to treatment without operative interference, in the destructive stage of inflammatory disease, by ulceration of the articular cartilages. The natural cure by anchylosis supervenes more readily than in other joints.]

Conditions of Elbow-joint Disease appropriate for Excision.—The three conditions which severally determine the propriety of excision with regard to the joints in general are applicable to the elbow-joint.

1. Functional inutility of the limb, depending on disease of the joint having resulted in destruction of the articular cartilages, without the supervention of anchylosis, will always justify excision; care being taken that the constitutional condition shall *not*, if possible, have approached to exhaustion. But the degree of reserve power requisite for recovery is much less than after excision of the knee or hip, owing to the average period of reparation being less by one-half, or about six weeks instead of three months.

2. Osseous anchylosis, and particularly in connection with a useless position of the limb, will also justify excision.

3. The structural conditions of disease pertaining to the elbow-joint, which specially affect the propriety of its excision, relate to the *extent of bone* destroyed by disease. The limits of excision of the elbow-joint are not restricted by two of the three considerations respecting the knee-joint. Thus, the length of the portions of bone removed from the elbow is comparatively unimportant, the corresponding loss of length in the arm not much impairing the use of this member eventually; nor is it of consequence, therefore, to observe the epiphysial lines affecting the subsequent growth of the bones. But it is equally requisite in the elbow as in the knee to preserve sufficiently wide surfaces for the formation of an adequately secure union; in the one case, with ligamentous mobility; in the other, with osseous consolidation.

The removal of only a thin superficial section of the articular ends of the bones in the elbow-joint, leaving the section ends too nearly in contact, is apt to be followed by osseous union and an unsuccessful result of the operation. On the other hand, any new bone which may not unfrequently have been produced in the form of a spiculated enlargement of the articular ends *above* their diseased portions, and thereby limiting the disease, should not be included in the excision. The importance of observing this limitation of the operation is particularly urged by Mr. Butcher in his work on Operative and Conservative Surgery.

Operation.—Excision of the elbow-joint, for disease, was originally performed by Justamond, of the Westminster Hospital, in 1775, as a partial operation, in which the olecranon and two inches of the ulna were removed. Complete excision was first performed by Moreau, senior, in 1794, and by Moreau, junior, in 1797; but the operation attracted little attention until it was revived by Stansfield, Chorley, and Hey, of Leeds, in 1818-19, and especially by Syme in 1830, since which period it has been more generally practised than the excision of any other joint for disease.

The operation is thus performed:—A single linear incision, longitudinally over the centre of the joint, and of sufficient length to turn out the bones, is preferable to any other, in relation to speedy recovery after operation. Other forms of incision offer certain special advantages. An **H** shape exposes the bones more readily, on reflecting the flaps, thus marked out, upwards and downwards; and this incision is particularly recommended by Sir W. Fergusson. A ⊣ shaped incision, with the vertical line parallel to and a little outside the ulnar nerve, allows of its being more surely guarded or drawn inwards, by a curved spatula in the hands of an assistant. In either case, the transverse line of the incision should be made across the end of the olecranon from condyle to condyle; or nearly to the inner condyle in the latter form of the incision; and thus the joint is laid open. But the single longitudinal incision is now, I believe, generally practised; and I rarely find any other more convenient. With a little detachment of the integument on either side, the knife is entered transversely above the olecranon, dividing the tendon of the triceps (the ulnar nerve being protected); the olecranon process must then be sawn off transversely, and by forcibly flexing the arm, and perhaps dividing the lateral ligaments with a light touch of the knife, the articular surfaces of the three bones are fully protruded and exposed. The articular surface of the humerus between the condyles is excised by means of a small saw; and the remaining sigmoid surface of the ulna and head of the radius in like manner, or removed with pliers, or simply gouged. The latter two bones should not be excised below the insertions of the brachialis anticus and biceps muscles; for thus the brachial artery will be protected by the intervening brachialis muscle from any fair risk of injury in removing these portions of bone. Any surrounding *outgrowth* of bone, resulting from exuberant reparative action, must not be taken for disease; it should not be included in the excision. The line of excision having been closed with sutures, the arm is laid semiflexed on an angular splint; or it may be placed in Mr. Christopher Heath's splint—an apparatus which combines with rest and graduated flexion or extension, graduated elongation of the limb, and entire access to the joint for dressing. To ensure the formation of flexible union, passive motion should be gradually recommenced when such reparation is established; say, in three or four weeks.

Results in relation to life, or the mortality.—Three *general* conclusions may be drawn from the results of one large collection of cases, with those in the hospitals of the United Kingdom, and in the practice of individual surgeons. 1. A lower mortality than that of any joint-excision in the lower extremity; and in the upper extremity also, so far as statistics hitherto collected supply the data for comparison. 2. An average mortality of 1 in 8, or 12 per cent., or even much lower, about 1 in 15, or about 7 per cent. 3. A very different mortality in the hands of individual surgeons.

The conditions of disease, local and constitutional, in the cases selected for operation, would seem to mainly determine this difference; while the mode of performing the operation and the after-treatment would also contribute to explain it.—*Lancet, Aug.* 26, 1871, *p.* 283.

37.—COMPOUND DISLOCATION OF THE RIGHT ELBOW—AMPUTATION OF THE ARM—NEW METHOD OF CONTROLLING HEMORRHAGE.

By Thomas Bryant, Esq., Surgeon to Guy's Hospital.

An old woman, aged 77, who had fallen down upon her right hand on some stone steps, was admitted under the care of Mr. Bryant, at Guy's Hospital, with a compound dislocation of the elbow-joint. On examination, it was found that the ulna was displaced backwards without fracture of the coronoid process, a portion of the inner condyle of the humerus was detached, and there was a lacerated wound along the inner side of the biceps tendon. From the nature and extent of the injury, associated with the age of the patient, Mr. Bryant was induced to amputate at once. This he did by having the arm raised so as to empty the vessels of the limb, by gravitation, as much as possible of blood, and then, without applying the tourniquet, he controlled the brachial artery with the fingers of his left hand while he amputated just below the middle of the arm. The patient is now doing well.

On examining the joint after it had been removed from the body, it was seen that, besides the injuries alluded to, the head of the radius was comminuted and the upper end of the lower fragment or shaft of the bone dislocated forwards, the uneven end of which having been pushed inwards as well as forwards, had, no doubt, at the time of the accident produced the wound in the skin and soft parts on the inner side of the biceps. Instead of securing the artery by ligature or by torsion, which is the method chiefly employed at this Hospital, Mr. Bryant used an instrument which had been brought to his notice by Dr. Fleet Speirs, *of New York.* This instrument is called the artery constrictor, and consists of a flattened metal tube, six inches (more or less) in length, open at both ends, with a sliding steel tongue running its whole length, and having a vice arrangement at one end and a hook-shaped depression at the other, by which the artery can be grasped. It is then made to contract upon the artery by means of the vice at the upper end, which forces it within the sheath. In principle its action is the same as that of torsion, but the integrity of the external coat is more thoroughly preserved, while the continuity of the vessel is maintained.

Mr. Bryant informs us he had made some satisfactory experiments with the constrictor previous to using it upon the case now described, and he is of opinion it will be of good service in cases where torsion is impracticable—as, for instance, in the cure of aneurism.—*Medical Times and Gazette, August* 12, 1871, *p.* 188.

38.—ON EXCISION OF THE WRIST.

By Frederick James Gant, Esq., Surgeon to the Royal Free Hospital.

Excision of the wrist comprises, properly speaking, not only the removal of the articulatory portions forming the radiocarpal articulation or wrist-joint, but also the carpus and bases of the metacarpal bones; this extent of excision differing from that of the analogous operation on the ankle-joint, which is restricted to the removal of the articulatory portions of the tibia and fibula, with that of the astragalus.

Conditions of wrist-disease appropriate for excision.—Partial or *com lete* disease of the wrist cannot be referred, like that of the larger and more simple joints, to an independent origin, either in the synovial membranes—three in number—or in the bones of the wrist. The lower articular end of the radius and that of the ulna, which in relation to excision is associated with the wrist—the carpus below, consisting of eight bones, in two ranges of four in each—and the bases of the five metacarpal bones, are all so contiguous as to obscure the precise seat of origin in caries affecting the wrist. Then, again, the three synovial membranes are as one, in relation to the origin of disease in the form of synovitis: the membranous investment of the radio-carpal articulation, which sometimes communicates with that of the radio-ulnar articulation through a perforation in the intervening triangular fibro-cartilage; the investment between the two ranges of carpal bones, with its two prolongations upwards, and sometimes extending into the synovial membrane of the radio-carpal articulation, and three prolongations downwards which always extend to and invest the four inner carpo-metacarpal articulations; and the separate synovial sac for this articulation in the thumb.

Caries of the wrist appears to be generally of scrofulous, and thence constitutional, origin—excited, perhaps, by some injury of apparently trifling character, as a sprain; while synovitis seems to have another constitutional origin, probably as chronic rheumatism.

Thus we recognise as conditions of disease for excision:—

(1.) Scrofulous caries of the wrist, often involving the lower articular end of the radius and ulna, the whole of the carpal bones, and the bases of the metacarpal bones, in a state of extensive caries.

(2.) Chronic synovitis, of perhaps more limited extent, but leading to caries and destruction of the articulations.

The *amount of bone* to be removed, as being apparently diseased, may vary according to the character and extent of the disease. Thus excision might include the lower ends of the radius and ulna, with the adjoining carpal bones—in the wrist-joint, or the bases of some or all of the metacarpal bones. But Professor Lister insists on the complete extirpation of the wrist in *all* cases, from the lower ends of the radius and ulna to the bases of the five metacarpal bones inclusive; the disease, however limited it may appear, being apt to recur in the articulatory portions left by a partial excision.

Operation.—Excision of the wrist was originally performed by the younger Moreau, at the close of the last century; subsequently, by a German surgeon, Dietz, in 1839; and then again by Heyfelder, of Erlangen, in 1849; but, in this country, the operation was revived by Sir Wm. Fergusson, in August, 1851. Since that period it has been resorted to by Mr. Simon, Mr. Erichsen, the late Mr. Stanley, and Mr. Butcher of Dublin; and practised especially by Professor Lister of Edinburgh, who has devised a particular method of operation for complete excision of the wrist.

Partial excision consists in the removal of only one or two of the carpal bones, or other limited portions of the bones forming the wrist. This procedure can be readily effected by slitting up any fistulous aperture leading to the carious bone, and extracting it by bone-nippers and forceps.

Complete excision may be performed in either of three ways. The choice of method is mainly determined by the consideration of difficulty in removing the affected bones without dividing the extensor tendons of the fingers and thumb, the supinator tendons, radial and ulnar extensor tendons, inserted into the bases of metacarpal bones, being comparatively unimportant, in consequence of the firm fibrous anchylosis of the wrist after operation, if the result be successful.

First method.—A curvilinear incision is made, extending from just above the styloid process of the radius, downwards across the back of the wrist, and upwards to the same level above the styloid process of the ulna; the flap of integument is reflected, carefully avoiding the extensor tendons of the fingers, and those of the thumb, on the ulnar half of the radius. Then, dividing the supinator tendons, and the extensor tendons of the carpus, and flexing the wrist, the radio-carpal articulation is opened; and while the other extensor tendons referred to are drawn aside with a curved spatula by an assistant, the articular ends of the radius and ulna, the carpal bones, and bases of the metacarpus, are successively removed by a small saw or cutting pliers introduced transversely.

Second method.—Two *lateral* longitudinal incisions are made, one on the ulnar, the other on the radial side, of the wrist, or on its *dorsal* aspect, thus readily avoiding the extensor tendons of the fingers and that of the first joint of the thumb. The operation is then continued as before, and completed by excising the bones in the same manner.

A *single ulnar* incision is deemed sufficient by Sir W. Fergusson, and is the best mode of operation.

But Prof. Lister has particularly pointed out that there are two obvious and important objections to the lateral method, which somewhat resembles that which he has devised. First, the radial incision is so placed as probably to sacrifice the extensor tendons of the metacarpal bone, and of the second joint of the thumb. Secondly, with regard to the bones, that in the transverse division of the bones an unnecessarily large amount of bone is removed from the radius and ulna, and from the metacarpus—a loss of length and breadth which interrupts the process of consolidation, and results in a more narrow wrist and impaired strength of the hand. Moreover, the bones being divided in the dark, that some portion of the disease may probably be left behind.

To obviate these difficulties as to the excision of the bones, and to avoid the tendons requisite for the efficient use of the hand and fingers, another method of operation has been proposed and practised by Professor Lister. It consists in two essential peculiarities; the radial incision is so placed on the dorsal aspect of the radius, as to avoid the tendons which are otherwise liable to be implicated—the extensor ossis metacarpi pollicis, and the extensor secundi internodii, while the limited but complete excision of the bones is accomplished by first removing the carpus, and then the articular ends of the radius and ulna and the bases of the five metacarpal bones. The hand is placed on a suitable splint, extending up the forearm and secured by a bandage. The most convenient form is an ordinary wooden splint, with an obtuse-angled piece of thick cork cemented to the palmar portion by means of fused gutta-percha, and with a bar of cork stuck on transversely to the under surface of the splint, so as to project at the side. Lister's splint, thus constructed, possesses certain important advantages. The hand lies semiflexed, which is its natural position of repose. The fingers are midway between flexion and extension, into which it is necessary to bring them by daily passive movements, while a certain range of voluntary motion is also permitted, which the patient should be encouraged to exercise frequently during the day. Then, again, this position is best adapted for allowing the extensors of the carpus to acquire fresh attachments ; and, the palm resting on the sloping surface of cork, the splint cannot slip upwards nor downwards, secured by turns of the bandage around the transverse bar of cork. This appendage to the splint specially keeps the *thumb* in position ; it is thus allowed to fall below the level of the rest of the hand, so as to be most serviceable for opposition to the fingers, while the tendency to adduction of the thumb towards the index finger is prevented by a thick pad of lint placed in the angle between the two, under the turns of bandage around the transverse bar of cork. The palmar piece of cork should be hollowed out to receive the ball of the thumb.

After-treatment.—Two principles must be kept in view, in order to obtain a successful result. Firstly, to procure firm anchylosis of the wrist by retaining it in a fixed position, during the process of consolidation, for a period averaging six or seven weeks. Secondly, at the same time, to maintain the flexibility of the fingers and thumb daily ; commencing flexion on the second day, whether inflammation has subsided or not. In executing these movements, each finger should be both flexed and extended to the full degree, while the connected metacarpal bone is held quite steady, so as not to disturb the wrist. Both these principles are provided for by the peculiar construction of the splint ; and Professor Lister attaches more importance to their fulfilment, in the after-treatment, than to his method of operation.

Pronation and supination, also, must not be long neglected : and as the new wrist acquires firmness, flexion and extension, abduction and adduction, should be occasionally encouraged. The period during which passive motion should be practised may be resolved into this rule: it must continue until the disposition to contract adhesions finally ceases—a few weeks or a few months.

When the patient leaves his bed, and carries his arm in a sling, the weight of the hand will make it gradually droop to the ulnar side—a tendency which is best counteracted by affixing two ledges of gutta-percha to the ulnar side of the splint, one to support the border of the hand, and the other to prevent any lateral shifting of the splint. As the hand acquires strength, more free play for the fingers should be allowed, by cutting away the splint up to the knuckles, leaving only the palm supported. Some support must be continued until the patient feels the wrist as strong without it as with it. Earlier disuse of this support would assuredly undo the work of previous management, and lead to an unsuccessful result. A year, or even two, of such finishing-off support may be required to gain the most useful hand.—*Lancet, Sept.* 2, 1871, *p.* 314.

39.—A FEW PRACTICAL OBSERVATIONS ON THE TREATMENT OF WOUNDS.

By Dr. John Swift Walker, of Hanley.

[The treatment recommended by Dr. Walker is a modification of the antiseptic plan.]

First, take a simple incised wound of the scalp ; after shaving the surrounding margin insert a suture (if large, a *sine quâ non*). In this class, freely apply Richardson's styptic colloid, and over this a pad of carded wool. In a few days the application of the solution will be again required, and a fresh piece of wool.

For simple incised wounds Richardson's solution is a very valuable addition to our Materia Medica, but only to be used as collodion or plaster to hold the two surfaces of a wound in position. It promotes union by the first intention, and if it does not so unite, the wound is not so gaping nor has it so large a cicatrix.

The carded wool or marine lint.—I have tried several kinds, but none seem to fulfil the object for which it is used so well as that obtained from Messrs.

Wood, of Manchester, made by Mr. Westhropp, of the Falcon Works, London. But more of its especial use hereafter.

Take now an amputation of arm or leg. After the operation, my plan of treatment is as follows:—Sponge out the wound thoroughly with chloride of zinc ʒ ss to ℥ j of water, then let all applications be quite dry for the first two or three days; as soon as there is the least secretion of pus, take a Higginson's syringe, and wash out the flaps with a strong solution of Condy's fluid, say ℥ j to ℥ x of water; apply strapping, and cover the wound with a small piece of lint wetted with a lotion of ʒ ij carbolic acid, ʒ ij liq. potassæ to six ounces of water; cover with a piece of gutta-percha tissue, and continue until healed, surrounding the edges or flaps with a little pad of carded lint.

Take another every-day case—an ulcer of the leg. If it is a large flabby ulcer, secreting a large quantity of pus, nothing is so good for compressing the granulations and checking the secretion of pus as the marine wool; but after it has fulfilled its object, a change of treatment, by the application of either a little dry precipitated chalk, zinc ointment, or zinc lotion, will heal it in a few days with rest and bandaging.

The marine lint is a very excellent application for a sinus after the removal of necrosed bone, and facilitates their closing with very little secretion of pus. Its tarry smell seems, in my opinion, to disinfect all ulcerating surfaces, but the application must not be continued too long, as the wool seems to contract the granulations, if such a term may be used.

I do not altogether agree with Mr. Lister as to the germ theory, but he has done good by teaching us to wash all wounds under our care with some disinfectant at each dressing; and every practitioner who will adopt this plan of treatment will be pleased at their satisfactory progress: and yet not a mere washing—it must be a stream of fluid. Any stump, after amputation either of the leg, arm, or finger, granulates very fast under this mode of treatment.

In my opinion, all Mr. Lister's solutions of carbolic acid are too strong,—and abrade the cuticle surrounding the wound; it may be through my clumsy mode of application, or not following the minute directions laid down. Thus far I can admire his treatment, that it teaches us all that we disturb the approximating surfaces of wounds too often very unnecessarily, and thereby retard nature's efforts.

Since writing the above, my attention has been directed to a paper in the *Lancet* of the 13th May, on the use of a similar article, "Tenax." This I have not had as yet; but doubtless it produces the same therapeutical effects as the carded wool. Its chief advantages seem to arise from being so porous as to soak up all discharges, and the tar or ingredients of the tar, probably the creosote, so neutralises and changes the character of the discharge that it becomes quite innocuous: for we see the same effect in an injection of creosote in corroding ulcer of the uterus or in cancer. But a creosote lotion has not the same effect upon open wounds.

My chief motive in writing this paper is to direct the attention of my professional brethren to the use of some preparation as the carded wool, as its therapeutical effects are only known to a few, and these highly appreciate its use.—*Practitioner, Oct.* 1871, *p.* 210.

40.—M. GUERIN'S NEW METHOD OF DRESSING WOUNDS.

During the siege, the Paris surgeons were much grieved and disappointed at witnessing the utter failure which attended the attempts at amputation and disarticulation, or the liberal use of the knife in any manner. In such cases a fatal result was almost sure to ensue, whatever the mode of dressing employed; and the surgeon was at a loss to discover what means to resort to for saving the patient's life. Attempts at conservative surgery were crowned with much comparative success; still the general results were very unsatisfactory, and the wits and abilities of the Paris surgeons were stretched to the utmost in their search after some safe mode of proceeding. It was quite obvious that the unsatisfactory character of all general hygienic conditions—the want of proper food, the moral state of the patients, depressed and disturbed by the events and emotions of the time, and the bad condition of the atmosphere—combined to entail this heavy mortality. The character of the wounds was also far more serious than had ever been observed under like circumstances; and the injuries inflicted by the cone-shaped bullet (tearing up the soft parts, splintering the bone in all directions, and causing extensive contusion of the marrow) necessarily brought on most disastrous consequences. The results of private practice were scarcely more favourable than those obtained in the wards of the hospitals; and this was observed to be the case as much during the reign of the Commune as during the German siege.

Towards the end of the Commune *régime* M. Alphonse Guérin, who had been previously much struck by the statements advanced by Pasteur, Tyndall, and others, in regard to the dust and germ theory of disease, bethought him of applying a new mode of dressings founded on these doctrines. The starting-point of his inferences and experiments was simply the filtration of dust and germs through cotton, as illustrated by Tyndall's experiments. His first attempts were attended by moderately favourable results, but he has since completed and perfected his system with a success which deserves the attention of surgeons.

The *modus operandi* is extremely simple. It consists, if we take the dressing of an amputation for example, in wrapping the stump round and round with successive layers of cotton. A liberal use of the substance must be made, and several yards of cotton wool successively disappear around the limb. It is quite obvious—to speak in the words of the surgeon of the Hôpital St. Louis—that this thick interposition of clothing is requisite to filter the air before it reaches the wound. It is not the less necessary to extend this dressing in all the directions of the limb, as foul or unfiltered air might find its way to the injured part. Thus, in amputation of the thigh, thick layers of cotton-wool are carried up to the hip and round the waist and the nates, so that all the approaches to the wound are carefully guarded in every direction. Coupled with this, M. Guérin exerts a gentle pressure over all the enclosed parts by means of ordinary cotton bands—a point to which he attaches great importance, as it enables him to tighten the whole appliance and to keep the parts snugly together.

This constitutes the entire proceeding. It may be observed that M. Guérin does not use carbolic acid in any way whatever. The wound is simply washed with camphorated alcohol after the operation. The surgeon's hands, the sponges, or instruments undergo no kind of preparation before the operation. The cotton-wool and cotton bands are steeped in no fluid.

M. Guérin lays great stress on the importance of carefully watching this dressing, which it is interesting to observe is intended to be a permanent one. He watches the dressing day by day, and never takes it off unless some extraordinary circumstance occurs, but contents himself with adding fresh layers of cotton-wool if he observes that it is in any way disturbed in such a manner as to permit of the introduction of unfiltered air into the wound. Thus the dressing may remain *in situ*, and does so in the great majority of cases, for twenty-five or thirty days. On removing the dressing after this lapse of time a healthy granulating surface is discovered, and half a wine-glassful of healthy pus is found within the folds of the cotton. It may here be stated that M. Guérin, on applying a first dressing, stuffs up the stump with cotton-wool, which he introduces beneath the flaps. The process of granulation gradually drives out the cotton, and cicatrisation takes place perfectly. Moreover, this mode of dressing may be applied, and has been with great success, to extensively abraded and burnt surfaces.

Such are the details of M. Guérin's proceeding; and the results which have attended it are deserving of much attention, if we consider the mortality which habitually follows such operations in Paris, and which was especially formidable during the two sieges, and if we also take into account the fact that almost all M. Guérin's operations were for gunshot injuries.—*Lancet*, Sept. 2, 1871, *p.* 322.

41.—MICHEL'S PROCESS FOR REMOVING EXTERNAL TUMOURS.

By Dr. William A. Bell, M.A., Cantab.

A little pamphlet gives an interesting account of the mode of operation for the removal of tumours practised by a French charlatan, for a knowledge of which Dr. Bell paid no less a sum than 25,000 francs, and which, having now obtained complete information, he has very properly and liberally made public. The preparation used in all cases where the tumour can with safety be reached externally is made in the following way. Asbestos, as soft and free from grit as possible, is reduced by rubbing between the hands to the finest possible fleecy powder; it is then mixed thoroughly with three times its own weight of strong sulphuric acid ($S\,O_3\,H\,O$). A mass is thus formed which may be easily worked with a silver or gold spatula into any size or shape, corresponding to the tumour to be destroyed. Any malignant growth of the breast which is detached and solitary, with the subaxillary glands unaffected, is suitable for treatment, whether open or not makes no difference. In the application of the caustic the adjoining healthy parts of the skin are carefully protected by applying a zone of collodion and pads of linen, and the patient is so placed that the surface of the tumour is perfectly level. The saturated acid asbestos is then laid on the surface to the thickness of half an inch for a tumour the size of a hen's egg. Rapid destruction of the tissues follows, with, after the first half-hour or so, but little pain. An oozing of clear watery fluid appears, which must be carefully sopped up. After twelve or fourteen hours' action, the first application is to be removed, and a new portion of smaller size adapted to the sore. After this has been applied for twelve hours, the operation is complete, and the healing of the deep excavation alone requires to be attended to, for the details of which we must refer our readers to the pamphlet. Dr. Bell does not pretend to say that this mode of operation will effect a permanent cure of cancerous cases, but he thinks that the plan presents various and considerable advantages over extirpation by the knife, as in producing much less shock to the system, in removing the tumour alone with but little of the surrounding breast, and in postponing, in malignant cases, for a longer period the recurrence of the disease.—*Practitioner, June* 1871, *p.* 377.

42.—ON A PECULIAR FORM OF WRIST-DROP FROM PARALYSIS OF THE MUSCULO-SPIRAL NERVE IN FRACTURE OF THE HUMERUS.

By Professor Erichsen, Senior Surgeon to University College Hospital, and Holme Professor of Clinical Surgery.

Simple fractures of the long bones are seldom accompanied by any serious complications; the vessels and nerves of the limbs being so situated, and being so well protected by the interposition of a layer of muscle between them and the bones, as to escape being injured, in the vast majority of cases, by the ends of the fractured fragments. There are, however, two exceptions to this general rule: one in the upper, the other in the lower extremity. In the upper extremity, in the relation of the musculo-spiral nerve to the shaft of the humerus; in the lower, in the position of the posterior tibial artery in reference to the upper end of the tibia. In both these instances the position of the nerve and of the vessel is such that it may be seriously injured by fracture of the contiguous long bone.

It is not my intention to speak of the injuries that the posterior tibial artery may sustain in and from simple fractures of the bones of the leg; but I shall confine my observations wholly to lesions of the musculo-spiral nerve from fractures of the humerus.

The complication of an injury of the musculo-spiral nerve, in a case of simple fracture of the humerus, must certainly be of rare occurrence, as I find no mention made of it in the standard works on surgery that I have had an opportunity of consulting, and I do not recollect to have met with this accident until recently. We have, however, had lately three cases under our care here in which, in consequence of injury to the musculo-spiral nerve, or one of its branches, more or less complete paralysis of the muscles supplied by that nerve has resulted. These cases I will presently relate to you, and on them I will found the observations I have to make on this interesting subject.

When we observe the manner in which the musculo-spiral nerve winds closely round the back of the shaft of the humerus in its flattened groove, and how in its course downwards towards the forearm it comes into tolerably close relations to the outer condyle, we can easily understand how, in fractures of the shaft, the main trunk may be implicated, and in those of the condyle one or other of its chief divisions injured.

When the main trunk of the musculo-spiral nerve is injured to such an extent as to induce complete paralysis of it, both the supinators of the forearm, and all the extensors of the wrist and fingers lose their power, and the patient becomes utterly incapable of performing those movements that are dependent on the action of these muscles. The hand consequently falls into a state of pronation and flexion, presenting the characterisic signs of "wrist-drop."

When the posterior interosseous division of the musculo-spiral is the nerve that is injured, the loss of supination and of extension is not so complete. The supinator longus and extensor carpi radialis longior, being supplied by branches from the main trunk, are not paralysed, and thus a certain, though very limited movement, in the sense of supination and extension, is preserved, although the forearm and hand fall naturally into a state of pronation and flexion.

These different conditions were well illustrated in the following three cases, which I will relate to you in the order of their degrees of extent and severity, so far as regards the paralysis of the various sets of muscles. An attentive study of the movements of the wrist, hand, and fingers, in these cases, throws a clear light on the actions of the muscles supplied by the musculo-spiral nerve and its branches, and some of the other nerves of the forearm and hand.

Case 1.—*Fracture of Shaft of Humerus: paralysis of trunk of musculo-spiral nerve, and complete loss of extension of the wrist, fingers, and thumb; loss, nearly complete, of supination.*—E. L., aged 29, by occupation an ironer, admitted Dec. 16th. Ten weeks since the patient fell and fractured her humerus about the middle. She was treated as an out-patient, and the limb put upon an angular splint, so as to fix the elbow-joint. When the splint was left off, at the end of four weeks, she was directed not to use her arm for a week. She noticed wrist-drop when she left off the splint, but thought it was mere weakness. At the end of a week, however, on trying to use her hand, she found that she had no power in the wrist or fingers. It was thought to be weakness, and she bathed it with cold water. The hand has for some time felt much colder than the other. On examination, it is found that there is a marked wrist-drop, with pronation. She cannot extend the hand at all. The right forearm, hand, and fingers are swollen. She feels a difference in the temperature of the two hands, but not so great as formerly. Occasionally she has a feeling of pins and needles all down the hands and fingers. The thumb and index-finger are numb on the dorsal aspect, especially the former. Sensation over them is imperfect. Flexion of the fingers is imperfect owing to stiffness of the knuckles. The temperature of the affected hand does not raise the index of clinical thermometer to 85°; that of the other is 90·6°. All the muscles supplied by the musculo-spiral nerve are paralysed completely. She has consequently totally lost all power of extending the wrist. She has no power of extending the thumb. She has no power of extending the fingers from the metacarpo-phalangeal joints; but when the fingers are completely flexed she can extend the joints between the first and second and second and third phalanges. This is evidently accomplished by means of the interossei and lumbricales, which are attached to the expansion of the extensor tendons on the dorsum of the fingers lower down than the metacarpo-phalangeal joints. On holding the index-finger forcibly down, and telling her to try to extend it, the thumb is drawn in towards the palm by the attachment of the first dorsal interosseous to the metacarpal bone of the thumb. Supination can be performed apparently in a very feeble and imperfect manner, but only when the forearm is flexed. *Flexion of wrist, hand,* and fingers is perfect.

Since this report she has been galvanised by faradisation regularly, and has regained considerable power in all the affected muscles, so that she can now extend the wrist and the fingers from their metacarpo-phalangeal articulations.

There are several points in this case that deserve special attention. That the trunk of the musculo-spiral nerve was paralysed by being implicated in the fracture which occurred in that part of the humerus round which it winds, there can be no doubt. All the muscles connected with the hand and wrist that are supplied by both of the terminal divisions of that nerve—the radial and the posterior interosseous—were paralysed, and none other were affected.

Now let us examine a little more in detail the condition of the hand and fingers. The wrist-joint was flexed, so that the hand hung listless and inactive, at nearly right angles with the forearm. It could not be raised or extended in the slightest degree. No effort that the patient made in this direction was of any avail. There was, consequently, complete paralysis of the two extensors of the wrist—the long and the short. The hand was prone; complete supination was impracticable, and no movement whatever in that direction could take place when the forearm was extended on the arm. But when the forearm was flexed, a slight supine movement could be made by the patient. To what was this due? Clearly not to the supinators, which would have acted equally well whatever the position of the elbow might have been. But apparently the slight effort at supination, for it was really nothing more, was the result of contraction of the biceps, which, as you are aware, when called into action whilst the hand is prone, has for its first effect a tendency to supinate the forearm and hand. There was, consequently, complete paralysis of the two true supinators—the long and the short. Thus there must have been loss of innervation in both the terminal branches of the musculo-spiral—the radial and the posterior interosseous. The long extensor of the fingers was paralysed, so that when they hung at right angles with the hand, they could not possibly be extended from the metacarpo-phalangeal articulations so as to be brought to a level with the dorsum of the hand. But there was one movement of extension still left to the fingers, and it was this—that when they were bent or drawn into the palm of the hand, the last two phalanges could be extended, and with some little force, from the articulations between the first and second phalanges. Now this is an extremely interesting point, and one to which I would direct your close attention. To what is this upward or extending movement of the two terminal phalanges due? —a movement that takes place independently of the action of the true extensors of the fingers. It is due to the combined action of the interossei and lumbricales —muscles that do not receive their innervation from the musculo-spiral, but chiefly from the deep branch of the ulnar; all the interossei and the two innermost lumbricales receiving their nerves from this source, whilst the two outermost of the lumbricales obtain theirs from the median. Thus these accessory muscles of extension, receiving their nervous supply from sources that were uninjured, continued in the free exercise of their special actions. The thumb was drawn in towards the palm, and could not be abducted owing to the paralysis of the extensor muscle of the metacarpal bone. The numbness and referred sensations occupied those portions of integument that were supplied by the terminal branches of the radial nerve. The temperature of the hand was considerably lower than the other. It was below the lowest marks (85° F.) on the clinical thermometer; and as that of the sound hand was 90·6°, there must have been a difference of at least 5° to 6° F. between the two limbs.

The next case that I will relate to you is one in which the paralysis seems to have been limited to the posterior interosseous nerve. It is as follows:—

Case 2.—Wrist-drop following Compound Fracture of the External Condyle of the Humerus; paralysis confined to the muscles supplied by the posterior interosseous nerve.—H. E., aged 30, by occupation a lace-cleaner, on the 17th of November slipped down on her right elbow on the pavement, and afterwards came to the hospital. On examining the elbow, much mobility and crepitus were found, and a fracture of both condyles, with separation, could be clearly made out. At the back of the arm, about an inch above the elbow, was a wound which would take the tip of the little finger, clean cut without bruising of the edges, apparently done by protusion of bone at the time of the fall. A probe could pass in different directions readily among the tissues. From Nov. 17th to Dec. 21st the limb was kept on an angular splint at the inner side, and the wound treated in strict accordance with Lister's rules for antiseptic dressing. On Nov. 24th (eighth day) there was much swelling, with some redness and tension, about the joint and upper half of the forearm. It was thought that fluctuation existed; and an incision, about three-quarters of an inch long, was made a little below the elbow. Much serous fluid came but no pus. Large quantities of serous fluid continued to come from the wound for a fortnight afterwards. The tension rapidly disappeared: no pus was at any time observed. Passive motion was commenced on Dec. 21st. The splint was left off on Dec. 23rd. There was much stiffness of the joint.

Jan. 23rd. Patient can bend the elbow to an angle of 45°; can straighten to about a right angle and a half. Can close the hand as far as to bring the tips of the fingers to about an inch from the palm; her hand can be made to close completely without much difficulty. There is perfect flexing and opposing power in the thumb. When the hand and forearm are supinated the wrist is quite straight. Cannot completely straighten the hand at the metacarpo-phalangeal joint. Very slight force brings these joints straight; but she cannot extend the fingers. When the hand and forearm are pronated there is a complete drop of the wrist. Cannot raise wrist or fingers. Cannot move the thumb outwards or backwards (through loss of power of the extensor.) There is almost perfect power of supination when the elbow is fixed. When the fingers are completely flexed, she is able to extend the joints between the second and third and first and second phalanges, by means of the lumbricales and interossei. When doing so, as the hand is very thin, the interossei can be seen working. There is slight numbness on the back of the thumb and index finger, but no absolute loss of sensation.

Now this case closely resembled the last in all respects, with the sole exception of the paralysis of the hand and arm not being so complete. The general aspect of the limb was the same. The pronation and flexion of the forearm and wrist were marked; but the power of supinaton was not completely lost—in fact, existed to a considerable degree, but was not perfect. So also with regard to extension of the hand from the wrist. The knuckles could be brought up nearly to their proper level. Now this imperfect power of supination and of extension of the hand was doubtless due to the supinator longus and extensor carpi radialis longior—muscles supplied by the radial nerve—retaining their power, and thus being able to act; whilst the short supinator and the short extensor of the wrist, both supplied by the posterior interosseous nerve, were completely paralysed. Hence the imperfection of the supination and extension that existed. There was further proof of the fact of the radial nerve having continued to maintain its action in the fact that sensation was not lost in its terminal cutaneous branches. The temperature of the hand also had not fallen, as in the first case.

Case 3.—*Fracture of Lower Epiphysis of Humerus*; *wrist-drop from paralysis of posterior interosseous nerve; tonic contraction of flexors.*—M. M., aged seven, was admitted as an out-patient under Mr. Heath, and by him transferred to Mr. Erichsen. In June, 1870, she fell over a croquet hoop, and the lower part of the right humerus was fractured. The arm was at first supposed by her friends to be dislocated, and a non-medical gentleman who was present pulled violently at it for some time, but, as he did no good, she was taken to a medical man. Splints were used for seven weeks; they reached to the tips of the fingers, which were kept extended. When the splints were left off the fingers became flexed at once. Her parents think they were more so than now. She can crotchet with the right hand, and can write, but badly; she has been learning to write with the left hand in consequence. She has very marked wrist-drop; she can, however, easily extend her wrist. Her hand is pronated, and can only be imperfectly supinated. The fingers are flexed and drawn into the palm of the hand. On the wrist being dropped, the last two phalanges of the fingers can be imperfectly extended by the patient. On the wrist being straightened, the fingers become flexed, and cannot be extended actively or passively. On forcibly extending the fingers *and wrist*, there is no tension of the palmar fascia, but there is great tension of the flexor tendons above the wrist. The hand is congested and cold. The arm is distinctly smaller than the other. The temperature of the right palm is not high enough to move the index of the thermometer —so that it is below 85°, that of the left being 93·6°. The sensibility of the hands was tested by the compasses; it seems quite as acute in the right as the left. There is some irregularity of the lower end of the outer condyle, which seems to have been the situation of the fracture, or separation of the epiphysis, which was the original injury.

Dec. 19th. Mr Erichsen ordered a splint to be specially constructed so as to permit of gradual extension of fingers by means of a moveable hand-piece worked by a rack and pinion.

Jan. 4th, 1871. Mr. Erichsen ordered her arm to be faradised daily. There was at first scarcely any contractility perceptible in the extensors.

9th. the splint has been applied, and the arm has been galvanised daily. The contractility of the extensors and supinators has markedly increased.

23rd. The fingers have become sore from the pressure of the instrument. The index finger is but little improved. The middle finger is better, and the little and ring fingers are very greatly so, being now almost straight. The wrist can be extended perfectly, so that the knuckles can be brought to a level with the back of the forearm. When so extended, the fingers are half bent. But when the wrist is dropped they can be extended by the patient. In doing this they always involuntarily spread out in a fanshape, owing to the action of the dorsal interossei.

It would appear that the chief resistance to proper extension was due to the contraction of the flexor carpi radialis and the flexor tendon of the index finger, both of which are very tense. Mr. Erichsen proposed to divide these subcutaneously ; but the child's friends would not give their consent, and the patient was consequently discharged from the hospital.

In this case, also, we had the partial loss of supination and of extension dependent on paralysis of the posterior interosseous nerve ; whilst those movements that were due to the interossei and lumbricales were perfect. The contraction of the flexors, which had become very marked, was apparently due to the loss of action of their antagonist muscles. It was most marked in the flexors towards the radial aspect of the forearm, and was also associated with a tonic pronation of the limb. The muscles thus injuriously affecting its movement had apparently undergone some rigid atrophy ; and I regret that the child's friends would not allow tenotomy, as it offered a good prospect of cure. The fall in the temperature of the hand was very marked. It amounted to at least 8½° F., and how much more it was impossible to say, owing to the marking of the thermometer not admitting of a wider range being noted. But the difference between the two hands in this respect was most obvious and very sensible to the touch. This great fall in the temperature of the hand is very remarkable when we reflect that it was due to paralysis of a branch of the musculo-spiral which is not directly distributed to the hand ; whilst the other nerves of the hand—the median and ulnar and cutaneous branch—were intact, and gave no evidence of paralysis. The movements of the muscles of the hand itself, and the sensibility of the skin covering it, were normal.—*Lancet*, *July* 1, 1871, *p.* 1.

ORGANS OF CIRCULATION.

43.—ON ACUPRESSURE.

By William Pirrie, Esq , F.R.S.E., Professor of Surgery in the University of Aberdeen.

[The late Sir James Simpson possibly thought correctly when he said " the invention of acupressure will one day do more for my fame than even the discovery of chloroform." At his last interview with Sir James Simpson, Mr. Pirrie stated his belief to him that the more general adoption of acupressure would be greatly accelerated by its friends recommending *three* methods *only*, and by their agreeing to distinguish them not by the old numbers, which would be no longer applicable, but by a new momenclature. Sir J. Y. Simpson highly approved of these proposals, and on the following day sent for consideration the names hereafter mentioned.]

Three modes only of acupressure are required, and it was resolved by the late Sir James Y. Simpson and myself to designate them by the names of Circumclusion, Torsoclusion, and Retroclusion. In arresting hemorrhage by circumclusion, an acupressure pin and a loop of inelastic iron wire are employed, whereas for the performance both of torsoclusion and retroclusion a pin only is required.

I prefer the pins suggested by myself to any others ; and those who are desirous of using them will have no difficulty in obtaining them, and the proper kind of inelastic iron wire, from Messrs. Weiss, Strand, or from Messrs. Coxeter, Grafton-street East, London.

Circumclusion.—As the metal goes round the vessel, this mode of arresting hemorrhage is called circumclusion. It consists in entering a pin in the tissues a line or two to one side of the artery to be circumcluded ; pushing it behind, and causing its point to emerge a little beyond the vessel ; passing a loop of inelastic iron wire over the point of the pin, bringing the wire over the track of the artery and behind the stem of the pin, drawing it sufficiently tight to close the vessel, which is proved by the arrest of bleeding, and fixing it by a single twist around the pin. This description will make the mode of performing circumclusion sufficiently clear, and the *rationale* of its operation in arresting hemorrhage perfectly intelligible.

In my experience of acupressure, no fact has struck me more than the slight degree of pressure required for the perfect arrest of circulation through an artery, provided the pressure be direct and continuous. In performing circumclusion, it is important to have this fact impressed upon the mind, so that unnecessary and injurious constriction may be avoided. Of all methods of acupressure this is the best. It is perfectly reliable, it is most generally applicable, it may be performed in an amazingly short time, and, like the other methods, in ordinary circumstances, without the aid of any assistant. When an artery is situated in a confined corner of a wound, or when cut short in a perpendicular surface, by taking hold of the mouth of the vessel with an artery forceps, and drawing it out very gently, the passing of the pin below, and the loop above, the artery will be greatly facilitated. When the artery is to be relieved from acupressure, the head of the pin should be gently twirled, and, having thus been rendered loose, may be pulled out with the greatest ease without causing any pain ; after which the loop, being liberated, can be readily withdrawn.

Torsoclusion.—Torsoclusion, formerly described by me under the name of the Aberdeen method, or the method by the twist, requires for its performance a pin only, and consists of two steps.

In the first step, the pin is inserted in the tissues pretty close to the mouth of the bleeding artery ; it is then pressed onwards in a direction parallel to that of the vessel ; and its point is caused to emerge to the extent of several lines.

The second step consists in giving a quarter rotation to the pin, so as to place its emerged extremity above and at right angles to the artery ; in pressing it well down against the *small portion* of tissue between the instrument and the vessel; and in sending the point for some distance into the tissues beyond the artery, for the purpose of securing the pin in position and of maintaining the twist. The artery is to a certain extent twisted, as well as steadily compressed, and in that way, no doubt, the occlusion of the vessel and the suppression of the hemorrhage are effected.

Of all methods of acupressure, that by torsoclusion as now described is the simplest, the easiest, the quickest, and it has the indispensable recommendation of being perfectly efficient. In reference to the advantages of this method, I beg to be allowed to quote from my own writings. "Simplicity, efficiency, quickness, and ease of performance, are unquestionably great recommendations; but this method possesses two other advantages, of the greatest importance for obtaining either immediate union or union by primary adhesion: the one, that there can be but little molecular injury or straining of tissues; and the other, that, by the gentlest twirl and traction, the pin can be easily withdrawn, with extremely little, if any, discomfort to the patient. Surgeons who know how little produces pain in an amputation wound, how slight a degree of pain is apt to cause involuntary contraction of muscles in the stump, how certain such contractions are to separate parts of the internal surfaces of the wound from each other, although by retentive means its edges are kept in apposition, and who, after the greatest anxiety to obtain the best results, have so often, in a few days, witnessed the disheartening effects of such contractions, will fully appreciate the value of a means of arresting hemorrhage which, at the moment deemed judicious, can be removed, not only without pain, but almost without discomfort to the patient.

The first time the femoral artery was secured by this method was by myself, in an amputation of the thigh, in the Aberdeen Hospital, in June, 1864; when those present were exceedingly gratified with the great simplicity of the method, as well as the facility with which it could be employed. Since that time it has been employed in an immense number of cases, by the late Dr. Keith, by Dr. Fiddes, and myself; and it is to be hoped that our united testimony in favour of this admirable mode of arresting hemorrhage may induce surgeons of other hospitals to give a fair trial.

Retroclusion—so named in consequence of the pin passing ultimately behind the artery—is a convenient mode of securing vessels of small or very moderate size, and, like torsoclusion, consists of two steps, and, like that method of acupressure, requires a pin only for its performance. In the first step, the pin is entered in the muscular tissue, a little to one side of the artery, held almost flat upon the wound, caused to emerge, and passed in front and a little beyond the track of the vessel. It should be particularly observed that, in this movement, only a few muscular fasciculi are raised upon the pin, and that it is caused to emerge from the tissues before being sent across the track of the artery.

In the second step, the head of the pin is made to describe the greater part of a semicircle, so as to be placed nearly flat on the opposite side of the wound; its point is then sent behind the artery, in the contrary direction to that in which it passed in the first movement, and pressed on until it is fixed in the tissues beyond the vessel.

When is it safe to free an artery from Acupressure?—If one could tell the earliest time at which an artery could be safely free from acupressure, that would accelerate its more general adoption; but time and experience alone can teach this, and time and experience have not yet done that work. Nevertheless, they have done enough to teach us that, in ordinary circumstances, where acupressure has been skilfully performed, vessels of comparatively small size, such as the facial, temporal, radial, ulnar, mammary, and spermatic, may be safely freed from acupressure in eight hours; and larger arteries, as the humeral, axillary, and femoral, in twenty-four. I have a strong impression that future experience will show that the early friends of acupressure have been too cautious in removing the compression, thereby losing the full advantages of its use, and that much shorter time will yet be proved to be sufficient; and the shorter the better for increasing our chance of the most coveted methods of healing wounds. On the adoption of acupressure in Aberdeen, in *1864, we had no experience* to guide us on this most interesting point, forty-eight to seventy-two hours being then deemed necessary; and, in working out this problem, we have perhaps gone on too slowly shortening the time; but we felt ourselves bound not to expose our patients to untried risks, and also not to injure what we consider a valuable proceeding by the occurrence of hemorrhage from imprudent haste.

Two cases produced a strong impression on my mind, as holding out great encouragement to shorten the duration of acupressure. One was that of a little boy, whose thigh was amputated by the late Dr. Keith, and who, four hours after operation, in the absence of the nurse, took out the pin which compressed the femoral artery without any bad result. The other was that of a girl, whose arm I amputated, and fifteen hours afterwards relieved the axillary artery, without the withdrawal of the pin having been followed by a drop of blood.

Every one knows how greatly the dread of hemorrhage has retarded the progress of surgery; but to show how little risk there is of bleeding on relieving arteries from acupressure, I may state that the late Dr. Keith and myself, in hospital and private practice, compressed altogether upwards of two thousand arteries; that in one only of Dr. Keith's cases was there bleeding on the removal of the pin, and in one only of my own; and in each case the artery was again acupressed in a few seconds. In the one case, Dr. Keith had performed amputation in the forearm, and the bleeding was from the radial; in the other, I had performed amputation of the upper third of the leg, and the hemorrhage was from the anterior tibial artery. We read of the pin having been left for five days compressing the femoral artery, which is contrary to every rule that obtains in relation to the process; and the wonder is, that, left so long, it should not have caused irritating results.

Effects of Acupressure on Arteries.—On dissection, I have not seen any division or laceration, any discoverable injury or degeneration, any suppuration or sloughing of the coats of an artery at the site where it had been acupressed; and, with the exception of two cases, the condition has been occlusion from adhesion of the internal surface, and an internal coagulum more or less adherent to the artery. In one of the exceptional cases there was a coagulum an inch in length adherent to the artery, without adhesion of the internal surface. In the other, four vessels had been acupressed; in the two larger there was an internal coagulum and adhesion at the site of the compression; and the two smaller were closed by lymph at their cut extremities without an internal coagulum. In the first-mentioned case, death was caused by shock thirty-six hours after operation; and in the second, by violent dysentery, which commenced on the third, and proved fatal on the tenth day after operation.

Merits of Acupressure.—Under this head there are two questions to which, as a firm believer in the advantages of acupressure, I earnestly beg the unprejudiced consideration of surgeons.

1st. *Is Acupressure a practicable and trustworthy method of arresting surgical hemorrhage?* That it is so is incontrovertibly proved by the fact that, since March, 1864, with the exception of two cases to be afterwards mentioned, in all my operations, both in hospital and private practice, comprehending, among many others, amputations in the thigh, leg, arm, and forearm, and at the ankle-joints; in excision of the mamma, testicle, knee and elbow-joint, those of the mamma and elbow-joint having been very numerous; in all operations admitting of the surfaces of the wound being brought together—in short, in almost every variety of operation, age, and sex, I have invariably employed acupressure, and never in a single instance failed in arresting the hemorrhage. What stronger proof of the practicability and trustworthiness of acupressure could possibly be desired? If to this evidence be added the experience of the late Dr. Keith, and that of Dr. Fiddes, two unwavering believers in the superiority of acupressure over every other method of arresting surgical hemorrhage, whose cases, along with my own, constitute the experience of acupressure at the Aberdeen Infirmary, surely the question of the practicability and trustworthiness of this method of checking surgical hemorrhage must be set at rest.

In two important operations above referred to—the one the removal of a tumour upon and underneath the angle of the jaw, the other excision of the lower jaw, submaxillary and sublingual glands, and a portion of the tongue—I considered it prudent to give the preference to the ligature, as the pins during their brief sojourn in the wound might have caused inconvenience during deglutition.

2nd. *What are the special merits of Acupressure?* The greater tolerance of living tissues to metallic than to textile bodies; the brief sojourn of the hæmostatic agent in the wound; the satisfaction to the surgeon of being able to remove that agent the moment removal is deemed advisable; the comfort and encouragement experienced by the patient on being assured, at the conclusion of a very brief period after operation, that all foreign matter has been removed from the wound; the immunity of the coats of arteries from laceration and sloughing, and, in consequence of that immunity, the diminution of the risk of septic poisoning, a case of which I have not seen since the adoption of this method of arresting hemorrhage—are unquestionably great advantages of acupressure. The superiority of acupressure, however, will be made more apparent when we consider what results we desire to obtain after operation, and how these may be best secured.

Union by the first intention, or by primary adhesion, is the aim of every surgeon, where accurate coaptation of surfaces and edges can be maintained. So long as we leave foreign bodies in wounds, this much to be desired result cannot be obtained. If, on the other hand, we free them from all such bodies at a period so early that their presence cannot give rise to irritation, an insuperable obstacle is removed, and union by one or other of these methods will in all probability ensue. Here, then, is demonstrated the superiority of acupressure over all other means yet devised for the arrest of surgical hemorrhage. For a few hours after an operation has been performed, we can with perfect safety withdraw the pins, and all *local* hindrance to early union is thus overcome. My own experience, as well as that of the late Dr. Keith, and of Dr. Fiddes, proves the correctness of this statement. Prior to our adoption of acupressure, n 1864, I had never seen, in any case where a large vessel had been secured, a perfect example of union by the first intention. Since then I have had examples of this method of healing in every amputation of the body, with the exception of the leg and at the hip-joint—the latter operation I have not, during that period, had occasion to perform,—in excision of joints and tumours, and many other minor operations. In addition to cases of union by the first intention occurring in the practice of the late Dr. Keith, Dr. Fiddes, and myself, already published, let me mention one or two remarkable examples of early union.

On a girl twelve years of age, I performed, according to my own method, amputation at the ankle-joint, on account of extensive caries of the bones of the foot. Three vessels were acupressed; the wound was exposed to the air until its surfaces had become glazed, and its edges, having been brought into accurate apposition, were retained simply by strips of plaster. The wound healed without there having been a single drop of discharge of any kind: and on the twelfth day after operation, the patient, being perfectly convalescent, left he hospital. Such a result cannot but be considered unusual in an amputation of the ankle-joint.

One of the most striking cases I have ever seen was that of a female, the subject of a huge adenoid tumour of the mamma, which I excised, she being at the time in the ninth month of pregnancy. The tumour weighed 13½ lb. The wound healed entirely by the first intention; fifteen days after the operation she was discharged from the hospital, and on the ninth day following she gave birth to a healthy female child, which she was enabled to suckle, much to her own satisfaction, as she attributed the loss of two former children to the want of breast milk.

In another case—a private one—a carcinomatous mamma, of large size, was excised on a Friday, and on the following Sunday week the patient went to church, the wound having entirely healed by that day, without the formation of a single drop of pus.

In another case of the same disease, I removed a large mamma; perfect union by first intention took place; and fourteen days afterwards the patient left the hospital, remarking that she had enjoyed her residence there very much—rather an unusual statement for a patient to make who had been the subject of operation at a period so immediately preceding.

I might enumerate many other equally striking cases, but for the present the foregoing must suffice. The favourable results I have experienced during the last seven years I attribute entirely to the coats of the vessels secured having been neither cut nor lacerated, to the non-irritative properties and early removal of the hæmostatic agent used, and to the comparative disuse of dressings. With regard to the last-mentioned point, I may state that, with the exception of wet lint, I do not now employ any dressings, nor do I use antiseptic substances.

Before bringing this paper to a conclusion, let me remark that acupressure is far from difficult of application, and that any slight difficulty that may be at first experienced will, with a little practice, disappear; and the results following its employment will repay a thousandfold any effort made for acquiring dexterity in its use.

I have now done my best, not in any controversial spirit, to express clearly and briefly my decided convictions regarding the essential modes and merits of acupressure; and as it is specially important, for the interests of those who may become the subject of surgical operation, that this method of treatment should be justly appreciated by the great body of surgeons, I earnestly hope that a fair and skilful trial—the only means by which a correct judgment can be arrived at—may be as fully accorded to this as to other important proceedings in practical surgery.—*Lancet, July* 1 *and* 8, 1871, *pp.* 7, 42.

44.—ON A NEW AND SAFE METHOD OF PERFORMING ACUPRESSURE.

By R. Clement Lucas, Esq., late House Surgeon to Guy's Hospital.

It is now some time since acupressure was introduced at Guy's Hospital; but at the time it was there practised I had an opportunity of observing that it was not unfrequently followed by hemorrhage, either from imperfect application in the first place, or from the wires and pins becoming loose from some cause, or from attempts made to remove them too soon after the operation. I then contrived the method of securing vessels that I am about to describe, which renders acupressure more simple and quite as safe as a ligature, and has the advantage of being equally certain in the hands of a novice as in those of the most expert professor of acupressure. The subsequent introduction of torsion, and the rapid conversion of all Guy's surgeons to that mode of securing vessels, prevented my drawing attention to it at the time; but finding that acupressure is still largely practised and advocated by surgeons of the greatest eminence, I have thought it right to lay this method before the profession.

An ordinary acupressure needle, having a ring at one end, is twisted so as to form a small loop about an inch from its pointed extremity, or at such a distance as the surgeon may think most convenient, varying with the size of the needle. A piece of fine wire is doubled in the middle as in ordinary acupressure, and the loop thus formed is passed first through the ring at the end of the pin, then through the loop on the staff. After sufficient has passed to reach easily over the point of the needle, the loop of wire should be bent up at right angles so as to be out of the way, as shown in Fig. 1. In this way needles should be kept ready for use.

Fig. 1.

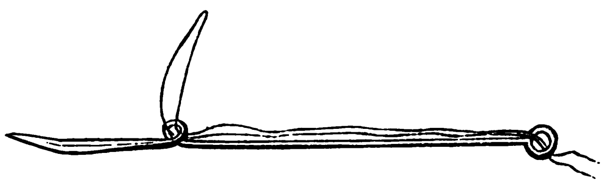
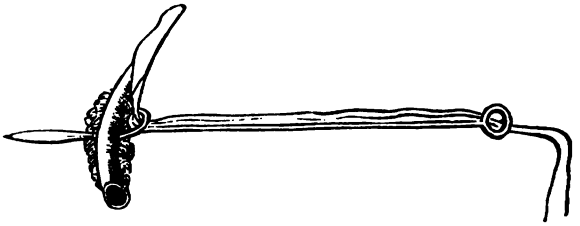
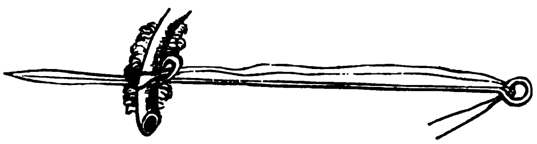


FIG. 2.



In performing the operation for securing the vessel there are three acts. First, the point of the needle is passed under the vessel as shown in Fig. 2. Secondly, the loop of the wire is bent down over the vessel and slipped over the point of the needle. Thirdly, the vessel is compressed by pulling the free ends of the wire tightly through the ring at the extremity of the needle, and finally secured by bending back the wire at this point as shown in Fig. 3; or it may even be made more safe by giving the ends of the wire a twist round the needle close to the ring.

FIG. 3.



To remove the pin, all that is necessary is to straighten the wire bent round the ring at its outer extremity; then on gently pulling the pin the loop of wire glides along towards the point and releases the vessel. Should bleeding now take place, the vessel may again be compressed by pulling on the wire, provided that the pin has not been so far extracted as to have allowed the loop of wire to pass over its point. After the pin has been removed the wire follows as in the mode of acupressure described by Professor Pirrie under the name of "circumclusion."

I venture to claim for the above method the following advantages: 1. That it secures the vessel against all chance of bleeding. 2. That it permits the pressure upon the vessel to be regulated. 3. That, when about to remove the wire it allows the surgeon to ease the vessel and again tighten the wire upon it in case of bleeding without reopening the stump, which it is not possible to do by any of the methods now employed."—*Lancet*, *September* 1871, *p*. 320.

45.—A CONTRIBUTION TO THE SURGERY OF BLEEDING VESSELS.

By Dr. Alex. Ogston, Aberdeen.

[About two years ago Dr. Ogston performed a number of experiments in order to test the comparative strength of the closure of vessels by the three methods of ligature, acupressure, and torsion. The result of these experiments was that ligature resisted the highest column of mercury brought to bear upon it, viz., 114 inches, whilst acupressure only resisted 23·5, and torsion 13 inches. This, of course, incontestibly proves the superior safety of the ligature.]

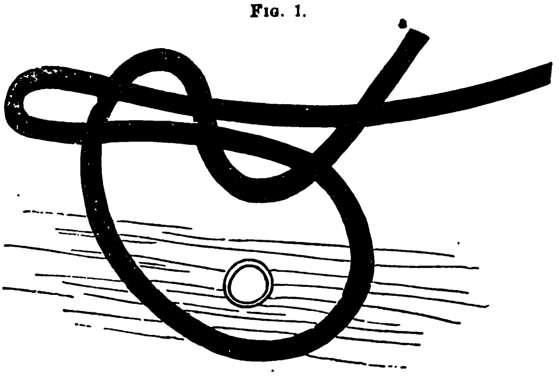
That none of our present modes is entirely satisfactory cannot be denied. Torsion seems, in some hands, to have given marvellously good results; but, having had no opportunity of witnessing it as applied regularly to large vessels, I can only say, under liability to correction by those who have tried and adopted it, that the small amount of internal pressure resisted by it forms a serious, although the only, objection to its use. In Aberdeen, on the contrary, we have abundant opportunity of studying acupressure, and the result of my own observation is to convince me that it cannot retain the place originally claimed for it in operative surgery. The one real advantage that it possesses is that it allows the wound to be freed from foreign bodies one or two days after operation—doubtless a very important matter, but which can be attained without its employment. The asserted frequency of primary union in wounds treated here by it, and the absence of suppuration in such wounds, are to a certain extent correct. In amputations of the mamma it yields first class results, primary union and absence of pus being generally achieved; but in other amputations suppuration occurs just as usual, the statements to the contrary being due partly to the great enthusiasm of its advocates, and partly to a careful nurse removing, as far as possible, all traces of discharge before the visit of the surgeon.

Acupressure has several disadvantages. The pins frequently prevent complete closure of the wound at the time of the operation, and the edges have to be accurately adjusted at a subsequent period. The removal of the wires requires a considerable degree of traction, and in the employment of the ring-loop, is sometimes a matter of difficulty, while, in at least one recent instance, an acupressed vessel in an amputation wound required, owing to secondary hemorrhage, the subsequent application of the ligature. The estimate of acupressure I have formed is similar to that of many other surgeons who have given it a trial.

Considering, then, the superior strength of ligatured vessels, it would be a step in advance could the ligatures be applied in such a manner as to retain their superior security, and yet so as to allow of their being removed about the second day after operation, before the tendency to suppuration has commenced in the wound, and also so as to avoid snaring off the ends of the arteries and the tissue grasped by the forceps in the usual mode of applying them.

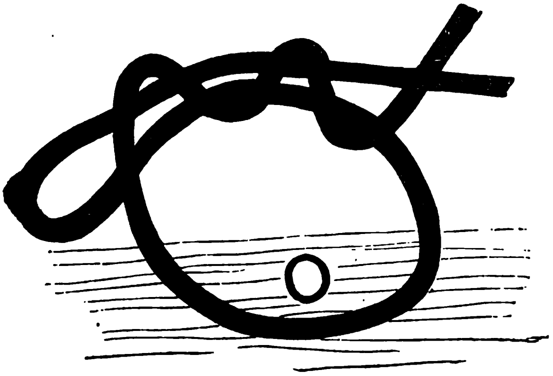
All these advantages are combined in the method I have now to propose—namely, that the vessels be secured by the knot depicted in Fig. 1, which, if it be run close, and the short end be cut off close to the knot, can be removed by very moderate traction on the long end. The knot, it will be perceived, is merely the "thumb or over-hand knot," similar to the first part of the knot ordinarily applied to arteries, with the single exception of a loop of the long end being employed instead of the undoubled cord. Its hold is perfectly sufficient when tied but I shall afterwards come to discuss its security.

FIG. 1.



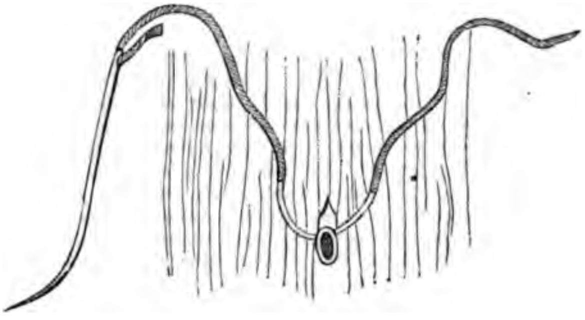
In applying the knot to the flaps of an amputation wound, the following is the method I have adopted:—A piece of *the strongest* ligature silk, well waxed to diminish the risk of slipping after it has been tied, has one of its ends passed an inch or thereby through the eye of a curved needle. (A few such armed needles are prepared before the operation.) The bleeding vessel being seen, and its course above the orifice estimated, the needle is entered from the raw surface of the flap, a quarter of an inch on one side of the course of the vessel and the same distance above

FIG. 2.



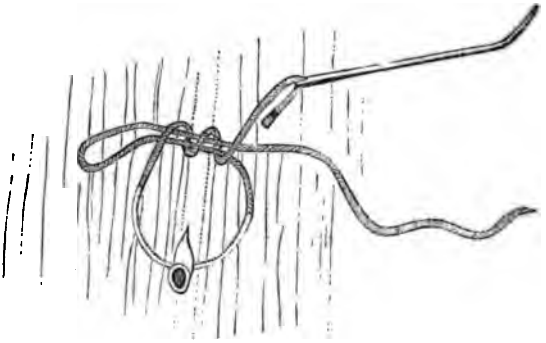
the bleeding orifice; then passed down behind the vessel so as to include it between the needle and the raw surface of the flap; and, having crossed behind the vessel, is brought out a quarter of an inch on the other side of its track. The needle is next drawn through, bringing a couple of inches of the thread with it, as in fig. 3, and it may then be detached from the thread. A loop of the long end, an inch or thereby in length, is then used to form, with the single short end, an ordinary "thumb knot," as in Fig. 1, or if additional security is required, a double knot, as in Figs. 2 and 4. The knot is closed upon the vessel and included tissue by traction on the loop and short end; the strands of the loop are placed close to each other, so as to lie in contact throughout and offer no obstacle to removal when the long end is pulled; and, finally, the short end is cut off close to the knot.

FIG. 3.



Such knots do not slip till traction is made on the long end, which ought to be brought out at the side of the wound next the original point of entry of the needle, and not doubled again over the vessel; and, correspondingly, the loop is laid suitably directed away from the vessel in the direction of the needle when it emerged at the further side of the artery. In small arteries the single knot is amply sufficient; in large arteries I use the double knot for the sake of security. The ligature is removed, on the second day after the operation, by gentle traction on the long end; and the amount of force required for its removal, while too great to be readily effected by accident, is, notwithstanding, less, even with the double knot, than that needed to remove the wire where acupressure has been employed.

FIG. 4.



The advantages of this proceeding consist in the speedy removal of foreign bodies from the wound, and in the fact that no tissue is compressed or strangled by it save the portion lying in the very knot itself. The distal end of the vessel is still in connexion with the living parts surrounding it, and its nutrition is not interfered with.

The strength of the method is another of its advantages. Vessels experimented upon in the manner given in detail in my former paper, resist, when secured with the single knot, from 24 to 70 inches of mercury; and on an average, 39 inches of mercury, or a pressure of 19 lb. to the square inch, as against 23·5 inches of mercury with acupressure, and 13 inches with torsion. The double knot resists the full column admitted of by my dynamometer—viz., 114 *inches of mercury, or* 57 lb. to the square inch. I have tested in animals this application of ligature to cut vessels, and to vessels deligated in continuity, before using it in man, and the results were pretty satisfactory. In man they have hitherto been equally so. I have at present a stump after Pirogoff's amputation treated in the above way, where the ligatures were removed on the second and third days, and which is healing with as small an amount of discharge as I ever saw obtained even with acupressure.

The disadvantages are, that, as in acupressure, some tissue is included along with the vessel; and where structures such as important veins and nerves run parallel to the artery, damage might be done by their inclusion. Thus it is best suited for amputations; indeed, without further experience of it, I should hesitate to employ it in the deligation of an artery in continuity upon a human subject. I have had no opportunity of testing it in that most difficult of all vessels to secure, an interosseous artery in the angle of a flap amputation, but believe that, by laying hold of the bleeding orifice and the tissue surrounding it, and then proceeding, as in a flap, to pass the needle behind it through the grasped mass, it would answer as well as the ordinary ligature.—*Lancet, May 27, 1871, p.* 707.

46.—OBSERVATIONS ON LIGATURE OF THE SUBCLAVIAN ARTERY —A NEW INCISION SUGGESTED.

By Assistant-Surgeon F. P. STAPLES, Medical Staff, Barully, Rohilcund.

That ligature of the subclavian artery on the living subject is not an easy operation, even in the hands of the most experienced surgeons, few will question; while, on the dead body, there is no doubt that most surgeons have seen attempts to occlude that vessel fraught with considerable difficulty, if not with actual unsuccess. To assert such truisms, however, is not my object in writing, but to bring before the notice of surgeons a method which I have practised for some time, and by which, I venture to hope, the difficulties of ligaturing that vessel in the third stage can be overcome.

Operation.—The patient being placed in the usual position, with his head back and to the opposite side, with his shoulder depressed slightly, but not violently, let the point of the knife be entered at the posterior edge of the sterno-mastoid muscle, one inch and a quarter above the superior margin of the clavicle, and let an incision be carried from that point, in a straight line, to within a quarter of an inch of the attachment of the trapezius to that bone, dividing skin and platysma. This incision should be a little short of three inches. The operator should then lay aside his knife, ligature the external jugular vein in two places, and divide it in the direction of the original incision. The deep cervical fascia should now be divided, and the edges of the wound gently separated, when the posterior belly of the omo-hyoid muscle will be exposed for its entire length. The edges of the wound should now be retracted, and the superior retractor should carry with it the omo-hyoideus; and when this has been done, the white cords of the plexus, with the artery inferior and internal to them, will be observed to occupy the bottom of the wound. The knife should now be laid aside, unless it is necessary to dissect a lymphatic gland out of the way, and the vessel separated from the lowest cord of the plexus with a director, and ligatured in the usual manner. Tying the external jugular vein is not insisted upon, providing it can be easily drawn aside, but generally a ligature would expedite matters, and any branches of this vein which cross the line of incision should, if divided, be treated in the same manner.

What are the advantages claimed for the operation recommended? Why have the stereotyped guides to the artery—viz., edge of anterior scalenus, and tubercle on first rib—not been mentioned? What special advantages has the operation described over that commonly practised—*i.e.*, by incision along or near upper margin of the clavicle?

The advantages claimed for the operation are—1. That the incision is parallel to the normal *course of the artery.* 2. That the true guide to the vessel—posterior belly of omo-hyoid—is exposed by incision recommended for its entire length. 3. That the edges of incision admit of easy retraction, and, in this way, of easy access to the vessel. 4. That the risk of venous hemorrhage obscuring the final steps of the operation is lessened.

My answer to the second question I have asked is very simple. To feel the edge of the scalenus anticus in a bleeding wound is next to, if not quite, an impossibility, and it does not follow that the tubercle on the first rib is always so well developed as to permit of recognition by the sense of touch; and, independently of both these surgical signposts, it has always appeared to me that a far more reliable guide is to be found in the omo-hyoideus.

Regarding the third question—What special advantages are claimed for this operation over that commonly practised? It may be stated, I think, that, if an incision is made in a line with the clavicle, it is obvious that, when carried deeper, it will not meet with the omo-hyoideus or true guide unless at its outer angle; whereas the incision recommended is parallel to that muscle throughout its entire length. Secondly, in the wound resulting from the ordinarily used incision, retraction can only be made in an upward direction, as the clavicle prevents retraction downwards; whereas with the incision now recommended retraction can be made in both directions. Thirdly, in the incision recommended there is no risk of dividing the transverse cervical vessels; whereas, when the incision along the clavicle is used, they are often cut, and, when it so happens, very troublesome bleeding obscures the further steps of the operation.—*Medical Times and Gazette, July* 22, 1871, *p.* 96.

47.—ON THE TREATMENT OF ANEURISM BY COMPRESSION.

By Dr. Rawdon Macnamara, Surgeon to the Meath Hospital, &c.

A case of popliteal aneurism presents itself for treatment. We determine to use compression. We first carefully ascertain the condition of the patient's general health. If anæmic or hyperæmic, we take appropriate measures; and when we are satisfied upon this point, we apply some one or other of the most improved compressors—those in which the compressing power is modified by elasticity. With this we compress the artery in the upper portion of its course, having previously arranged, some three or four inches lower down, the auxiliary instrument by means of which we propose to alternate the pressure. The upper instrument is now made to control the artery, so as but just to arrest the pulsation in the sac. This is the most delicate step in all the procedure, and is regulated by the hand of an intelligent assistant, who at once informs us when the pulsation is arrested; and then and there the further application of pressure is arrested. A roster of intelligent students is now organised, and to them is entrusted the management of the case. Two are appointed to take charge of the patient for one hour, when they are relieved by two others, and so on during the day, whereby we secure unwearied attention during the period that pressure is kept up; and, as in Dublin we visit our hospitals at 9 o'clock a.m., the treatment generally commences about that hour, and is continued up to 9 o clock p.m., when all pressure is removed, and the patient is encouraged to take his night's rest undisturbed. Next morning the treatment is resumed, and so on until the cure is perfected. At the commencement of the case, we take the patient into our confidence; explain to him the nature of his case and the method we are about to adopt for his cure, placing clearly before him the alternative, with all its possible dangers, which we should have to adopt in case compression should fail. The value of this procedure is very frequently demonstrated by the intelligent interest exhibited by our patients in the management of their own cases—so intelligent as in protracted cases to supplement, if not altogether to supersede, the supervision of them by our students. In the selection of our compressing force, we adopt in its widest sense the maxim "Nullius addictus jurare in verba magistri." Should one compressor prove irksome, we try another; if all should fail, we have recourse to digital compression, or to compression by means of weights; but, in every instance, convinced of the soundness of this plan of treatment, we leave no stone unturned to secure its success.

As you will collect from the preceding remarks every exertion is made on our parts to conduct the case with as little inconvenience or pain to the patient as is possible. Pain in the seat of pressure we look upon as an evil; but there is a pain, and that sometimes for a few hours a very sharp pain, to the advent of which we anxiously look forward. I allude to pain in the neighbourhood of the knee-joint, which pain is the harbinger of cure. This pain is due to the rapid enlargement of the collateral circulation, dependent upon the diminished calibre of the aneurismal sac, encroached upon by the deposit in it of fibrine, and is best combated by narcotics.

So far for our Irish plan of treating aneurism by compression: I now come to the question so often asked me, why we should be so successful, and you so much the reverse, in thus treating aneurism.

First. And above all, we have faith in the efficacy of our treatment, and, reverently be it expressed, faith can do all things. Your experience can scarcely justify you in entertaining such faith.

Second. In this instance, at all events, the spirit of nationality pervades our ranks, and the humblest, the most indolent, the most apathetic surgeon amongst us, will try every expedient ere he will acknowledge that in his hands the Irish method of cure has failed.

Third. Our hospitals are visited at 9 a.m.; yours, I believe, at 2 p.m. By our arrangements we are enabled to visit our patient, and to commence his treatment under our personal supervision at an early period of the day.

Fourth. All our hospitals are within a few minutes' drive of the residences of their surgeons; thus are we enabled to pay during the day repeated visits to such cases, and so to watch them from hour to hour; and no matter how zealous, how intelligent, be the house-surgeons or the pupils, there is no eye equal to the master's for seeing that the work be done. That such surveillance imposes great trouble on the attending surgeon I freely admit, still it must always be borne in mind that it was *per tædia et labores* that this plan of treatment has been brought to its present perfection, and that *pour reussir il faut travailler.* You know best your arrangements in these respects.

Fifth. Thoroughly aware, as we are, of the pathological changes which must be brought about in the contents of the sac to effect a cure, and convinced, as we are, of the principles whereby alone these changes can be produced, we are not afraid, during the night, to remove all pressure, and so to secure for our patients uninterrupted sleep, whereby we keep up their health to the condition most favourable for fibrinous deposit in the sac.

Sixth. And here I trust that I will not be considered as blowing too loud a blast on our Irish trumpet, I am of opinion that some of our success must be attributed to the intelligence of the patients whom we are called on to treat. Many are the instances in which their co-operation has proved to us of inestimable value. One of the class of instruments (the clamp)of which, even to the present day, we frequently avail ourselves, is due to the suggestion of a patient (a carpenter by trade) of the late Professor Harrison's; and, did time permit, numerous other instances of the truth of this assertion could be adduced.

Finally. The physique of our patients differs widely, I suspect, from yours. I fancy that the disease occurs, in the majority of English patients, in the persons of well-fed, robust, and plethoric individuals; such patients will require a compressing force of high tension; but, as a rule, it may be laid down that if the force employed exceed that which would be represented by a *dead* pressure of nine pounds, the pain to which it will give rise will soon render it intolerable. Our patients rarely, if ever, present such appearances; they rather incline to the opposite extreme—and, when such is the case, we are obliged to call to our assistance good diet. 'Tis true that the plethoric individual may be reduced by regimen, &c., to the *juste milieu.* Still, after all, it is but human nature that we should find patients more amenable to a plan of treatment which involves generous diet, *than to one which* enforces starvation and depletion.—*Brit. Med. Jour., Aug. 19, 1871, p. 200.*

48.—ON ATHEROMA OF ARTERIES.

By Dr. J. Milner Fothergill, late Senior Resident Medical Officer to the Public Dispensary, Leeds.

Atheroma is essentially a parenchymatous inflammation of the inner arterial issues, and may either exist in patches or be more general, leading to dilatation of the arteries, and loss of elasticity. When the arterial elasticity is impaired, the artery distended by the ventricular systole is unable to recover itself. It thus remains more and more distended, and, when possible, elongated —*i.e.*, tortuous. This impaired elasticity then lessens the recoil; the distension and elongation of the arteries interfere with the recoil; and, instead of perfect recoil, the artery remains permanently enlarged. This change goes on gradually, incessantly aggravated by exertion, and all calls on the heart for increased action. The arteries become elongated and distended, from the aorta to the tortuous temporal artery; they are increased in length and breadth; they become, too, more brittle and more easily ruptured by strain. Here, now, we see a wonderful instance of conservative change, tending directly to prolong the existence of the organism. The diseased arteries recoil less perfectly, and thus the coronary circulation becomes involved, and its circulation diminished, and thus impaired nutrition of the heart; and from that, again, lessened and impaired activity, and thus diminished risk of the heart rupturing the diseased and friable arteries. Thus we see that not only is fatty degeneration a preservative lesion, as Sir William Jenner showed at Leeds, but that that very change which is preservative in its action, is entailed by the diseased condition of the arteries, which the very want of that lesion would endanger. Thus the changes go on, hand in hand; the more diseased, brittle, and non-elastic the arteries, the less their power of recoil, and thus impaired structural integrity of the heart and impaired contractile power. The diseased arteries thus fortunately lead to the very muscular change imperatively necessary in order to prevent the heart from rupturing them. Where the atheroma is general, there is a stronger probability of fatty degeneration following, and thus this condition is more favourable to life than when the atheroma is in patches. Where the atheroma is deposited in ring-like, annular patches, encircling the aorta at the branching off of some smaller artery—its favourite seat—it is more fraught with danger. The hypertrophy consequent on the obstruction to the flow, and increased tension resulting from the obstruction, does not so soon undergo preservative degeneration, as the sound parts preserve their elasticity and recoil fairly. Thus the obstruction of the isolated masses, especially if annular, keeps up the hypertrophy by increasing the tension behind them and the results of increased arterial tension, and thus endangers the patient's existence by the increased risk of some degenerate mass giving way. In these conditions, increased ventricular action could only render them more and more dangerous; the greater contraction and distension would try still farther the degenerate vessels, and could only slightly improve the coronary circulation, and thus only to a small extent diminish the risk of arrest of the heart's action, while it would certainly greatly enhance the more imminent risk of arterial rupture. If ever hypertrophy is to be regarded as otherwise than an unalloyed good, it is in these conditions. Certainly any attempt to secure it artificially in the manner described, would be detrimental to the patient's real interests; as the attempt to extenuate one danger would greatly enhance the hazard of another.—*British Medical Journal, July* 15, 1871, *p.* 58.

49.—NEW OPERATION FOR THE CURE OF VARICOSE VEINS.

Mr. Stokes has been recently treating varicose veins on a plan which was suggested to him by Sir Dominic Corrigan. It occurred to Sir Dominic that as hemorrhoidal tumours are, as a rule, so successfully treated by the application of strong nitric acid, the application of this acid to varicose veins in other situations would probably be attended with equally good results. In a case which is still under observation in the Richmond Hospital, this plan of treatment has been attended with the happiest results. The patient is a young man aged 21, and was admitted into hospital on the 15th of last month. He had a varicose tumour, of the size of a small orange, on the inner aspect of the middle third of the right leg. It had existed for seven years. He suffered also from a large varicose ulcer, which existed over the inner ankle of the same leg; and there was also a second tumour, formed of a cluster of varicose veins, in the right groin. Mr. Stokes performed the operation in the following way. Pressure having been made above and below the tumour, the integuments were raised from the tumour, and an incision, by transfixion, was made over the veins. The *fuming* nitric acid was then applied to the external coats of the veins. No pain attended this application. On the following day, the contents of the tumour appeared solidified at the base; and the acid was again applied. The process of solidification then went on rapidly, the tumour at the same time decreasing in size. A week after the operation, some coagulated blood appeared at the site of the operation; and the following day a portion of the vein came away. This was followed by a slight local inflammation; which, however, after a few days, quite subsided. The wound was then for some days dressed with tinct. benzoin co. and glycerine; and it rapidly healed, as did also the ulcer; and the large varicose tumour in the groin entirely disappeared.—*British Medical Journal, May* 28, 1871, *p.* 550.

ORGANS OF RESPIRATION.

50.—ON PARACENTESIS THORACIS.

By Dr. H. W. Fuller, Physician to St. George's Hospital.

[So lately as when Dr. Fuller held the office of Medical Registrar to St. George's Hospital, paracentesis thoracis was rarely practised—never except when the probed needle showed that the effusion was purulent. Even then the operation was not unfrequently delayed too long, until emaciation and hectic proved that the patient must soon succumb to the malady.]

Nowadays the folly of unduly deferring the operation is clearly recognised, and the tendency is to operate prematurely, before the improbability of producing absorption of the fluid has been ascertained, rather than to subject the patient to unnecessary delay. But there are certain points relative to the operation about which even now erroneous notions are entertained. The fallacious dread as to the admission of air into the pleural cavity which formerly exercised such a pernicious sway as almost to preclude the operation of paracentesis, even now often leads to the failure of the operation. Under the impression that the momentary admission of air will set up fresh inflammatory action in the pleura, or will injuriously modify existing inflammation, mechanical contrivances of various kinds have been employed to draw off the fluid from the chest without admitting air. The chest has been tapped under water; elastic tubes have been attached to the canula, and made to terminate under water; air-tight elastic bags have been attached to the canula—and various syringes have been made use of connected with an elaborate system of stopcocks. But whatever their form or precise character, they have one object in common, namely, to prevent the admission of air; and their advocates have asserted, not only that they effect their object, but that the chest may be readily emptied of its fluid contents by their agency. My objections to their use are both practical and theoretical. I object to their employment, 1st, because they are unnecessary, and complicate a very simple and harmless operation; 2nd, because the admission of air during the process of tapping causes no injury to the patient; 3rd, because it is impossible by any contrivance to prevent the admission of *a certain quantity of* air during the withdrawal of the canula—

and, therefore, even on theoretical grounds, there can be no valid reason against the admission of a larger quantity; 4th, because they mostly occasion unnecessary pain, and when suction-syringes are employed, a forcible strain is put on the parts, which is not only felt and complained of by the patient, but in some instances sets up fresh and serious local inflammation; 5th, because, although a certain quantity of fluid may be drawn off, it is physically impossible to *empty* the chest by their agency; and observation at the bedside has convinced me, that recovery takes place less frequently when a small quantity only of the fluid is drawn off than when the chest is thoroughly emptied. On one of these points only will I make any farther remark, viz., the impossibility in most instances of emptying the chest without the admission of air. When effusion into the chest has taken place rapidly, and tapping is had recourse to early, while the lung is still capable of expanding freely, it might be possible to evacuate the fluid contents of the chest without admitting air, if only some mechanical means could be devised for the purpose. In the case here suggested the lung would expand to fill up the space previously occupied by the fluid. But in many instances which occur in practice the pleurisy is of old standing; the lung has been compressed by fluid for weeks, or even months; it is carnified and incapable of speedy expansion; in many instances it is so bound down by firm adhesions, that expansion can only be brought about by the natural inspiratory efforts after a period of many months. In these cases, which are very numerous, it is simply impossible, whether by a suction-syringe, or by any other means, to draw off more than a small proportion of the fluid without the admission of air. The chest is a closed cavity like a cask; and just as it is impossible to empty a cask by tapping it without admitting air, so also is it in respect to the chest. The only difference between the two cases is occasioned by the rigidity of the walls of the cavity in the one case, and by their pliability and yielding character in the other. The walls of the cask being rigid, its fluid contents will run out only to the extent to which air can find admission to supply their place; whereas the walls of the pleural cavity will yield in some measure, to supply the place of fluid which is drawn off. The chest-walls will fall in, the diaphragm will rise, the mediastinum will encroach on the affected side; and up to the point to which they are capable of yielding, there is no necessity for the admission of air, inasmuch as by their displacement the space previously occupied by the fluid which is drawn off is at once filled up. But, provided the lung is permanently or even temporarily incapable of expanding, it is physically certain that, without the admission of air, the fluid contents of the pleural cavity can only be drawn off to the extent to which the walls of the cavity are capable of falling in to supply the place of the fluid withdrawn. If, in contravention of this physical law, an attempt is made by forcible suction-syringes to draw off still more of the fluid, injury to the patient must ensue; for there must be a forcible dragging and stretching of the walls of the chest, or of the lung itself, corresponding to the extra amount of fluid withdrawn. And as all mechanical violence is necessarily hurtful, it is obvious that in using forcible suction a considerable risk of injuring the patient is incurred, without the slightest corresponding advantage.

Thus, then, I come to the conclusion, on theoretical grounds, that all mechanical contrivances devised to exclude the air, or to exercise forcible suction of the fluid contents of the chest, are useless, or worse than useless; and this conclusion has been confirmed by my bedside experience; for I have seen patients injured in this manner. On the other hand, I maintain that the temporary admission of air is of little or no importance; and that if only a free opening is made into the lower part of the chest, whether by a full-sized trochar or a scalpel, the operation of tapping is a simple one, and almost uniformly successful.

From the time of my appointment as physician to St. George's Hospital, I have tapped every case of pleurisy, which has come under my care, in which, either from the urgency of the symptoms or the difficulty experienced in producing absorption of the fluid, recourse to more active measures seemed desirable; and out of the large number of cases in which tapping has been performed, one *only has proved fatal.* Within the last six months you have seen four of my cases tapped; no attempt was made to exclude the air during the operation, and yet you know how rapidly and satisfactorily they all recovered.

My advice, then, founded on large bedside experience, may be summarised thus: 1st, tap whenever dyspnœa is very urgent, or as soon as it becomes evident that remedies fail to produce absorption of the fluid in the chest; 2nd, tap as low down as possible, and make a free opening, allowing the chest to empty itself thoroughly; 3rd, so far as possible, avoid causing any local irritation; 4th, if the fluid withdrawn is serous or sero-sanguineous, close the opening with carbolic plaster as soon as the operation is concluded; if, on the contrary, the fluid is purulent adopt some means to prevent the wound from closing, and take care that the matter is allowed to drain off as fast as it is formed; 5th, after the operation, support the patient by bark and good nourishment, and for a day or two give him opium if necessary.—*St. George's Hospital Reports*, 1870, *Vol.* v., *p.* 7.

ORGANS OF URINE AND GENERATION.

51.—ON THE CHOICE OF OPERATIONS AT ALL AGES FOR STONE IN THE BLADDER.

By Sir Henry Thompson, Surgeon Extraordinary to His Majesty the King of the Belgians, Surgeon and Professor of Clinical Surgery to University College Hospital.

[The following paper is founded upon twelve cases of stone recently in the wards of University College Hospital. Four were treated by lithotomy and eight by lithotrity. Why were the four selected for one mode of treatment, and the eight for the other?]

My aim, then, is chiefly to answer for you the following question: "How do you select for each operation, respectively, the cases best adapted to each method?"

I reply, it is essential, in order to form a judgment, that you must previously become acquainted with the following particulars:—1. The size of the stone; or the amount of calculous matter, if multiple. 2. Its composition. 3. The age and constitution of the patient. 4. The conditions of the local organs.

First, the size of the stone. This is the most important feature taken alone; and the earliest examination of a case should inform us accurately on this head. For if a stone is small, it is almost certain to be successfully disposed of by lithotrity; and if it is very large, it will certainly be incurring a considerable risk to depend on the lithotrite for its removal. Were the alternatives always so complete as this, there would be no difficulty in determining the question at issue; but, as you well know, there are stones of all sizes between these two extremes, and many exist on the border-ground, so to speak, of size, for which other circumstances than that of magnitude may influence a decision in the case. To speak approximatively, however, in the matter of size, I think I may fairly say that a stone which measures 1¼ in. to 1⅜ in. in its longest diameter is for the most part fairly amenable to lithotrity. A stone of which any measurement is 1¾ in. is mostly too large.

But how, it may be said, do you determine these measurements? You who accompany me round the wards know that, at my first or second visit to any case of stone, this quality of size is easily ascertained by means of a flat-bladed lithotrite. I catch the stone first in one diameter and then in another, and the distance between the two blades is easily read off at the handle. There are other and subordinate ways—such as with the sound itself;—but none are so certain or so accurate as this, and it should always be employed. At the same time, you determine whether other stones exist besides that which lies within the grasp of your blades, by feeling for them while still grasping the stone already caught.

But you want, *secondly, to know* the nature and composition of the stone.

This is really scarcely less essential to know than its size. Remembering, then, that three-fifths of all stones, speaking roughly, are composed of uric acid and the urates, the other two-fifths phosphatic or mixed, and only now and then an oxalate of lime is met with, you will be prepared to expect a uric-acid stone if no circumstance contraindicates its presence. But, further, if in an ordinarily healthy man of from fifty to seventy years you find a hard stone, it is 7 to 1 that the stone is composed of some form of uric acid, pure or with a base; but if he is a strong, hale, hearty man of that age, and his urine is clear, it is 15 to 1 that the calculus belongs to this class. An oxalate-of-lime stone is more likely to be found at an earlier period of life, and the man has probably a less healthy appearance. If the stone be phosphatic, the patient is often broken in health, and the urine mixed with mucus, pus, phosphates, and a little blood. Next, there is the evidence to be derived from the "feel" of the stone by the sound, and the note which it utters when you give it a short, sharp tap. With uric acid and oxalate of lime the note is clear, and on drawing the beak of the sound along it a firm, hard surface is felt. But this is by no means decisive; you must feel your stone in the jaws of the lithotrite, and this will tell you more. The "feel" of an oxalate of lime calculus is wholly different from that of uric acid. I grant that this is appreciable rather to the practised hand than to that of the tyro, still I can describe perfectly what I mean. Suppose you have a good-sized uric-acid stone in the blades of your lithotrite; on turning the screw a little, and gently, you can easily feel the blades "bite"—that is, sink a little into the crust of the hard body; but if you have an oxalate-of-lime calculus in the blades, on screwing you make no impression, the stem of the male blade recoils with a spring, there is no "bite;" it is as though you had a piece of unyielding metal between the blades, not like a bit of some friable rock, which the other more resembles. On the other hand, a phosphatic stone gives a soft, uncertain sound when struck with an instrument, and the sensation of a rough, gritty body to it when drawing along its surface the beak of the sound; and the "feel" in the blades is also soft, gritty, and yielding. Indeed, with a little attention to these matters, and to the state of the urine, there is little or no difficulty in ascertaining the nature of a stone. It is quite true that in a few exceptional cases you may have a uric-acid stone or an oxalate-of-lime stone coated thickly with phosphates, and that you may be deceived thus; but these are rare instances, and by no means necessarily interfere much practically with the object we have in view—viz., the determination of the nature of the stone before operating. Thus, a sitting or two by lithotrity will soon remove the phosphatic crust, and a hard, unyielding mass of oxalate of lime may be arrived at, which must be extracted by lithotomy unless it be small.

Thus far we have considered under two heads the character of the stone itself. We will now study the questions as affected by the condition of the patient in regard of age and constitution. First, then, as a rule, to which there are very few exceptions, no proceeding is so perfect for all cases below puberty as lithotomy. In the series before us, however, one of these exceptions has fortunately occurred, which I shall refer to hereafter. The best information we have enables us to say that the deaths are not more from lithotomy than one in fifteen cases below the period of puberty, and in infancy they are still fewer. All these are consequently so far disposed of. For adults only, therefore, the question of choice remains. I may say, then, that, for a small stone, whatever the age, without any hesitation, lithotrity is the operation. I have to repeat that I have never yet lost a case of small stone by lithotrity. I mean by "small" the size of a bean or small nut—removable, say, at two easy sittings. This is a fact of the utmost importance, and the practical value of which it is impossible to overrate. Every stone ought to be detected by the time it arrives at the size indicated; and this being so, it is possible to remove it with a certainty of success and without risk to the patient's life.

Thus far no difficulty exists in our choice. But it is when we come to stones of an inch and upwards in diameter that other circumstances demand consideration. It is here that we look for other lights to guide us in our decision. Now I must remind you that in former time it was usual to admit that several conditions existed which were very unfavourable to lithotrity, and which, it was held, should incline the operator to prefer lithotomy. Among these, the strong tendency to rigors after almost any instrumental interference, supposed to depend on some derangement of the kidneys, was regarded as strongly contra-indicating the crushing operation. Now I think we may say it is very doubtful whether the phenomena so referred to are to be regarded as putting aside lithotrity altogether. I have of late given much consideration to this subject of "urethral fever," as it has been termed, although I have not time to say much about it now. I have long observed, in common with others, the strong tendency which exists in some persons to manifest constitutional disturbance after even very slight mechanical interference with the bladder and urethra. It has been the habit to say, and I have often said it, that this tendency, when strongly marked indicates perhaps the existence of some degree of mischief in the kidneys. I confess I doubt this. I have yet to learn that this tendency is necessarily or even often associated with renal disease. It is true that if disease of those organs exist, dangerous fever is apt to arise from an operation of lithotrity, or from any slight mechanical interference. But that a very strong tendency to the occurrence of what has been termed "urethral fever" does exist in many who have no sign of renal disease is to me equally certain. Understand that I mean by this "fever" not ordinary surgical fever, nor pyæmia, and of course not any specific fever, as typhoid, &c., but an occurrence of the following phenomena: Within a varying period of time after the use of an instrument, it may be after the first micturition, or after twenty-four or forty-eight or even seventy-two hours, the interval being often one of apparently perfect health, the patient is attacked with rigors, followed by dry heat, then sweating, often with much pain in the back, limbs, and head, and considerable depression of the vital powers, the urine being scanty and concentrated. This paroxysm has sometimes a tendency to recur, and is uninfluenced in this respect, as far as I can judge, by quinine. The occurrence of these events appears to me to be more an affair of constitution or temperament than of the presence or absence of renal disease. These symptoms appear readily among certain excitable or sensitive people, and I think I am right in saying they are more common among patients who are of Celtic than of Anglo-Saxon origin, using these terms in the broad general sense in which they have of late been usually understood. I say this advisedly, after a good deal of experience of patients among the two races, and I am quite satisfied that when I have to do with one of marked Celtic type I am likely to meet with the phenomena described after the most delicate mechanical interference. Such bear pain badly too, dread an instrument excessively, are impatient of control, and lose vital power rapidly under the influence of continued slight irritations. People with this constitution, whatever their nationality, do not well bear numerous sittings, nor the trouble of passing fragments, are apt to lose pluck and hope, and will be exhausted by pain and loss of rest before the numerous sittings essential for a rather large stone can be accomplished. I think, then, that this excitable temperament, this disposition to the fever I have spoken of, are the most unpromising conditions we can encounter in a patient with such a stone, so far as lithotrity is considered. And I cannot promise you that lithotomy is much better suited to them; more observation is necessary; but I am inclined to believe that when you are so unfortunate as to meet with a large stone in one of these people you had better cut than crush. It is to be remarked, however, that it is always some one's fault if such a patient is met with having a large stone. He is sensitive, soon complains, and his stone ought to be found when it is small. There is little excuse for missing it. These patients are easily identified, not merely by the general evidences of excitability which they readily manifest, but by the tendency to rigor after the mere preliminary sounding.

Next, we have to consider the condition of the urinary organs in aiding us to determine the choice of operations. In both operations a healthy condition, or nearly so, of these organs is of course more favourable than the contrary. What are the deviations from health most commonly met with in adult calculous patients? 1. Chronic cystitis. 2. Enlargement of the prostate. 3. Inability of the bladder to empty itself at the act of micturition. 4. Stricture of the urethra. 5. Pyelitis and renal disease.

1. Chronic *cystitis is by no means* always present; but, when it is, the remo-

val of the stone mostly, not always, suffices to effect its cure. When this result does not follow the operation, the bladder probably does not empty itself. (To be considered under No. 3.) Only when very severe and of long standing is chronic cystitis to be considered in connexion with our special object of inquiry.

2. Enlargement of the prostate rarely opposes any obstacle to lithotrity. In some of the most favourable cases I have had, and with the largest stones to which I have ventured to apply lithotrity, this condition has been present in a marked degree. The form of the lithotrite is particularly well adapted to enter the bladder easily when there is much enlargement of the prostate. There is not necessarily engorgement of the bladder and retention of urine, althougho ften it is associated with—

3. Inability of the bladder to empty itself by the natural efforts. With this there is often associated more or less of chronic cystitis. If a patient passes a part of his urine, still more if he passes the whole of it, by catheter, troubles of a grave kind *may* arise in lithotrity, although this by no means necessarily happens. Those who are accustomed to daily catheterism bear instruments, and consequently lithotrity, remarkably well; and not only that, but they bear the withdrawal of the débris—a proceeding essentially necessary in these cases, since it cannot be passed by the patient's efforts. If the stone, then, is not large, and the cystitis is but slight, no difficulty need be apprehended. But if the bladder is irritable, the patient requiring to use his catheter frequently, lithotrity, and the presence of fragments, will increase the irritation; and he will require the catheter at such short intervals that his rest is greatly broken, the urethra becomes very tender, and he runs the risk of making false passages, or at any rate injuring the canal, perhaps loses power to pass his instrument properly, and his position is then one of extreme danger. A patient, therefore, who requires very frequent catheterism, whose urine is loaded with thick organic deposit, and whose stone is above an inch in diameter, as a rule, to which there are exceptions, had perhaps better be cut. If the stone can be removed at two, or at most three, sittings, no doubt lithotrity would be the safer operation. If six or seven should be requisite, perhaps lithotomy will afford him a better chance.

4. Stricture of the urethra is by no means an obstacle unless the stone is rather large. The method of meeting the difficulty will be mentioned in remarking on one of the cases.

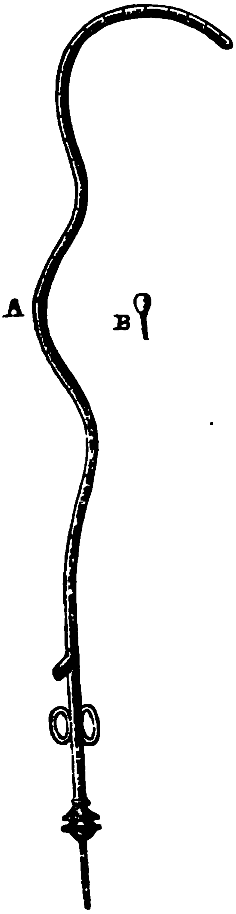
5. Renal disease and pyelitis are unfavourable conditions for either operation. When the stone is small, lithotrity ought to be held responsible, and I accept the onus of removing such by crushing, although great care is necessary. The intervals between the sittings must be long, and the patient must be very carefully tended and watched.

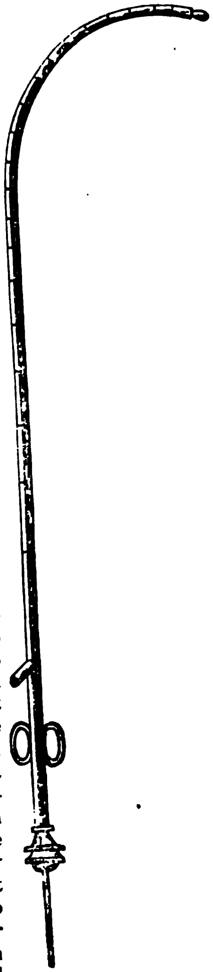
Now for a few words touching the mode of lithotomy employed. All those in our list have been operated on by the lateral method. A few years ago you know the median operation for a time came into favour, or, more correctly speaking perhaps, an extended trial was accorded to it by many surgeons in this country, myself among them. Confessedly, however, even on the part of its advocates, it was not considered adequate to the removal of large stones; and as meantime lithotrity has been advancing, and has become more and more extensively applicable, the want of an operation for small and middle-sized stones has disappeared. The only cutting method which, as a rule, is necessary now is one by which we can most safely and easily extract stones of rather large size, almost all beneath these being amenable to lithotrity. For this purpose lateral lithotomy is, doubtless, preferable, and excepting only for some unusually large calculus, to which a high operation may be better suited, is almost invariably applicable both to adults and children. Hence I have applied it to each of these four cases of two years, sixteen years, and two of sixty-five years with unexceptionable results in every case. The only drawback was in one in which, owing to various circumstances, a slight degree of sloughing took place between the wound and the rectum in the second week after the operation. This was a case in which there was much deformity from anchylosed hip; in it there was much hemorrhage, and a tube had to be introduced, with tight plugging round, to arrest the bleeding; and the stone in this case weighed nearly three ounces. When the patient left the hospital but a few drops of urine passed by the rectum, and the quantity was steadily diminishing. In the other cases no tube was employed; indeed now, unless bleeding takes place, in which case I use a "petticoated" one, well stuffed with lint I have discarded this instrument altogether. I think its presence hinders the healing process, and often causes unnecessary and additional irritation of the bladder.—*Lancet*, *July* 22, 1871, *p.* 115.

52.—SAYRE'S VERTEBRATED PROBE AND CATHETER.

By Dr. Lewis A. Sayre, Surgeon to the Bellevue Hospital, New York.

[The following is a letter to the *British Medical Journal*, in consequence of requests from different surgeons and instrument makers to examine Dr. Sayre's Vertebrated Probe and Catheter for detecting dead bone in tortuous sinuses, and for drawing off urine in cases of enlarged prostate, and also for use as a bent probe.]





It consists simply of a series of hollow silver disks, made a trifle smaller at one end than at the other, so as to fit into one another, like a pile of cups or tumblers. These are held together by a linked chain running through the series, and jointed nearly opposite each disk-insertion. This chain terminates in a square rod, which runs through the last disk, and is much larger than any of the others; and on the end of the small rod is cut a thread on which runs a small button-screw, which can make the chain tight or loose at pleasure. Of course, when the screw is turned back, the chain being lengthened, the disks fall away from each other, and the probe is as limber as a chain, capable of following any sinuosity into which it may be pushed. And by a few turns of the screw, the chain being shortened, the disks are drawn firmly together, so as to make a *solid* probe, which will give the concussion against carious or necrosed bone, the same as any other probe. A small slot is made in the canula containing the screw, for the purpose of putting a small nut which regulates the tension of the chain, and thus prevents any possibility of applying sufficient force to break it. There are two fenestræ in the distal disk, for the purpose of drawing an oakum seton through deep sinuses and carious joints: this makes it also very useful as a catheter in cases of tortuous urethras from enlarged prostate. It is impossible to make a false passage with it; and, as it is simply a series of ball-and-socket, or universal joints, it will follow any passage, however devious. By simply unscrewing the steel bulb at the end, and inserting a bulb of porcelain, according to the suggestion of Néaton, you have the most perfect bullet-probe that can be desired.

I have used this probe for more than seven months, and found it of the greatest advantage in the three several cases for which I have recommended it. I presented it to the New York Medical Society in July last; and an account of it will be published in their Annual Reports this year. But until now it has not been published to the profession, except in my public lectures at Bellevue Hospital, where I have used it frequently with great advantage. Mr. Weiss of the Strand is now making a number of them, by the order of Sir Alexander Armstrong, Director of the Medical Department of the Royal Navy; and I am happy to say that he has copied the model very admirably. Of course, like all other probes, this one requires to be made of different lengths and sizes.

To clean it, it is necessary to unscrew it at the end, and to remove the small screw at the slot in the canula, when it will immediately fall to pieces. After washing, it is easily put together, just the same as a string of beads, only remembering to put the small end of the disk on the wire first; and, as each disk increases in length until the end, of course no error can occur in making them fit properly. The cut, together with this description, I think, will make it plain. —*British Medical Journal, July* 22, 1871, *p.* 109.

AFFECTIONS OF THE EYE.

53.—CATARACT AND ITS TREATMENT BY SEMILUNAR CORNEAL INCISION.

By Jabez Hogg, Esq., Surgeon to the Royal Westminister Ophthalmic Hospital.

In placing before the Medical Society of London the results of thirty-three extractions by the semilunar corneal incision, and the experience of my last year's operations, I have been mainly influenced by a desire to show that this well-known method of extraction has in no way lost its hold upon the Surgery of the Royal Westminster Ophthalmic Hospital; neither has it forfeited the confidence it inspired before the introduction of alleged modifications and improvements, which we were told would supersede the old flap operation, and also that of more modern date, lineal corneal section, both of which were so successfully practised by Adams, Travers, Ware, Gibson, Guthrie, Mackenzie, and others equally well known in this department of Surgery. Indeed, what Mr. Travers wrote of semilunar section some fifty years ago is quite in accordance with my view of it at the present time. He says:—"This operation is by far the most perfect ever devised for the cure of cataract, but it is one of considerable difficulty, and the several modifications which have been at various times suggested owe their origin to the disappointments and defects which operators meet with in learning to execute it with success."

Soon after iridectomy was fairly established in the practice of eye-Surgery, it was proposed to engraft it upon all the operations for extraction of cataract, and this was followed by a (so-called) modified linear extraction, made by cutting out through the sclerotic, excising a portion of iris, and assisting the delivery of the lens by the aid of the hook or scoop. By such means it was said that the percentage of our failures would be considerably lessened. I am not so sure that published statistics bear out the assertion. Professor Hasner's statistics of, I believe, some 2000 cases give an average of 10 per cent. of failures. I believe that if the statistics of the flap operation in this country were carefully collected and compiled, it would give the same percentage, and not, as has been stated, 20 per cent. But whichever way this may ultimately prove to be, it is *pleasant to find that* the feverish stage of excitement which ushered in iridec*tomy has entirely* passed away, leaving us free to discuss it and judge of it solely on its merits, which are no longer denied in certain cases. In our profession, experience goes a very long way in toning down the most ardent and enthusiastic admirer of any particular operation or mode of treatment; and, however obstinately blind we may be to a weak point, time generally induces a more generous estimate of opinions thus gleaned. I am about to offer you my own experience on the flap operation, but I shall take care not to weary you by a too lengthy description of my cases, or mode of operating, which, in truth, differs very little from that with which, perhaps, most of you are perfectly familiar. Neither do I intend to make an extended comparison between it and other methods practised. I shall rather confine my remarks to the results of operations performed at the Royal Westminster Ophthalmic Hospital, between April and November of 1870.

It is pretty well known that the mode of performing the operation for the extraction of cataract, like every other in surgery, has undergone a variety of changes. The instruments also have received various modifications in form and character. Nevertheless, some of the earliest known modes of operating still maintain a place in our *ars chirurgicus.* The well-known couching (needle operation,) described by Celsus, and which was in vogue long before his day, is still largely practised in India and other Eastern countries. The removal of a cataract by extraction is of much later date, and was first regularly introduced by Daviel, a French surgeon, in 1747. In his excellent essay on "A New Method of Treating Cataract by Extraction," he acknowledges that he "caught the idea from Petit, who in 1708 opened the cornea to extract an opaque lens, which, having re-ascended after depression, had fallen into the anterior chamber, and that he felt himself urged to devise some new mode of operating by the want of success which he found to attend couching, and the destruction of the internal textures of the eye disclosed upon dissecting the eyes of those who had been operated upon in that way."

Daviel commenced his operation for extraction by passing a small, narrow, lance-shaped knife into the anterior chamber at the junction of the cornea and sclerotic. Curiously enough, Von Gräefe's modified linear extraction is commenced in the same way, and with a sharp, narrow-bladed knife; so that we are asked to return to a knife, and, I may almost say, an operation, which surgeons of great practical experience long since determined to abandon, for that more perfect knife introduced by Von Beer, and which seems to most of us so well adapted for the corneal incision.

The success of those who have preceded me in this field of surgery point to the brilliancy of the results obtained by semilunar section pure and simple; and, so far as my observation enables me to offer an opinion, I feel certain that, in cases free from local or constitutional complications, extraction by the old flap operation is to be preferred to any other. Moreover, it is the only one which secures a circular movable pupil to the patient—a matter of some moment —as in this way a limited power of accommodation for viewing distant objects is preserved; while, in all the more modern operations, preceded by an iridectomy, the pupil becomes fixed, and accommodation is very nearly destroyed. Extraction by semi-circular corneal section, then, possesses the advantage of rendering the sight clearer and quicker than most other methods, while its disadvantages are comparatively unimportant. One of these, particularly dwelt upon by those who do not follow this plan of operating, is that with a large semicircular section we are very likely to have an escape of vitreous. My answer is, that it is not always necessary to make a large corneal section; it should be made of a sufficient size to admit of an easy delivery of the lens, and not larger. The probable size of the lens can generally be fairly estimated by fully dilating the pupil before attempting the operation.

Dilatation serves another useful purpose: it lessens the danger of wounding the iris, and it renders the capsule more accessible, while it facilitates the delivery of the lens. I look upon the instillation of a drop of atropine a day or two before the intended operation as a precautionary measure—in another way—that of determining suspected adhesions; it, therefore, greatly assists a diagnosis, and clears up a doubt as to which operation will be the safest, or attended with the best possible results. A patient having once had a rheumatic attack, is very likely indeed to suffer from rheumatic iritis after the most carefully performed operation. An iridectomy in such a case will often be attended with a greater amount of success than the simple flap operation. In such instances, I do not scruple to resort to the two operations at one sitting rather than follow the practice of allowing a week to elapse between the iridectomy and the extraction.

In so much, then, my cases may be said to be selected; but this remark applies only in a very restricted sense, for it would be extremely difficult to say so when we have a frequent complication to deal with, a fluid vitreous, which might escape detection in any examination. By a careful inquiry into the previous history of the patient, we avoid the risk of failure from diabetic and other causes. I lately had a patient sent up from the country for operation with a fully formed cataract in the right eye, and the sight of the left much diminished. Upon making the usual enquiries into his general health, I discovered—what had previously escaped observation—that he was passing a considerable quantity of sugar in his urine. The opacity was due to the diabetic state of the patient, and any kind of modified operation would, in all probability, fail to restore vision.

In my thirty-three operations, three total failures occurred: these were due to causes over which it may be fairly said I had no control. In the case of Elizabeth H., admitted to the Westminster Ophthalmic Hospital, July 20, 1870, the house-surgeon reports:—"The operation, by the usual flap method, was perfectly successful, and everything went on exactly as could be wished until the third day, when the patient accidentally struck her eye rather violently, which caused considerable pain. Belladonna was applied and warm fomentations. The next morning there appeared a suspicious swelling of the lids, and on making a more careful examination, the corneal flap was found turned down. Inflammation ensued, and ultimately sloughing of the cornea. In the second case, a nervous restless patient uncovered the eye, and attempted to use it on the second day after the operation; inflammation and separation of the flap followed, and the eye was lost. In the third case, failure arose from an extremely fluid vitreous, the greater part of which escaped during the operation, causing collapse of the eyeball, and probably tearing the retina, as on the third day erysipelatous chemosis of the lids and deep-seated inflammation supervened, and the eye was lost.

It by no means follows that a large loss of the vitreous during the operation will be attended by such results as those just narrated, as I shall show you in the case of Maria C., admitted to the Hospital November 16. She was nervous about the operation, and, therefore, wished to have chloroform administered, and it was not until after a considerable quantity of the anæsthetic had been inhaled that she became unconscious and quiet enough to permit me to attempt the section. Having completed this, I was about to rupture the capsule, when she gave a plunge, and out popped the lens, followed by a considerable quantity of vitreous humour, and a prolapse of the iris. As, from the restlessness of the patient, the iris could not be returned, I snipped it off. On the following day the patient complained of much pain; this was relieved by the administration of full doses of opium. On the fourth day the bandage was removed, and the eye open, but as the patient complained of intolerance of light, it was reapplied. A few days later an examination was made, and the eye was found to be going on well, the pupil being quite clear, and she could count fingers. At the end of six weeks, with a convex glass of two and a half inches focus, she was able to read ordinary-sized print with comfort.

I may observe that I prefer, for two or three reasons, to operate without chloroform. In the first place, the natural resistance of the recti muscles gives firmness to the eyeball, and the corneal section is made with a rapidity and cleanness, if I may so express it, quite unattainable when an anæsthetic is administered. Sickness often follows the use of chloroform, and then the vitreous either escapes, or rupture of the hyaloid membrane or of the internal vessels produces hemorrhage, and seriously compromises the operation. The risk of sickness is however, lessened by the use of bichloride of methylene.

I look upon the loss of the vitreous as a much less serious affair than some operators do. I have repeatedly noticed that the eyeball has been restored to its normal size, after the loss of a considerable quantity, in twenty-four hours, and as complete a recovery made, with a good pupil, in an ordinary period of time, as if no such accident had happened.

Adhesions of the iris are not unfrequently the cause of an escape of vitreous, and, if we fail in detecting them, the operation is not only rendered more difficult, but the convalescence of the patient is liable to be retarded.

This was the case with a patient, aged 52, in whom I met with the additional difficulty of an unusually large lens, and attachments to the iris at its inferior border. Our house-surgeon reports of this case as follows:—

"Ellen H., double cataracts. On July 18, Mr. Hogg extracted a very large hard lens from the right eye. In attempting to deliver the lens, extensive adhesions were found to exist, and, on careful pressure being made, a large quantity of the vitreous humour escaped. With some difficulty the lens was scooped out.

"July 19. The patient complains of great pain, to relieve which I gave twenty minims of nepenthe, which in half an hour afforded considerable relief. The eyelid is a little swollen, but no tenderness of the eyeball. Ammonia and bark mixture prescribed three times a day, and four ounces of brandy in the twenty-fours hours.

"25th. Gradually improving.

"August 5. With a two and a half inch convex glass, can read ordinary print."

[The following are a few remarks by Mr. Hogg on his mode of performing the operation.]

The upper section has always been preferred at the Royal Westminster Ophthalmic Hospital, and very justly so. As I have already intimated, it possesses certain important advantages over the lower section—that of less easily permitting escape of the vitreous, while the support afforded by the lid certainly favours healing of the flap. Should irregularity of the pupil occur, either from unfavourable healing of the cicatrix or injury to the iris, it is concealed by the upper lid. After the operation, separation of the section and prolapse are less likely to take place from a fit of coughing or vomiting. If the patient, however, is unable to fix the eye in the proper position, and a real difficulty presents itself in attempting the section, by reason of the eyeball rolling upwards and inwards, this is readily overcome by directing your assistant to seize, with a pair of fine-toothed forceps, a fold of the conjunctiva, and gently retain it in position. Everything considered, although upper section may be rather more difficult of execution, this is counterbalanced by its greater advantages. Another difficulty will occasionally occur, and one less easily overcome. In attempting the section, the knife penetrates the cornea too obliquely; the point then traverses the lamellæ for some distance without at all entering the anterior chamber. In such a case, the knife must be withdrawn and re-entered, or an opening must be made from above downwards with a broad-shouldered triangular knife. Such an accident will be avoided if the knife is fairly poised between the two forefingers and thumb, and the point made to enter perpendicularly to the cornea and within it, at the distance of a line from the sclerotic coat. When the point has penetrated the anterior chamber, which is known by its brightness as distinguished from the dull appearance of that part of the instrument still remaining within the density of the cornea, and by the absence of resistance which is readily felt by the practised fingers, the handle is directed slightly towards the temple, so as to give the edge such a direction when drawn in a right line that the point shall come out at the part of the circumference of the cornea exactly opposite to the point entered, thus causing the knife to divide the cornea parallel to its edge, and within a line or two of the sclerotic. It is, I consider, a matter of considerable importance that the section should be purely corneal, and therefore it must not be carried low enough to merge into the sclerotic; a corneal margin should be left of a sufficient breadth for union.

The main impediment to success is, as I have already intimated, a section too small to admit of an easy delivery of the lens. The easy extraction of a cataractous lens, like the easy extraction of a stone in the bladder, depends upon a clean cut and a fair-sized opening; while a difficult and forcible removal is almost as certain to end disastrously. The enlargement of the section after the escape of the aqueous is dangerous to the iris and other membranes, often provocative of laceration of the hyaloid and a loss of vitreous. Should the lens sink down behind the iris to rise up in judgment against us, prolapse of this membrane is very likely to follow, and it becomes entangled in the lips of the wound. Any attempt to excise the iris after the lens has escaped will be followed by bleeding into the anterior chamber, causing not only protracted healing, but opacity of a portion of the cornea, and a dense secondary cataract from the combined effects of retained portions of capsule and serous exudation. When all goes well, very gentle pressure exerted on the eyeball, after rupture of the capsule, serves to deliver the lens, and the operation is complete. Before applying the bandage, it is necessary to separate the eyelids to ascertain if the pupil be perfectly clear. Should any portions of capsule be seen, these must be removed with a curette or scoop, care being taken not to use enough force to rupture the hyaloid, which, it should be remembered, is now bulging forward, and may very easily be torn.

By way of securing more perfect adaptation of the edges of the cornea, I take a very soft piece of sponge, and make a few circular movements over the closed eyelid, and, lastly, apply a pad of carded wool and a firm bandage. At one time it was my practice to close the lids with a narrow band of black sticking-plaster; but as this sometimes led to unpleasant consequences when there was occasion to remove it, I abandoned it for cotton-wool. The plaster is still preferred by some operators, and it certainly possesses the advantage of maintaining perfect adaptation of the lids, and secures the eye from the meddlesome interference of nurse and patient. I generally prefer to keep my patient in bed from four to six days; at the end of this period, if no inflammatory symptom appears, the bandage is partially removed, and he is allowed to move about. After the first forty-eight hours, little or no restriction is placed upon the diet, and the average duration in Hospital is under twenty days.

The construction of instruments employed in this, as in most other operations, is a point which everyone must be permitted to decide for himself. The knife known as Beer's is that more generally used in our Hospital. For my own part, I think it a little too broad towards the base; the angle it subtends is too great. I therefore employ a modified form of this knife, in shape somewhat between a Beer and Wenzel. It is, as you will see, rather smaller, but very well adapted to fill the wound it inflicts, which is a point of no little importance in making the corneal section, as it prevents the too hasty escape of the aqueous.—*Medical Times and Gazette, May* 27 *and June* 3, 1871, *pp.* 600, 630.

54.—TREATMENT OF GRANULAR OPHTHALMIA BY THE LOCAL USE OF QUININE.

By C. Bader, Esq., Ophthalmic Assistant-Surgeon to Guy's Hospital.

The unsatisfactory results of the treatment of granular ophthalmia, especially if combined with pannus (vascular cornea), will, I hope, justify the publication of the following remarks. Experiments were made, with various substances, on patients suffering from granular ophthalmia with pannus. The idea which led to the selection of the substances experimented with was, that granular ophthalmia is the result of some extraneous substance becoming lodged in the conjunctiva, and giving rise to what have been called granulations. Experiments made elsewhere with antiseptics on organic matter have shown quinine to be one of the most effective antiseptics.

The substances experimented with on granular ophthalmia were the scrapings and juice of the root of bryonia nigra, the nitric oxide of mercury ointment (hydrarg. nitrico-oxidi, gr. iii., adipis ʒ i.) and the bisulphate of quinine *powder*. *The* remedies were applied morning and evening; of the quinine about as much as would go on the point of a penknife was placed, with a dry camel's-hair brush, on the inner surface of each lower eyelid. No other treatment, such as exclusion from light, use of lotions, &c., was made use of. The hypothesis that quinine acts specifically upon granulations, as mercury, for instance, does on lice, may be erroneous; the effect may merely be that of an irritant, causing moderate suppuration, and, with it, removal of the granulations.

The effects on granular conjunctiva of bryonia nigra and of the nitric oxide of mercury ointment, though curious in other respects, are of little interest here. Suffice it to state those of quinine. In some cases its application was followed by severe smarting, which continued for ten or fifteen minutes; in other cases no pain whatever was felt; in all cases appeared increased purulent discharge from the conjunctiva, with shrinking of the granulations, and clearing of the surface of the cornea. The intolerance of light ceased rapidly in all cases; the dilatation of the pupils appeared in from twelve to twenty-four hours after the first application of quinine. The pupils, though dilated in ordinary light, contracted well on exposure to strong light.

Case 1.—A. B., aged fourteen. Granular ophthalmia for six years; at present vascular cornea (pannus) in both eyes; pupils barely visible; granular conjunctiva; extreme intolerance of light. The application of bryonia nigra was followed by symptoms of purulent ophthalmia (chemosis, swollen lids, &c.), which, with slight improvement of sight, subsided within ten days; the intolerance of light persisted. Quinine was used and the bryonia discontinued. The eyes improved rapidly; the granulations became smaller, and the intolerance of light disappeared completely within four days.

Case 2.—C. D., aged ten. Granular ophthalmia with pannus for two years, with intolerance of light. The use of the nitric oxide of mercury ointment (for fourteen days) was followed by slight improvement. It was discontinued and quinine substituted. Within ten days all intolerance of light had ceased, the cornea became much clearer, &c.

Case 3.—E. F., aged ten. Granular ophthalmia for one year. The intolerance of light persisted for three weeks while the nitric oxide of mercury ointment was used; it disappeared on the second day after the quinine had been applied.

Case 4.—G. H., aged thirty-four. Granular ophthalmia for one year, with pannus and intolerance of light. Quinine was used for ten days; on the third day the pupils became dilated, and on the fourth all intolerance of light had ceased.

Case 5.—I. J., aged eleven. Granular ophthalmia for four months, with slight pannus and intolerance of light. Quinine treatment for three weeks. All intolerance ceased; cornea much clearer; granulations red, much smaller; slight purulent discharge.

Case 6, aged fourteen, bad for three months; *Case* 7, aged fifteen, bad for four years; *Case* 8, aged ten, bad for two years, improved equally rapidly under the quinine treatment; in all did the intolerance of light disappear within from five to ten days, with marked improvement in the state of conjunctiva and cornea.—*Lancet*, *Oct.* 28, 1871, *p.* 604.

55.—ON THE USE OF CROTON OIL IN OLD OPACITIES OF THE CORNEA.

By Thomas Windsor, Esq., Manchester.

During the last few years I have from time to time tried a number of substances as local applications to the eye. Some of these, as croton oil, had not, to my knowledge, been employed previously; others, as the ol. sabinæ, had already been used on the Continent. All, except croton oil, seemed to me in no way superior to the medicaments commonly ordered.

This oil, however, proved to be of service in cases of old opacity of the cornea where the ordinary remedies caused little or no improvement. I must add that I never used it, until other means, including the pure turpentine oil, were found insufficient, so that I can at present only say that when the eye bears the latter without difficulty, croton oil, diluted with sufficient ol. olivæ, is an efficacious and yet manageable stimulant. I have generally used one part of it to seven of olive oil, commencing with a weaker form: I have never applied it stronger than in the proportion of one to five. The latter has, indeed, sometimes caused slight diffuse haziness of the cornea, which disappeared again in the course of a few days. Severe pain, much lachrymation, and great redness of the eye, usually come on in a few minutes. I let the patient bathe the eyelids freely with cold water when the pain is very severe.

To exemplify its effects, I may take a case of parenchymatous corneitis of a year's standing, in which, at the time of application, the periphery of the cornea was almost perfectly free from vessels. Three minutes after, one drop (dilution $\frac{1}{7}$) had been applied, there was a pale purplish blush, about two lines in breadth, around the cornea; the posterior conjunctival vessels were much enlarged, and there was free lachrymation. Eight minutes later the limbus conjunctivæ was much injected, the surrounding purplish blush was more intense, whilst the conjunctival vessels remained much in the same state. A few minutes more, and examination by oblique illumination showed that numerous small vessels connecting the anterior and posterior conjunctival vessels were distended with blood. Two days later the congestion had entirely disappeared.—*Man. Med. and Surg. Reports, Oct.* 1871, *p.* 56.

56.—THE TREATMENT OF WOUNDS OF THE EYE BY SUTURE.

By Thomas Windsor, Esq., Manchester.

[It was in the trials which were so often made between 1820 and 1840 to transplant the cornea from one animal to another, that surgeons first found themselves obliged to apply from one to four sutures to the eye for the purpose of accurately fixing the fresh cornea.]

Critchett, preceded however by Wilde, whose example attracted little or no notice, began about 1861 to suture the wound caused by abscission of staphyloma. When he published his paper two years later, the surgeons at Moorfields had treated thus about thirty cases, and suppuration had occurred in only four. The suture was generally left in for some weeks.

An American surgeon, Dr. Williams, has extended the use of the suture to flap extraction. He thinks that it obviates the danger of loss of vitreous after the operation, in case vomiting should occur, that it renders primary adhesion much more certain, that it removes nearly all risk of prolapse of the iris, that it renders the free use of atropine admissible, that it enables the surgeon to examine the eye at an early period with safety, and that it shortens the period of convalescence. He uses a single strand of the finest silk and a fine needle, a quarter of an inch long, with a flat cutting point. The needle is held by strong forceps, and passed as near as possible to the edges of the wound, which are held by very delicate forceps. Latterly he seems inclined to change his method a little; in a paper on this subject published in 1869, he remarks that, "at present I am disposed to extend the corneal flap at its apex a little way into the conjunctiva, so as to allow of the placing of the suture in this membrane, where it is more easily inserted than through the tougher corneal tissue." The suture, if left to itself, as was at first his usual practice, generally comes away in a few days; he thinks it, however, best to remove it in a week or ten days, though in more than one case it has remained for seven weeks without giving rise to any but trivial irritation. In no case—and in 1867 he states that it had been used in nearly 100 cases—did it do harm.

The only case in which a wound of the sclera had been sutured was, so far *as I knew when* the following case was under my care, one recorded by O. D.

Pomeroy, though I have since met with two in Mr. Lawson's book, and another has lately been published. To these I shall again refer. I may add that Von Walther just mentions the use of sutures in wounds of the eye; he dissuades from their use, though he makes no mention of ever having tried them. Pomeroy's case was as follows:—

"John S. N., aged 21 years, a soda-water manufacturer, was struck on the eye by fragments of a soda bottle which had burst, inflicting a wound of the sclerotic at the margin of the corner, about four lines in length, causing a prolapsus of iris (and perhaps of choroid,) an unsightly coloboma resulting. I easily succeeded in reducing the prolapsus, although it remained but momentarily. Two sutures were then passed through the conjunctiva, near the margin of the wound, and gradually tightened as the prolapsus was reduced; they failed, however, in reducing the whole, so a suture was placed between the two already in. The sclerotic aspect of the wound afforded a sufficiency of conjunctiva, but not so with the corneal; so the suture was passed through a portion of the sclerotic at its corneal junction, which closed the wound satisfactorily. The after-treatment consisted in the application of ice to the eye, atropine, and leeches. In fifty hours the lateral sutures were removed, when the wound was found to be well adhered. On the next day an effort was made to remove the central suture, but as the wound showed signs of gaping if much interfered with, it was not removed until the following day, when the wound was found to be well united. There was a little conjunctival injection, which disappeared in a few days. Patient sees as well, or nearly so, as with the other eye, which is normal."

This case came at once to my recollection when, on August 4th, 1870, I was called at the recommendation of Dr. Bower Harrison to see a child whose left eye had been injured two or three hours previously. The account given was, that whilst an elder brother was cutting twigs with a gardener's curved knife, large and blunt, the injured boy happened to stoop down, the knife slipped and was driven into his eye.

On my arrival I found a delicate-looking boy, very frightened and inclined to cry. Gently raising the upper lid, I found an incised wound, commencing in the inner and upper side of the cornea and extending through the sclera backwards for fully half an inch; there was a nodule of prolapsed iris in the anterior part; the rest of the wound was gaping widely, and through it the vitreous was bulging, though none appeared to escape.

The question of a ligature at once arose. It appeared to me, however, inadvisable to give chloroform, because the child was certain to cry and struggle, and the least effort would probably force a quantity of vitreous through so large a wound. I therefore decided to wait. A bandage was applied so as to gently compress the injured eye, and the other one was closed by plaster. He was kept in bed some time; hardly a trace of inflammation occurred, and the wound gradually contracted.

The progress being slow, I decided to remove the prolapsed iris. For this purpose Dr. Finlayson gave chloroform on August 17th, and I excised the nodule. This had a beneficial effect; the anterior part of the wound was soon firmly healed, the posterior part uniting also for a little distance further. In November the centre was still open and allowed the vitreous to project; the bandage was now tightly applied and continued till December 27th, when as the prolapse of vitreous rather increased than diminished, Mr. Fletcher giving chloroform, I excised the projecting nodule and passed a suture through the middle of the wound. As the edges of the sclera seemed to be thickened, I endeavoured to pass the thread obliquely through it, after the manner of suturing a vesico-vaginal fistula. The stitch was then drawn tight and completely closed the wound. The eye was kept at rest by a bandage. Next day there was considerable injection of the parts immediately around the wound, accompanied by some pain. On the 30th the congestion was less, and the suture had cut partially through.

A day or two later the suture came away spontaneously, and on January 6th the wound appeared completely closed. On the 20th the cicatrix was quite level, and all irritation had disappeared.

My last note is, "February 24th—Wound appears firmly healed. Pupil of good size, but a little displaced inwards, with a rather large coloboma extending to the margin at the inner and upper side; it reacts moderately to light. He reads slowly No. 5. and letters of No. 4 (Jäger.) The lens is not displaced. It, the other media, and fundus are normal."

I directed that the bandage should be gradually discontinued, and have not seen him since. The other eye remained throughout normal.

Mr. Bowman's cases were equally satisfactory. In the first one, John M., aged 25, was admitted into the hospital, October 31st, 1863, on account of an injury of the left eye, received three days previously. When admitted, there was a horizontal wound in the sclerotic at the inner side of the cornea, about one-sixth of an inch in length. The edges were not in apposition, and through it vitreous was oozing. The anterior chamber was excessively deep; the tension of the globe was—T^2 and was daily diminishing. He could count fingers at from six to seven feet.

Mr. Bowman performed iridectomy at the inner side, and then the edges of the wound in the sclerotic were brought together by a very fine suture, the needle being made to pass through each lip of the wound from within outwards.

The man had a good deal of pain the second night after the operation, but it was relieved by the application of a leech to the temple. On November 4th the iris had resumed its proper place; the tension was normal. The thread had come away. He could see letters of 16 Jäger's types. He left the hospital November 25th, and could then read letters of No. 10.

Mr. Lawson gives the following account of the second case:—"Mary S., aged 9 months, was brought to the hospital on July 7th, 1863, suffering from an injury to the left eye. A piece of china in falling off a shelf struck the eye, and inflicted a small wound in the sclerotic, close to the corneal margin. The wound (notwithstanding that a week had passed since the accident had occurred) was still a gaping one: the edges of it not being in apposition. Mr. Bowman applied a single suture, and with it brought the lips of the wound accurately together. No irritation followed; and on July 21 the child ceased its attendance at the hospital, the eye being quite well."

Mr. Lawson has lately published a case that had been under his own care: W. G., aged 40, was admitted on March 5th, 1869. There was a wound in the sclerotic at the lower part of the left eye, a short distance from the cornea; this had been caused by a blow of a stone. A small quantity of vitreous had evidently escaped. The wound gaped; its lower edge was prominent, and stood away from the upper margin, which was somewhat depressed. A single thread brought the parts into apposition. The patient progressed most favourably, and the wound completely united; the stitch produced no irritation. On April 27th there was no redness of the eye, which seemed to have recovered completely; he could however, only count fingers. An extensive detachment of the retina was found to correspond with the site of the puncture.

These cases require no lengthy commentary; the effect of the suture has been in them too palpable to leave room for doubt. I think it is clear that whenever a wound of the sclera or cornea gapes, a suture should be applied, unless there is some special contra-indication. Chloroform should be generally employed, and the thread should be very delicate. If it causes little or no irritation, it will be usually the best plan to wait for its spontaneous discharge.

The principal objections to the suture are—the frequent necessity of chloroform for its insertion, and possibly for its removal; the difficulty of inserting it; and the risk of its exciting inflammation. The latter appears to be very slight judging by the cases hitherto recorded.

I may conclude with the suggestion that, as union at once followed in my case, though the edges of the wound had been separated for many weeks, similar treatment would not be unlikely to have a similar effect in fistulæ of the cornea and sclerotic, which it has been hitherto found to be so difficult to close. —*Manchester Medical and Surgical Reports, Oct.* 1871, *p.* 3.

DISEASES OF THE SKIN.

57.—ON AURICULAR HERPES ZOSTER AND ITS TREATMENT.

By Dr. F. E. Anstie, Senior-Assistant Physician to the Westminster Hospital.

[During the week or two preceding the writing of this paper, Dr. Anstie had been the subject of herpes zoster in an unusual form, and the apparent results of treatment involved a therapeutic fallacy, which it is instructive to investigate. For about twenty-three years, Dr. Anstie has been subject to attacks of neuralgia in the right trigeminus, the severity of which however has of late years been much diminished.]

On August 24th I began to suffer most acute pain in front of the tragus of the right ear, which came and went several times in the day, and which darted from the place where it commenced into the meatus auditorius and the maxillary joint, and ran up the side of the head. I concluded that I had caught cold from unusual exposure to draughts in a house that was undergoing repairs, and at first was not certain that there would not be abscess of the meatus; but in the course of forty-eight hours it became quite obvious that that was not the nature of the malady, for the pains recurred with great regularity four times in each twenty-four hours, and in the intervals the parts were quite free from pain; besides, they could be handled, at any time, without showing tenderness. On the 29th the pains became so violent as to necessitate the use of morphia whenever they recurred, and had they been merely superficial I should have endeavoured to stop them (as I have repeatedly stopped my attacks of frontal neuralgia) with the constant current: but I shrank from the application of the constant current deeply within the meatus (which must have been done), from an uncertainty as to its physiological effects upon the auditory nerve. On this the sixth day of the neuralgia, I begged Mr. Carter to examine the ear with the speculum; he found the meatus and the membrana tympani perfectly healthy, without redness or tumefaction. The pain now was at its acme of severity, and the paroxysms extended not only into the branches of the auriculo temporal, but into the labial branches of the inferior dental nerve, and passed over into the superficial cervical nerves, running quite down to the right shoulder. The intractability of the affection puzzled me greatly. On the 31st August Mr. Carter, examining the meatus again, reported that there was now some slight internal tumefaction and redness; but it seemed difficult to say if this were not partly caused by local applications which I had used, in desperation, during the attacks. I now requested Mr. Hinton to examine the ear; he did so, and verified the moderate tumefaction, and thought he detected a very small "pimple," but believed that it was merely the effect of local applications, and that the case was one of pure neuralgia. Mr. Hinton advised quinine, the use of soothing unirritant applications, and the exclusion of air. Accordingly I treated the periodic attacks with the continuous application of hot poultices covering the entire ear and the painful point in front of the tragus (where there was, by this time, a veritable *point douloureux* of Valleix), and the use of large doses of quinine. In the course of forty-eight hours I had taken nearly sixty grains of quinine; and by the end of that time a striking amendment was produced, so striking, in fact, that it seemed impossible to avoid the inference that the quinine had caused it. (No discharge of pus whatever had taken place.) I discontinued the poultices, and on the 6th September ventured also to remove the cotton-wool with which I had plugged the ear, in the intervals of poulticing, to exclude cold; but soon found that the part was too sensitive to admit of this being done with impunity. My attention was now attracted to a few small vesicles in the central folds of the pinna, and a little group of two or three at the external edge of the cartilage; the latter, being rubbed, ulcerated and became very painful. A draught of cold air on the spot would momentarily set up the old neuralgic pain. At the same time a small group of characteristic herpetic vesicles formed below the right angle of the mouth, and invaded the membrane; unluckily, in forgetfulness, I irritated these by smoking a pipe on that side of my mouth, and they coalesced to form one rather nasty ulcerated pustule. It was only on September 11th that I discovered that there had also been a few vesicles (then dried up) on the neck, over the right sterno-mastoid; though I had noticed some soreness in washing and wiping the part. By the 13th the vesicles had everywhere completely disappeared, and all tendency to neuralgic pain had also apparently left me.

The first remark on this narrative that occurs to me (beyond the reflection that had the case been anyone else's than one's own, one would perhaps have discovered its nature much sooner), is that the apparent curative influence of the quinine was probably altogether delusive. The time during which it was apparently producing such marked effects was evidently that during which the herpetic eruption was beginning to come out, and that is, I need not say, a period at which the violent pain of neuralgic herpes often ceases: in fact, in the majority of cases that I have seen, the pain was then at least temporarily suspended. To say truth, the decidedly beneficial action quinine had seemed to exert had greatly surprised me, for a long experience, especially in the treatment of my own case, had led me to an utter distrust in quinine for non-malarial neuralgias; and I had even begun to speculate whether, in a recent shor trip to Westmoreland, I had not caught a slight taint of malaria, from which the neuralgia had resulted. I think this idea may now be entirely set aside, and the explanation adopted that the relief of the pain was altogether spontaneous.

The true treatment for such a case seems to me not at all doubtful, since it is the same which I invariably adopt with success in ordinary shingles attended with neuralgia. Morphia should be given hypodermically, ⅛ grain twice a day; local injection (near the trunk of the auriculo-temporal nerve) is for once to be preferred; and at the same time the painful parts should be hermetically sealed from the air. For the external parts collodion flexile constantly re-applied is the best thing; within the meatus, a good thick coating of simple ointment (put on melted with a brush), and an external plug of cotton-wool, should be used. The grand points are, to exclude the irritation of external cold, and to prevent the neuralgic pain from rising to any great severity: with this treatment I believe that zoster may always be kept within bounds, and the vesicular eruption prevented from developing to any large extent. And, what is very important, the vesicles are prevented from becoming open ulcers, a condition which is almost certain to involve a troublesome revival of the neuralgic pain.

The other remark to which the above related history gives rise is pathological, and has only an indirect bearing on treatment, but I may be excused for introducing it, because the rarity of the case makes it important not to lose any lessons that it may suggest. I say the *rarity* of the case—but my very point is, that such cases are perhaps not so uncommon, there being a possibility, and even likelihood, that the majority of them simply escape recognition. The idea that has occurred to me is this: that a large proportion of cases of earache, which are generally set down as examples of suppurative inflammation of the meatus auditorius, are in reality *nothing but neuralgic herpes, depending on an affection of the auriculo-temporal nerve.* What is the history of common earache? So far as I have seen, there are comparatively few cases in which the pain begins very gradually and then continuously increases up to a moment at which pus is discharged and immediate relief obtained; yet that should be the invariable order of events in the case of suppurative inflammation of the lining of a narrow bony canal like the meatus auditorius. Inflammations occurring in organs where there is room for expansion, may, and usually do, cease to be actively painful during the middle stages of the process, and only become painful again (with a throbbing agony) when pus is actually forming. But in a confined space like that within the meatus auditorius, the presence of pain is continuous, since the products of inflammation very early begin to produce severe tension. But in ordinary earache, for instance that to which children are liable, the course of events is, I believe, quite different from this. The pain often comes on in the first instance violently; the patient cries out with the severity of the suffering; but then there are remissions, amounting often to complete intermission. Moreover, so far as I have seen, it is but rarely that a well-defined discharge of pus is observed, even when relief to the pain has been somewhat suddenly obtained, as is not unfrequently the case. Indeed I have often been exceedingly puzzled by my entire failure to detect more than a very moderate amount of tumefaction within the meatus, and by the fact that the closest observation could not find a single globule of pus on the poultice or other dressing applied to the ear at the time when the pain became suddenly mitigated. On the contrary, there very frequently is found, what I observed in my own case now related, a shedding of small scales, either dry or moistened with a very little bloody sanies, from the lining of the meatus, a few days after the cessation of the pain: in fact, what looks exactly like the dead epidermis of vesicles that have faded. The whole course of events seems to correspond much more nearly with that of auricular neuralgic herpes, than with that of common abscess of the lining membrane of the meatus.

Should this suggestion prove correct, and should it be established that a neuralgic auricular herpes is the cause of a considerable proportion of cases of earache that are commonly supposed to be suppurative on account of the sudden way in which relief is at last obtained, some very interesting further considerations will be opened up. Earache is comparatively common among children, whereas the ordinary forms of neuralgia are exceedingly rare, before puberty. But on the other hand, shingles attacks children as frequently as adults; a very common age for it is between 8 and 10. Ordinary unilateral herpes is, however, not attended by actual neuralgic pain, in children; although there are highly disagreeable sensations (especially previous to the outbreak of the eruption) resembling, for the most part, very hot water running about underneath the skin. Why should *auricular* shingles alone, be attended with true neuralgia in children? as would be the case on the hypothesis which I have put forward. This is, no doubt, a difficulty, and for the moment I see no answer to it; but the mere circumstance that there is a difficulty in understanding why so uncommon a phenomenon as true neuralgia should attend this one form of herpes in children, does not alter the fact as to which I am confident, that in a vast number of cases of earache, in children as well as in adults, the pain is intermittent in character, and there is never any evidence of that discharge of pus which is presumed to account for the sudden departure of the pain. Under these circumstances, what seems like an anomaly ought only to serve us as a starting point for fresh inquiries which may perhaps lead us to interesting results; and I beg to throw out one suggestion that may be valuable. What is the meaning of the greatly higher irritability of the inferior maxillary (of which the auriculo-temporal is a branch) and of the superior maxillary, than of the ophthalmic division of the fifth nerve in childhood? for that this higher degree of irritability exists is an unmistakeable fact. How exceedingly rare is neuralgic headache before puberty; and how very common, *in children of a nervous temperament*, is toothache, from a very slight amount of caries, assuming all the intensity of a severe neuralgia, and darting into the ear, the throat, and the cervical nerves! I think it may be unhesitatingly said that the superior maxillary and the inferior maxillary nerves are the first of all sensory nerves in the body to exhibit any tendency to pain of a grade approaching the severity of true neuralgia: and I can only suggest that the reason of this is, possibly, the depressing and exhausting influence exerted upon them by the growth of the teeth. But in truth we are as yet only just beginning to see the great importance of the questions that are connected with the comparative dates, in the physiological life of individual portions of the nervous system, at which those portions respectively exhibit certain phenomena, such as the tendency to pain, or convulsive action, &c.

Meantime, if my suggestion—that ordinary earache may be, in a considerable number of cases, merely auricular herpes zoster—be correct, it ought to produce certain practical consequences by which it can be partly tested. It must surely be unnecessary, or even hurtful, to apply leeches: a business which is always tedious and uncomfortable, and which (when the application is to the mastoid) sometimes leads to an amount of loss of blood which a delicate child can very ill bear. The only remedy of any value beyond the hypodermic injection of morphia, and the sedulous exclusion of air from the skin of the whole affected nerve-territory, would be distal or reflex stimulation, by the application of mustard or cantharides over the branches of the great occipital nerve, in the "occipital triangle" of anatomists. There is reason to think that this might sometimes mitigate both the acute pain and the tendency to extensive vesiculation. But it would be necessary to avoid making a very severe impression, for in the latter case there would be some danger of setting up erysipelatous inflammation of a large tract of the neck, face, and scalp; a consequence which I once saw follow a too violent and extensive blistering of the nape for the relief of supra-orbital neuralgia.—*Practitioner, October* 1871, *p.* 198.

58.—ON THE TREATMENT OF GANGLION AT THE LONDON HOSPITALS.

The treatment of the common affection called ganglion, simple as it may appear and is generally supposed to be, is still a subject on which a very considerable difference of opinion exists among surgeons, as may be seen by a perusal of the following notes. It was with the view of gathering the opinions of hospital surgeons on the various methods of treating this affection, that we thought of collecting a few notes on the subject. We have been by their kindness enabled to obtain a mass of valuable information, containing the experience of most of the London hospital surgeons; and shall endeavour to procure similar notes from provincial hospitals and from Scotland and Ireland. We shall be glad to receive at the same time short notes of interesting cases of ganglion, for publication, in connection with our report on the general treatment of the affection.

King's College Hospital.—Mr. John Wood treats ganglion by the following plan:—A spear-pointed needle, cutting on both edges, and mounted on a handle, is passed into the cyst, and made to transfix it again and again, so as to let out the synovial contents into the areolar tissue of the surrounding fascia. The needle is then made to scarify briskly the interior of the cyst, and is used pretty freely in dividing the cyst-wall at its opening of communication with the sheath of the tendon. Pressure is then made with both thumbs upon the tumour, so as to squeeze out completely its contents, partly into the subcutaneous areolar tissue, and partly out through the opening in the skin by which the needle entered. Iodine paint is then applied thickly over the surface, and upon it a thick pad of lint, over which firm pressure is made by a bandage. This is kept on for several days, after which the iodine paint is again applied, and the pressure readjusted. After a few applications in this way, the tumour seldom reappears; and, if it do so, a repetition of the process rarely fails to succeed. No case has been met with, out of many hundreds treated, in which suppuration or any bad results have followed this plan; but several cases in which a seton had been employed have given rise to much trouble and danger from erysipelatous inflammation and abscesses, followed by stiffening, and in some cases permanent impairment of the use of the limb.

Mr. Henry Smith meets with a large number of cases of ganglion in the out-patient department of the hospital. After having tried various means of cure, he has come to the conclusion that the most effectual is that of operation by the seton. He passes a single ligature thread through the cyst, and allows it to remain according to circumstances. In some instances, severe inflammation and even suppuration will be produced in forty-eight hours, and then the thread is to be withdrawn. In the majority of instances, however, especially when patients are careful not to use their hand, the seton may be retained for a period varying from three days to a week or more, without producing any inconvenient symptoms; but, so soon as suppuration takes place, Mr. Smith withdraws the thread, and the cure is almost invariable. It is necessary to bear in mind in this treatment, that, in some constitutions and under certain conditions, the presence of the seton may produce very severe consequences; in fact, this is the only objection to the treatment. With care this rarely occurs; and there has only been one instance amongst Mr. Smith's patients at the hospital where bad results did happen. This was in the case of an unhealthy man, who applied, with a ganglion as large as a crown-piece on the back of the wrist. Mr. Smith passed a seton. The patient did not apply until after four days, and in the meantime most violent inflammation and suppuration occurred. Free incisions were necessary, and the wrist-joint itself was threatened for a time; but the use of a splint, and careful treatment prevented any mischief. The patient, however, was compelled to remain under treatment for several weeks.

Mr. Royes Bell often finds that a ganglion is too tough to be burst by any reasonable amount of external pressure, or is so situated that this form of treatment is not applicable; and that it may be radically cured by a puncture with a grooved needle, squeezing out the contents as perfectly as possible, and rubbing the sides of the cyst together. Firm pressure must be applied by means of a pad formed of lead, gutta-percha, or a piece of money, tightly strapped and bandaged over the site of the ganglion. Should this treatment not succeed, he prefers lacerating the walls of the ganglion subcutaneously with a strong needle with cutting edges, to the use of the seton. He has seen many excellent results follow this method, which is less severe than the seton, though the latter no doubt is the more radical form of treatment. He has seen a severe case of ganglion connected with the flexor tendons of the wrist, and passing with them under the annular ligament, cured by laceration. Mr. Bell has also evacuated a ganglion by means of an incision, squeezing it roughly, so as to get rid of all its contents. In this case, the ganglion returned. These swellings have a tendency to recur; and in weakly persons, after a radical cure of one, another and a fresh one may form. For ganglia which resist these simpler plans of treatment, the seton remains, which Mr. Bell thinks less manageable than the other methods. In passing a seton through a ganglion at the back of the wrist in an out-patient, he advises the hand and wrist to be placed on a straight splint for the time being. In the severe form, affecting the flexor tendons under the annular ligament, the patient ought to be in hospital, under constant observation and control, as out-patients are proverbially careless. The after-treatment, by carefully-applied pressure, to be kept up for several weeks, is of considerable importance.

University College Hospital.—Sir Henry Thompson applies, for ordinary and recently-formed ganglia about the wrist, tincture of iodine for four or six weeks, usually with good effect. If they resist this, he passes carefully through the centre, with a sharp needle, a double thread of silk; ties the two ends in a knot, squeezes out the contents by the needle-opening, and leaves the thread in for three days, applying water-dressing. At the end of that time, if a purulent discharge be seen, and a little inflammation have taken place, Sir Henry removes the thread, and applies water-dressing. Almost always, there is no more trouble with the ganglion. If little or no action be produced by means of the tiny seton, he leaves it in a day or two longer. Sir Henry has never had occasion to regret this but once. An out-patient at the hospital, who did not attend at the end of three days, returned a week after the operation with erysipelatous inflammation of the arm. She did badly, and got some permanent stiffness of the hand in consequence. That is the only unfortunate event among a great many cases which Sir Henry has thus treated; and, had he seen her at the end of the three days, he has no doubt all would have gone well.

Mr. Christopher Heath finds ganglion affecting the extensors of the wrist to be a common affection among the out-patients of University College Hospital, and readily amenable to treatment. If the cyst do not yield to the pressure of the thumb, steadily exerted for a minute or two, Mr. Heath is in the habit of puncturing it with a grooved needle, and evacuating the jelly-like contents. He has never seen any harm result from the practice, and finds the subsequent application of iodine paint for a few days apparently prevent the refilling of the cyst. Ganglion of the extensor tendons on the back of the foot is by no means rare; and within the last few months Mr. Heath has seen the disease in more unusual situations—viz., deeply in the ball of the thumb, in connection apparently with the tendon of the flexor longus pollicis; and again behind the external malleolus, in connection with the peronei tendons. In both instances, a puncture settled the diagnosis and concluded the treatment simultaneously; in the latter case affording great relief to the mind of the patient, who had been assured, by a leading authority on deformities, that the tumour was fibrous, and would require a serious operation for its removal. In the compound ganglion of the flexors of the wrist, where the swelling forms above and below the annular ligament, and the fluid contains bodies like rice or melon-seeds, Mr. Heath has evacuated the fluid by a puncture, taking care to keep the hand on a splint; and when this has failed, as it generally does, to effect a cure, he has employed the seton with success.

St. Bartholomew's Hospital.—Mr. SAVORY treats those sacs filled with glairy fluid, which are so apt to form about the wrists and hands, by puncture, complete evacuation of contents, and firm, equable, and continued pressure, as described by him in the second volume of *St. Bartholomew's Hospital Reports*, p. 92. Mr. Savory treats ordinary cases of enlarged bursa over the patella and these ganglia in the same way.

Mr. THOMAS SMITH believes the best way to cure ganglion to be to rupture the cyst by forcible compression, if possible (not by a sudden blow), and to keep a pad and bandage applied for some days afterwards. Even where the ganglion does not suddenly collapse, the effect of a hard squeeze may be to cause gradual absorption. When the synovial cyst is too tough to be affected by the previous plan, he is in the habit of making a subcutaneous section of the entire cyst-wall with a fine tenotomy-knife, and keeping up pressure for some days. If, from any objection on the part of the patient, or from other causes, this cannot be done, he has occasionally cured the disease by blistering with ointment of biniodide of mercury. He has no experience of injecting, excising, or passing setons through ganglia.

Of the treatment of ganglion (as distinct from the recognised bursal swellings) at St. Bartholomew's, Mr. WILLETT says that attention must first be drawn to the frequency with which this affection is met on the dorsal aspect of the wrist and hand. For its cure, Mr. Willett relies on forcible dispersion, either by rupture of the sac by the thumbs (which, by the way, requires dexterous manipulation, rather than any great amount of force), or, this failing, by puncture with a Paget's knife. But, whether the ganglion be ruptured or punctured, the essential element of success lies in the complete evacuation of the cyst's contents; and, to ensure this being done, tolerably rough handling in the way of pressing and squeezing the sac may be employed. Then the application of a compress of lint and roller completes the operation. Mr. Willett believes that, when dispersion fails, the reason is, that the operator has been content with simply rupturing the cyst; but, if proper care be only taken to empty the ganglion thoroughly this plan of treatment may be regarded as almost invariably successful. With regard to the so-called ganglion affecting the sheath of flexor tendons at the wrist, Mr. Willett coincides with the opinion that, should operative measures become necessary, it is desirable to lay open the swelling above the wrist tolerably freely.

Guy's Hospital.—Mr. POLAND adopts every possible variety of measure, with success and non-success. Puncturing and emptying out the contents, with subsequent pressure, has had very satisfactory results, but not always so; and when failure has resulted, he has laid open the ganglion, with a cure resulting.

The treatment of ganglion which Mr. HOWSE adopts varies with the nature of the cyst. If it be small and the cyst-wall thin, he thinks that forcible compression, so as to produce rupture, will give the best results. If, however, it be a large thin-walled cyst, close under the skin, without the distinct outline which such tumours generally present—tending, in fact, to become diffused—then repeated blistering nearly always gets rid of the affection. In cases of the above description, which are only slowly amenable to the blistering treatment, the cure may be much expedited by puncturing the cyst. There are, however, a certain number of cases which are not curable by any of the above methods; where the cyst-wall is thick, and not capable of being ruptured, or where it is situated under dense fascia, as in the palm of the hand. Such tumours are often complicated by the presence of a large number of "millet-seed bodies." These are, he thinks, best and most expeditiously treated by excision of the cyst in the antiseptic mode. The usual objection to this plan of treatment is the fear of diffuse inflammation supervening. The antiseptic method, however, entirely obviates this objection, and with its aid he has no fear of opening the sheaths of the tendons even extensively. In this operation, Mr. Howse has found it no objection that the whole of the cyst-wall cannot at all times be removed. In spite of its presence, the wound will generally close by adhesion, and not a drop of pus be found. Even in puncturing a ganglion he would generally adopt the antiseptic method, considering it safer so to do. In most cases Mr. Howse prefers excision of the cyst to injection with iodine or any other irritant, having once or twice seen a good deal of inflammation set up by its means. For the same reason, treatment by setons is, he believes, not a very safe mode of procedure.

Mr. Davies-Colley's usual practice has been to disperse ganglia by pressure; and, if they are too hard to be treated in this way, to blister them.

St. Thomas's Hospital.—Mr. Le Gros Clark treats ganglion when in the wrist by puncture, subcutaneously, with a narrow lancet, or broad spear-shaped needle and scratches the interior of the sac. He then presses out the contents beneath the skin, and subsequently applies a lead compress for a few days. This rarely fails, is almost painless, and is, in his experience, unattended with risk. When on the hamstring muscles, over the great trochanter, &c., he finds repeated blistering the safest and most efficacious treatment, or a blister kept open with iodine-ointment. Opening these large ganglia is not unattended with risk. It is safer, if opening be intended, to blister first. On the front of the wrist, and extending into the palm, blistering may be tried. Mr. Clark has not often been successful when they are large in this position. If opened, it should be by a free incision in this position. He prefers this to seton. Mr. Clark has never injected a ganglion, nor has he found it necessary to dissect one out.

Middlesex Hospital.—Mr. Campbell De Morgan prefers to leave slight cases of ganglion alone. If troublesome on the back of the wrist, he fairly cuts through them subcutaneously, and keeps on pressure. The large ones on the palm of the hand, if he interferes at all, Mr. De Morgan lays open fully, saving the annular ligament; and dresses with some balsamic tincture. Enlargements of the bursa patellæ he treats with rest and iodine. If they be inflamed and suppurating, he lays them open. When they are indolent, he uses puncture and rest, sometimes setons, though Mr. De Morgan states that he has seen mischief from these. He has seen great good from blistering. Ganglia in the popliteal space he never touches, if he can help it. If it be necessary to interfere, the greatest care should be taken to preserve rest. Inflammation is often propagated to the joint from them.

Mr. Nunn treats ganglion in the following manner;—Ganglion—that is to say, circumscribed inflammation of synovial tissue in juxtaposition with the sheaths of the tendons, forming a tense roundish tumour, more or less moveable—if not capable of being dispersed either by steady pressure or by sudden force, rupturing the sac and extravasating the contained fluid into the surrounding cellular tissue, cannot be treated off hand, like an abscess or a boil, by puncture, without considerable risk of diffused inflammation involving the sheaths of the tendons and intermuscular cellular tissue. As ganglion is frequently caused by excessive movement of the tendons in their sheaths—as in persons training for professional pianoforte-playing—it is difficult to imagine that ganglia are so completely independent of the synovial structure lining the proper sheaths of the tendons as is generally accepted. One might suppose that a pouch could be commenced by a slight effusion of fluid in the sheath, distending the delicate true synovial membrane through a gap in the investing firmer tissue, and that this pouch could become a cyst, as in well-known parallel examples, by obliteration of neck of sack. The fact is (as far as Mr. Nunn's experience goes) that the ganglion is usually, if not invariably, found on the back of the hand. Where the sheaths of the tendons are undefined, and apertures common, Mr. Nunn understands that circumscribed effusions within the sheaths or flexors or extensors should not be included in the term *ganglion, although* in many cases, doubtless, essentially of the same nature. These swellings are, on the other hand, sometimes also due to constitutional causes. Subcutaneous puncture, or rather incision, is, in cases not to be ruptured by blow or pressure, probably the best next step, but may require repetition, and should not be done without using a splint as a precaution. The subcutaneous puncture is probably quite as safe as severe counter irritation—a plan which Mr. Nunn has seen tried. But, before using even subcutaneous puncture, he would advise diligent trial of alternate douches of very hot and cold water; this will remove almost with certainty any tenderness along the course of the tendons, and diminish the ganglion if it will not get rid of it Mr. Nunn has never met with results at all satisfactory from the employment o blister, or of localised pressure by pad and strap.

The mode of treatment which Mr. **George Lawson** adopts for the small ganglia on the extensor tendons of the wrist is, first, to try if he can rupture them by firm pressure with his two thumbs whilst the hand is laid upon the table and then by steady rotatory rubbing to cause the contents of the cyst to b extravasated into the adjacent cellular tissue. He then paints the part with strong solution of iodine, and applies a firm pad and a bandage. When however, the ganglion resists the pressure of the thumbs, and cannot in this way be ruptured, Mr. Lawson introduces a tenotomy knife through the skin at short distance from the ganglion, and lays it freely open subcutaneously an then, by pressure with the fingers, evacuates its contents into the surroundin tissues. The parts are then painted with iodine, and a pad and bandage applied as above stated. Mr. Lawson strongly deprecates the plan of using setons fo the cure of ganglia, as on two occasions he has seen the hand nearly lost from diffuse cellulitis which followed this mode of treatment.

St. George's Hospital.—Mr. **Henry Lee**'s plan of treating ganglia is to puncture them subcutaneously, and to press out their contents into the subcutaneous cellular tissue every day or two until it ceases to reaccumulate. Mr. Lee lately treated in this way, with success, a ganglion as large as a French walnut, on the instep, of many years' duration.

Mr. Rouse bursts the cyst, when recent, with his thumbs, and well rubs the surfaces together; and in many instances this succeeds. When ganglia are of long standing, he punctures them, squeezes out their contents, then rubs the surfaces, and subsequently blisters or uses liniment of iodine. If this fail, he again punctures, blisters and dresses the sore with blue ointment. This last generally succeeds, especially if the sac be thick. In those rare cases in which melon-seed bodies exist, which are usually found on the anterior surface of the wrist, Mr. Rouse believes that no treatment save laying open the sac and dressing it in is of any avail. He has had no experience in dissecting out the sac or laying it open and dressing in with lint, in the ordinary forms of ganglion.

London Hospital.—Mr. **Walter Rivington** invariably punctures the swelling with a fine pointed bistoury, squeezes out the fluid thoroughly, and then applies a compress tightly for a few days. Failure to cure is in his experience rare.

St. Mary's Hospital.—Mr. **Gascoyen** restricts the use of the term *ganglion* to a swelling which is connected with the sheath of the extensor tendons on th back of the wrist, and on the dorsum of the foot. He does not regard this a an independent growth, but as a protrusion or hernia of the synovial membran through the fibrous sheath of the tendon; and considers that its enlargemen is produced by the gradual escape of the synovia into it during the movement of the tendon. Mr. Gascoyen therefore advocates a subcutaneous method o treatment and adopts the old plan of rupturing these swellings by pressur with the thumbs or by a smart blow, afterwards applying a conical pad with bandage. This treatment he has generally found to effect a cure in recent cases In those of old standing, when the above means fail to disperse the tumou he recommends free incision with a tenotomy-knife, introduced in a valvula manner through the skin; and the application of firm pressure in the sam manner after evacuating the contents. When these tumours have existed fo a long time they have, however, a great tendency to re-form after any plan o treatment. Blisters, iodine, and other stimulant or absorbent applications have seldom proved of service; they merely diminish for a time the size of th swelling. The very severe inflammation which often follows the use of setons in these cases renders their employment undesirable. Mr. Gascoyen deprecates excision of these swellings, except in those few instances where the tumour, having become solid, is disposed to ulcerate from friction or some other cause. In these cases, though the connexion between the ganglion and the sheath of the tendon has become severed, so long as the contents remain fluid, showing that there is a communication with the interior of the sheath, he is strongly opposed to any attempt to remove them.

Charing Cross Hospital.—The treatment of ganglion which Mr. **Hancock** has found most successful, and which he now always adopts, is subcutaneous section—making, as far as possible, a crucial incision; and afterwards gradual compression, with rest of the part.

Mr. **Bellamy** has found that, in the case of the ordinary non-inflamed ganglia generally met with about the dorsum of the wrist or foot, a good smart squeeze with the thumbs, one on the top of the other, is sufficient to disperse the contents of the sac into the surrounding tissues. This, however, appears to be effective only in ganglia having very thin walls, and situated either above or beyond that point where the annular ligament is thickest, or exists merely as fascia. In ganglia that have existed for some time, where the walls are thick and the cyst frequently multilocular, a puncture should be made, but in a peculiar way. A sharp-pointed tenotomy-knife should be introduced flatwise through the skin, and caused to traverse the cyst or cysts, making a free opening in them. Pressure with the thumbs will then readily diffuse the contents, and a pad and bandage will frequently effect a cure. In other cases, where the thick annular ligament of the wrist is lifted up by a subjacent cyst, Mr. Bellamy prefers free incision, pushing the knife straight into the sac through the annular ligament and integument, dividing these structures thoroughly; the contents are then evacuated externally, and a strong pad or compress is kept on for some time. It is remarkable how rarely even the slightest inflammation supervenes, and how thoroughly the ganglion is destroyed without further applications. He has cured large obstinate ganglia by passing setons through them, but this was after the ordinary methods of treatment had failed.

Westminster Hospital.—Mr. **Holthouse**'s most frequent method of proceeding in simple ganglia about the wrist is, first to flex the joint to the utmost, and make firm pressure on the tumour with the thumbs. Failing to get rid of it by this means, he passes a single thread through it; again makes pressure as before, and so empties it through the apertures made by the thread. The two ends of the thread are then tied together, and a compress applied. In the course of one, two, three, or more days, according to the amount of inflammation set up, the thread is removed; the inflammation subsides, and the ganglion is cured. Mr. Holthouse never adopts this plan of treatment unless he can see the patient within twenty-four hours, lest inflammation of an unhealthy character or too violent should be set up. Failing this condition, he punctures the tumour with a tenotomy-knife; squeezes out the contents; applies a firm compress over it; and straps it tightly round a splint placed on the palmar aspect of the wrist. There is a variety of ganglion with the pathology of which Mr. Holthouse is not well acquainted. It appears suddenly, without obvious cause, and cannot be distinguished either by its appearance or feel from an ordinary ganglion. It differs from this, however, not only in the mode of its appearance, but in its not forming a perfectly closed sac. Under pressure, it may be made to disappear completely, and without rupture of its walls: for this reason, Mr. Holthouse always first tries pressure and manipulation of the tumour before resorting to seton or puncture.

After a considerable experience of the various methods of treating ganglion, Mr. **Francis Mason** believes that, in those cases in which the cyst has a thin wall, the forcible rupture with the thumb or a flat book is the simplest and best plan of effecting a cure; but in cases of long standing, or those in which the sac is very thick, by far the least painful and most successful proceeding is to puncture the cyst in a valvular manner with a grooved needle (*a hare-lip pin does equally well*); *and, having* traversed the contents, to make five, six, or more punctures in various parts of the cyst, especially on the opposite side, taking care not to wound the skin in this situation. On the needle being withdrawn, pressure with the thumb evacuates the contents of the sac into the surrounding tissues; absorption ultimately ensues; and, if the precaution be taken to place a pad of lint, secured with a bandage, over the part, the cyst seldom, if ever, re-fills. With regard to the common and popular method of suddenly rupturing ganglions, with a book, Mr. Mason, has remarked that, when the ganglion is tough, no little skill is required to strike the part so as effectually to accomplish the object in view; and another point of some importance is that, if the cyst be not broken at the first attempt, the patient is unwilling to have the blow repeated, much discomfort being occasioned by this somewhat apparently coarse procedure. Again, a book of suitable size and shape is not always at hand. Moreover, the proceeding is in many instances, even if successful, attended with considerable pain, generally causing extreme pallor, and not unfrequently inducing a fit of fainting. Few patients object to the prick of the needle, provided they have the assurance that there is no "cutting operation." Iodine paint at the onset may be tried, but permanent good seldom follows from its use. The removal of the cyst by excision is seldom absolutely necessary; and when the operation is performed, it is, as Mr. Mason has seen, sometimes followed by intense inflammation of the sheaths of the tendons, accompanied with profuse suppuration, and leaving the use of the fingers much impaired, not to mention the constitutional irritation set up, endangering the life of the patient. Such cases are of course rare; but the practice of puncturing the cyst in the way already described is, so to speak, painless, and is almost entirely free from risk in any shape.

Mr. Richard Davy pursues the following methods amongst the out-patients viz., excision, partial or complete; subcutaneous slicing; subcutaneous puncture and injection of tincture of iodine; mechanical crushing; and subcutaneous puncture. The seton, being a tedious and painful process, has not been employed.

Great Northern Hospital.—Mr. Gay has tried a variety of ways, with different success in different cases. His experience has led him to the conviction that these bursæ must be treated with caution, and that danger may accrue at least to the limb if such a rule be not observed in every case. Rupturing the cyst by violence, with subsequent pressure, is of little or no avail. The cyst has, in his experience, repaired the injury and become refilled. External puncture, and even free incision, followed by compression, have been more successful. A series of blisters, followed by some irritant application, such as tincture of iodine, has occasionally succeeded, especially if the cyst be subsequently punctured externally, the contents squeezed out, and compression be ultimately applied. In these cases, the blistering must be continued so as to ensure the propagation of the excited inflammatory action to the inner lining of the cyst. Mr. Gay has not tried iodine and other injections. He has removed these cysts on two occasions, and with results that determined him not to attempt this mode of treating them again. In the first instance, the operation was followed by a violent attack of diffuse subcutaneous cellulitis and acute inflammation of the wrist-joint, which proved serious, although ultimately the limb and joint were saved; and in the second case the healing of the wound was very prolonged and was attended with deep-seated suppuration and some permanent contraction of the extensor tendon. The plan which Mr. Gay usually follows (and uniformly with success) is that by seton. He passes a single thread of silk through the cyst and allows it to remain until undoubted pus issues from the punctures. He employs a needle so large as to permit of the contents of the cyst being expressed through the openings which it makes; and, in case the thread does not excite sufficient inflammatory action, he not only moves it daily, but soaks the thread with tincture of iodine, and draws the portion thus soaked within the cyst. Even this plan is sometimes followed by severe inflammation and suppurative action, when the thread has had to be taken out. Occasionally, it has been so severe that he has had to open the cyst as an abscess. For the most part however, the process is simple; no superfluous action is set up. About the *seventh day*, some pus exudes with the thread; the thread is then withdrawn gentle pressure is applied, and the cyst contracts and disappears, all but its walls. Even when treated thus, the case must be under daily observation.

Mr. W. ADAMS thinks that, in the simplest form of ganglion, such as that frequently seen over the carpus, when recent, rupture of the sac by a sudden blow, or by hard pressure with the thumb, should first be tried, and will frequently succeed, even if it have to be repeated once or twice. This failing, he always resorts to a free subcutaneous section of the sac in different directions, by introducing a tenotomy-knife; and, after transfixing the sac, cutting freely in one direction, and then turning the knife, cutting as freely in the opposite direction. If the thin ganglionic sac appear to yield before the knife, the latter may be partially withdrawn, and the point made to pierce the sac in two or three situations. Firm pressure must afterwards be made by means of a piece of metal or small coin, wrapt round with lint, and kept in position by a bandage for a week. By this means he has generally succeeded in obliterating ganglia by one operation. Occasionally, however, failure occurs; and either the same operation may be repeated, or a seton may be introduced. In employing the seton, Mr. Adams always introduces six threads, and removes three on the following morning. This at once allows the fluid to escape, and relieves the inflammatory tension and pain produced by the seton. The remaining three threads may be allowed to remain for a week or more. From a neglect of this rule of removing half the seton, he once saw acute suppurative inflammation extend to the wrist-joint in a man under the care of the late Mr. Mackmurdo at St. Thomas's Hospital; and the patient died. The preparation, showing complete destruction of articular cartilages of the carpal bones, is in the museum of St. Thomas's Hospital. Mr. Adams believes the seton to be a perfectly safe and reliable remedy, if half the silk be removed on the day following its introduction; but it may occasionally fail, and in two instances he has cured an obstinate ganglion by a second introduction of the seton. To what extent ganglion may be dissected out with safety, he cannot say; but in one instance, mistaking the character of the tumour, Mr. Adams dissected out a large ganglion of a flattened and lobulated shape, which had formed over the extensor tendons as they cross the ankle-joint. He mistook it for a fatty tumour, such as he has seen in the forearm. On section, the ganglion was seen to be thick-walled and loculated; and, from its flattened and expanded form, he does not think a seton could have been employed. Metal sutures were used, and Dr. Richardson's colloid styptic with cotton-wool applied. Union by the first intention took place. Old thick-walled busal tumours over the patella, we know, have frequently been removed with safety.

Mr. BUXTON SHILLITOE always treats simple ganglion by subcutaneous puncture with a grooved needle, thoroughly emptying the cyst, and irritating its interior by a few scratches with the point of the needle. He is particular in keeping pressure upon the emptied cyst until he can apply a firm pad, which is retained in place by plaster and bandage for a week or so. It very seldom fails to cure, if thoroughly done. If it do, he repeats it immediately the cyst commences to refill. After the bandages are removed, he orders the application of iodine or frequent douches of cold water. Mr. Shillitoe has seen awkward results from the use of setons; and he believes, in the treatment of simple ganglions, that they are unnecessary. Compound ganglion requires much more careful treatment, partly on account of its extent and position, partly because its contents are mixed up with more or less solid fibrinous deposits. They occur chiefly in the palm of the hand and side of the sole of the foot. He has cured those on the palm of the hand by dissecting out a portion of the cyst, without division of the annular ligament—squeezing out the remainder of the semi-solid contents, and allowing the wound slowly to granulate up. Mr. Shillitoe says he should not hesitate to inject iodine in such cases.

University College Hospital.—Ganglion at the wrist Mr. BERKELEY HILL never forcibly ruptures, but evacuates subcutaneously by first incising the sac freely with a tenotome, then squeezing out the contents of the sac, and firmly pressing the walls together with a compress and strap of plaster. Should it recur, a fine silk thread is carried through the swelling with an ordinary suture-needle, and left for three or four days. To prevent the ligature from exciting too great irritation and suppuration, the patient is always directed to attend the following day at the hospital, that a dresser may examine the seton, and remove it if the action be too great. Injection of iodine is a less satisfactory mode; but it is employed by Mr. Hill for the deeply-seated ganglia of the ham, where the abscess that may follow the seton is extremely troublesome to heal. A ganglion now and then forms between the tibia and the inner ham-string, which Mr. Hill has found very tedious. In one case, the patient was obliged to relinquish his occupation through the pain which he felt in walking. His ganglion, being close to the knee-joint, was at first simply tapped and injected with iodine; but, as the fluid accumulated again, a thread was passed through, which was worn a week without much inconvenience; and the swelling gradually grew less, but did not disappear wholly, causing, not in ordinary walking, but when the limb was used to raise the body, as in going upstairs, a tolerably sharp pain. An attempt was then made to dislodge the hard nodule subcutaneously; this was so far accomplished that the tumour vanished, but some pain is still felt on going upstairs. The knee-joint was quite unaffected throughout these experiments. The ganglia that result from fluid collecting in the synovial sheath common to the tendons in the palm, and in that which lubricates the peronei behind the outer malleolus, Mr. Hill has tapped and injected with success, and without causing troublesome symptoms or stiffness afterwards. The application of pressure and blisters over these compound ganglia, in Mr. Hill's experience, is a waste of time, as these agents do not cause absorption of the fluid.

Great Northern Hospital.—Mr. Vincent has treated cases of ganglion in various ways, and with variable success. Forcible rupture by digital pressure, followed by continued pressure, and, if possible, a splint, have given the best results. Mr. Vincent considers the use of the latter advisable, to obtain the perfect cure of ganglion, as, in some cases of old standing, the ganglion, instead of being confined, as in recent cases, to the tissue between the tendon and the skin, extends around the tendon, and thus every movement of the tendon after the rupture prevents the adhesion of the walls of the sac; and hence the recurrence of the disease, and the necessity for the splint in cases where the patient can and will submit to its application. In private practice, the splint should always be insisted on. As hospital patients, however, refuse to discontinue their work, this plan is frequently impracticable.—*British Medical Journal, July* 1, 8, *and* 15, 1871, *pp.* 9, 36, 64.

59.—AN INSTRUMENT TO FACILITATE SKIN-GRAFTING.

An instrument designed to facilitate the process of skin-grafting, was brought into use at St. Bartholomew's Hospital in the early part of last March. It consists of a pair of curved scissors, which are provided on their concave surface with bent forceps. These are controlled by a lever which descends with the separation of the blades, and rises when they are brought together. The movements of these several parts are so concerted that the forceps meet between, and just below, the blades, immediately before the closure of the latter, and then rise between them to such a height that whatever they have seized will be divided from its attachment when the blades actually meet. Thus the whole process of seizing a small portion of skin, separating, and raising it, can be almost simultaneously performed with one hand. The size of the severed piece of skin is proportionate to the force with which the forceps are pressed against the surface from which it is to be removed.

This contrivance was suggested to Mr. Ferguson, of Giltspur-street, early in the present year, by Mr. Crips, a student of St. Bartholomew's. It has since been in constant use at the hospital, and is very favourably spoken of. Mr. Pollock also has, we believe, expressed his approval of it.—*Lancet, June* 3, 1871, *p.* 745.

60.—ON OAKUM AS A DRESSING FOR BURNS.

By Dr. Herbert L. Snow, Shrewsbury.

I wish to bring under the notice of readers of the journal the value of common picked oakum as a dressing in the suppurating sores resulting from burns. Among the good effects which I claim for it are these: First, it ordinarily induces the cicatrisation of extensive sores with remarkable rapidity; secondly, it induces healing action in such of these as, occurring in debilitated subjects, or under defective hygienic conditions, have almost assumed the character of indolent ulcers, and remain stationary under the application of the ordinary lotions; thirdly, I may allude almost to the entire absence of any offensive smell under the use of oakum, to its cheapness, and to the saving of time and trouble effected by its employment; lastly, (and this I hold to be by far the most important point of all) the *resulting cicatrices do not contract.* I have not yet used oakum in a sufficient number of burn-cases to advance the last statement as absolute; but in the limited number in which I have been able to employ this dressing thoroughly (in several of these there were very severe and extensive sores), I have always found the cicatrix of a peculiar dark-reddish colour, and soft consistence; none of the usual tense fibrillated appearance has been visible, and no subsequent contraction has ensued.

I now always use the common oakum, which appears to me to answer much better than the recently introduced "tenax," and probably contains more of the tarry principles. This is wetted with cold water, then placed on the surface of the sore, and a bandage loosely applied over it. No pain follows the application; but the oakum has to be wetted (without being removed) with cold water several times a day, or the patient will complain of its feeling dry and sticky: the latter may generally be left to do this *ad libitum.* The dressing need not be changed oftener than on alternate days—sometimes not until every fourth or fifth day, according to the condition of the wound and the amount of suppuration present.

Under this application some very large and deep sores heal with marvellous rapidity—many which would not be well for months under the ordinary modes of treatment, cicatrising in the course of as many weeks. All make very favourable progress towards healing, but in some the process is much more rapid than in others. The granulations seem to have a much diminished tendency to become exuberant, and seldom require either pressure or the application of escharotics. The oakum should not be applied until suppuration is well established; previously, it does not seem so beneficial as many of the applications in ordinary use. It may also be used with benefit for suppurating ulcers resulting from other causes than burns; but I have not found the former heal with nearly so much rapidity as the latter.

The last of the considerations which I have adduced above must be my main apology for sending this paper, which I have done in the hope of inducing further trials of the material. Admitting that my own experience has not yet been sufficiently extensive to decide the point absolutely, if indeed we acquire a means of preventing the frightful sequelæ of burns, unfortunately so common, we shall have made a material contribution to the alleviation of human suffering.

I may mention, in conclusion, that I have lately adopted Mr. Skey's plan of applying a solution of nitrate of silver (twenty grains to the ounce) to all recent burns, with the best effect; the pain being soon much alleviated, and the subsequent ulceration considerably diminished.—*British Medical Journal, June* 10, 1871, *p.* 610.

SYPHILITIC AFFECTIONS.

61.—SYPHILIS AND VACCINATION.

[The following remarks on this subject are from the *Medical Times and Gazette* and the *Lancet.*]

The Committee appointed by the Royal Medical and Chirurgical Society upon the cases of vaccino-syphilis referred to in Mr. Hutchinson's papers, made their report on Tuesday evening. They state that they saw three of the cases in each series, and also the two vaccinifers. We are sorry to say that the Committee confirm the inferences of Mr. Hutchinson. They have seen no reason to doubt the fact that syphilis was in each series conveyed in vaccination, but whether through the medium of the lymph, or of blood, or both, they are not prepared to state. At the same meeting it was announced that Mr. Hutchinson had forwarded an appendix to his former papers, but it was not read in consequence of the meeting being the last for the session. We are far from wishing to throw undeserved obloquy upon the practice of arm-to-arm vaccination; but it cannot reasonably be expected that the public, who are always more open to unfavourable than to favourable reports, will ignore the facts now established. There is a danger—infinitesimal indeed, but still a danger—and they will demand to be protected from it. No one who knows what syphilis is, and how it impresses its stamp upon a whole lifetime, will say that such a demand is unreasonable. At the present moment no one can tell how often a similar accident has occurred before; for these cases can scarcely be believed to be unique. And the question which now presents itself to us is, what steps the Government intends to take to provide that the population shall not plead danger as a bar to the vaccination of their children. Much has been written about the non-success of heifer vaccination, and much which is of questionable authenticity; but, after all, it is due to the public that the alternative should be offered of vaccination from the child's arm or of vaccination from the calf. In London, Manchester, Liverpool, and other large towns we certainly think that, at definite and short intervals, an animal vaccinifer should be provided by the State for the use of persons who object to arm-to-arm vaccination. A few calves thus provided at large centres of population would suffice for the exigencies of the case, since it has been shown that the lymph can be preserved for several days upon thoroughly charged points, and, if then used by the method of scarification, is nearly as efficient as humanised lymph.—*Medical Times and Gazette, July* 1, 1871, *p.* 13.

[The Editor of the *Lancet* says:]

It is especially necessary that we should call attention to the Report of the Medical and Chirurgical Society's Committee on Mr. Hutchinson's cases of alleged transmission of syphilis by the medium of vaccine lymph, because a most extraordinary misrepresentation of the purport of that document has been published in the columns of a contemporary. The journal has stated, no doubt by inadvertence, that the Committee have confirmed the diagnosis of syphilis in Mr. Hutchinson's cases. This is a most incorrect version of the report. It must be in the recollection of everyone that two series of cases were brought forward. In the first of these a number of patients, of different families and of various ages, vaccinated from the same child, were affected at the site of the operation with sores which presented a considerable resemblance to indurated chancres; in the other series, two children born of the same parents were affected after vaccination from another vaccinifer not merely with local symptoms resembling chancre but with symptoms strongly resembling those of true secondary syphilis. In the first group of cases (Mr. Hutchinson's own) there had been full opportunity for verifying the fact that the vaccinifer was syphilitic and that the vaccinations had been performed with all proper care, save in one respect—that there was reason to think that blood as well as lymph had been taken from the vaccinifer. In the case of the other two children (patients of Mr. Warren Tay) there was, on the contrary, no sufficient evidence forthcoming to determine the origin of the syphilitic infection.

Such, in a few words, was the position in which the matter rested after the discussion at the Medical and Chirurgical Society on May 16th, at which time the subject was handed over to a Committee for further investigation. In our leading article of May 20th we pointed out the essential difference between the two series of facts so far adduced; we showed that in Mr. Hutchinson's cases there was unmistakable evidence that the vaccinifer had communicated one and the same *irritant* influence to a variety of persons whose constitutions had nothing in common with each other, but that until undoubted secondary symptoms should appear in the sufferers there was no reason for assuming that that irritation was due to the communication of syphilitic virus; while in the two cases of Mr. Tay there was, unfortunately, very strong evidence of syphilis, but so far no sufficient proof that the disease had been communicated in vaccination. Let us add that Mr. Hutchinson himself believed the patients of his first series to have so far escaped from secondary symptoms because they had been treated with mercury.

Now the further evidence produced by the Committee is this:—As to Mr. Hutchinson's cases they report that the whole thirteen still (five months after vaccination) remain free from secondary symptoms; and consequently there is, at present, *no proof of syphilis in these at all.* As to the cases of Mr. Warren Tay, unhappily, the result of the inquiry is very different. In addition to the brother and sister originally known to be syphilised, two other children vaccinated from the same vaccinifer, and not related to each other, have been found to have also exhibited signs of constitutional syphilis. It must be confessed that so far as this fact goes, it considerably strengthens the probability that vaccination was the medium of the infection. But, on the other hand, there are enormous gaps in the chain of proof. It is not merely that the Committee had no personal interview with the vaccinator, though that is a serious defect. What is of far more consequence is that the evidence as to the vaccinifer of Mr. Tay's cases having had syphilis is about the slightest on which we ever heard of a diagnosis being founded. On that subject we may refer our readers to what we said on a former occasion (*Lancet*, May 20th); and we may now add that the Committee have not obtained one morsel of additional evidence that the vaccinifer was syphilitic, except the fact that two other children among those vaccinated from it became syphilitic. Our readers do not need to be told, however, that it would by no means be anything very extraordinary if four children, belonging to three families, who all happened to be vaccinated from the same child, should all have suffered from syphilis of separate origin. In short, Mr. Tay's cases, though warranting a certain amount of suspicion, are far indeed from affording proof of vaccino-syphilis. As to Mr. Hutchinson's cases, we are abundantly justified in saying that, so far, their value as proofs is absolutely *nil.*

Upon the whole we consider that very grave errors have been committed by nearly everyone concerned in bringing these cases forward. The inquiry in its crude early stages was one that was not fit to be undertaken by a miscellaneous body of medical men like the Fellows of the Medical and Chirurgical Society; very few of whom could be supposed capable of deciding, in the brief period of a debate, the precise value of evidence on an intricate question of pathology, or of recalling (even if they had ever studied) the whole previous history of the vaccino-syphilis controversy. We think there can be no doubt that the whole preliminary investigation should have been conducted by the Medical Officer of the Privy Council and his able subordinates; and that the evidence should only have been presented to the general profession for discussion when it had undergone a most rigorous sifting at their hands. We repeat what we said before—that it is a discreditable exhibition when prominent members of a great Society like the Medico-Chirurgical actually declare that So-and-so is syphilised on the simple evidence of his having an indurated sore on the arm which had reopened after healing up. Why, that is a common characteristic of the effects of unusually irritant lymph of any kind; it is a phenomenon which has occurred to many practitioners during the recent small-pox epidemic; and it is a specially frequent and unwelcome result of that same clumsy and discredited heifer-vaccination which our sage contemporary, already referred to, seeks to bring into fashion again! Readers of the *Lancet* have only very recently had an example of the latter fact brought before them by Mr. Trotter, assistant-surgeon to the Coldstream Guards; a series of patients having had sores exactly resembling chancres after vaccination from the cow.

We must conclude these remarks by earnestly reminding those of the public who may chance to read this article, that even supposing the worst were proved which it was sought to prove regarding these cases, it is only a microscopic danger that has been shown to exist, and one that the reasonable care which a respectable practitioner may be expected to exercise will altogether set aside. There is not the shadow of proof that vaccine lymph itself ever infects; and we hope we may confidently state that after the recent discussion practitioners will be doubly careful to observe that old and well-founded rule—never on any account to employ lymph which contains even the smallest speck of blood or any foreign matter. And undoubtedly they will also still more strenuously carry out the injunction which all instructed men knew to be binding long before the recent controversies were heard of—by all possible means to avoid taking lymph from a child that is unhealthy either in itself or in its family history.—*Lancet, July* 8, 1871, *p.* 52.

62.—ON SYPHILITIC INOCULATION WITH REFERENCE TO THE PRODUCTION OF THE SPECIFIC PUSTULE.

By JOHN MORGAN, Esq., Professor of Surgical and Descriptive Anatomy, R. C. S. I., &c.

Mr. H. Lee in some remarks on this subject, quoted from his lectures in the *Medical Press*, April 19th, has alluded to observations which have been made and published by me in Dublin, as to the production of certain chancroids or sores, simple in character, from the secretions or discharge of contaminated patients on patients also contaminated.

The question is one of interest—as the observations and direct investigations I have made tend to show that the chancroid or soft sore is but a descendant from the chancre or hard sore, and is not a distinct poison, but can be produced from the discharge of a constitutionally infected person, and more especially from one form which is an obvious and fertile source—the vaginal secretion of an infected female.

Mr. Lee says, alluding to my testings:—

"It will be said it is the soft sore which has been produced in the experiments alluded to. Now, to explain this, two circumstances must be borne in mind: First, in certain states of constitution in syphilitic subjects, a pustule and subsequent sore is produced with great facility. Here is the drawing of a well-developed pustule in a syphilitic subject, produced by the inoculation of some pus from a case of excision of the knee-joint in a child. The second point to which I advert, and which has a more direct bearing upon the experiments in question, is that inoculations may no doubt be performed occasionally on syphilitic patients which will not present the characters of either of the forms of disease which I have described. I have inoculated and succeeded in inoculating syphilitic patients from the secretions of indurated sores, but the ulcers produced have not presented the sharp ulcerated border of the local suppurating sore, nor have they been accompanied by the adhesive form of inflammation of true syphilis."

As to the first point—It may readily be admitted that the inoculation of pus produced a pustule in a syphilitic patient, but—Was it a specific pustule? Was it the same as a pustule the result of a specific inoculation? Did it differ for instance from the pustule of a poisoned wound? Was it *re-inoculable* on the patient's self or on others? If it was not *re*-inoculable, and capable of becoming the head of a series, we must eliminate the occurrence of such an inoculation as being valueless in the argument. Inoculation with pus from an excised knee-joint would be likely to produce a pustule on any one, specially if not in vigorous health. I saw not very long since a well-marked pustule *produced* on the back of the hand of a surgeon in an hospital by simply the *contact of a drop of* blood, yet I would be extremely sorry to brand him as syphilitic on that account, and I am satisfied the case was not syphilitic. The remark is made that in certain states of the constitution in syphilitic subjects a pustule and subsequent sore is produced with great facility; and it is mentioned by Mr. Lee "that it has been proved over and over again that the matter of gonorrhœa is not inoculable." I have tried it on several occasions on syphilitic subjects, and when taken from males who never had a sore, yet it failed. If a pustule and sore can be produced by inoculating with matter from an excised knee-joint, why should the gonorrhœal matter fail to do so? Is it not presumable that the inoculation of the matter from a diseased joint acted on a syphilitic patient just as it would on any other person by producing a pustule, which was not specific or specifically inoculable, though it may have been highly irritable.

It appears proved that inoculation from the gonorrhœal discharge of a *male* produces no result on a healthy person, or what is more important in the question debated, *on a syphilitic* patient, yet that the gonorrhœa or vaginal discharge of a syphilitic *woman* will produce a *re*-inoculable pustule on a syphilitic patient, and what is also to be specially borne in mind, on the patient's *own* person —illustrating the well-known argument of those who hold to the existence of *two* poisons—that the produced sore is of the *auto*-inoculable character of the chancroid or simple affection.

The case of Hunter is referred to by Mr. Lee, as an evidence of the power of the gonorrhœa "of a syphilitic subject," to give, as it did Mr. Hunter, an indurated sore; this I do not consider a very unreasonable suggestion, and that matter was derived from a syphilitic male who may have had a sore in existence, or been syphilitic. The gonorrhœal or vaginal secretion of a syphilitic female, constitutionally and not locally affected, however, I have found to produce the soft simple sore, with its characteristic pustule, when inoculated on syphilitic subjects (as they alone were suited for such a test). To this it has been objected by Mr. Lee, that "the experiments have all been performed on patients whose systems were already affected by syphilis," and that the same difficulty would attend as if experimenting "with cow-pox on those who already had the disease." But, unfortunately, the theory of the dualists is carried out by the virus got from this source, it is *auto*-inoculable. The gonorrhœa of a syphilitic male should have been inert on Hunter a second time, or on anyone "already affected with syphilis," but the inoculation of the vaginal discharge of a syphilitic woman is *auto*-inoculable, and is preceded by the characteristic and highly re-inoculable pustule which attends the inoculation of a soft sore produced by direct inoculation from another soft sore—or from a sore the product originally of artificial inoculation with a syphilitic vaginal discharge. The appearances and conduct of both sores *are identical*—the one a descendant in a direct line from a vaginal discharge, the other the descendant of a chancroid or soft sore.

The following testings which I carefully performed prove abundantly the *auto*-inoculability of this discharge, and its general inoculability on other syphilitic patients, and proves that the ordinary sore or chancroid, with its characteristics of inoculability, and auto-inoculability, and pustular development, may be the direct product of contamination by a secondary discharge.

July 6, 1870.—A strong, fresh-looking girl of twenty-three, had been admitted to hospital on two occasions:—First, on June 1, 1868—for several soft sores; secondly, on October, 1868—for a sloughing phagedenic sore of the nympha, followed by patches, papules, pains, and finally by ecthymatous spots here and there over the body: the phagedenic ulceration was most acute, and was cured by the use of escharotic and local applications.

She remained without further symptoms till about ten days before admission, when she got a discharge with some pain and tenderness, and a sore formed of a secondary character, at the cicatrix of the former sore near the nates; the stains left by the ecthyma are still very evident; also a wound on the arm, made about six weeks ago, presents a deep coppery stain, indicative of a latent taint.

On careful examination with the speculum, no ulcer whatever is to be seen, but there is a purulent vaginal discharge.

I inoculated with this on herself, on July 7, producing, by the 11th, a well-marked pustule, becoming a sore shortly afterwards.

July 7, I inoculated No. 1193, suffering from copious patches, alopecia, and pains, and produced, by the 11th, a characteristic pustule and sore.

On July 7, I inoculated No. 921 (who was at this time partially under the influence of another inoculation), and produced, by the 11th, a perfectly formed umbilicated pustule. This patient was intensely syphilitic at the time.

On July 7, I also inoculated No. 988, who was almost free from syphilitic symptoms, having been frequently inoculated, but produced only a small ill-formed pustule and superficial sore.

July 7, I also inoculated No. 1000, who had already been several times inoculated, and produced a specific, but not vigorous pustule.

July 7, I inoculated No. 1075, and at the time suffering from a vigorous chancroid; and produced a perfect pustule and small chancroid.

This case is very interesting, as there was no rash, and the secretion from the sore could not *possibly* be mingled with the vaginal discharge. The sore had not the character of a primary either in appearance or secretion, and the patient might otherwise have been looked on as a specimen of rude health, but as evidenced by the stains, and the discoloration left by the wound occurring within two months, together with the re-opening of the original cicatrix, the venereal taint was, though latent, still as active as ever.

It is suggested that such a testing as this is but "a different form of the experiment Hunter performed on himself," but there is no evidence that the source of Hunter's virus was a constitutional secretion. I confess I think it is widely different as to result and form.

1. No indurated sore was produced.
2. The secretion or gonorrhœa of constitutionally infected *women* was used.
3. These were undoubtedly syphilitic patients.
4. The recipients were all undoubtedly syphilitic.
5. Inoculations and also *auto*-inoculations were performed.
6. The resulting sores were re-inoculated in many cases.

I have arranged in a tabular form a few of these cases. The result narrows the question to this: either these inoculations would have produced chancroids or soft sores on the individual herself, or on an infected male; and be capable, at the same time, of producing (as Mr. Lee remarks in Hunter's case) an indurated sore on an *un*tainted person; or else this is the true origin of the chancroid or soft sore, which is but a descent, with modification, from syphilis proper, and derivable from a secondary lesion; that it would produce "indurated sores" cannot be reconciled with the every day fact that indurated sores are comparatively unfrequent as compared with the simple sore which, as in the accompanying series, is proved to be most easily and vigorously reproducible from a discharge only.

Remarkable Inoculations from Vaginal Discharge—all producing Characteristic Pustules and Chancroids more or less persistent, capable of Indefinite Propagation.

Inoculation of the Vaginal Discharge of No. 1204.

Produced chancroid on No. 1118, on June 2nd, which produced chancroids	June 10, on No 1000.
	Do. 12, on No. 1000.
	Do. 12, on No. 1085.
	Do. 12, on No. 1159.

Inoculation of the Vaginal Discharge of No. 988.

May 23,	do.	on herself.	From this, June	3, on herself.
			Do. ,,	10, on 1163.
			Do. ,,	12, on 1112.
June 3,	do.	on herself.		
,, 13,	do.	on herself.		
,, 20,	do.	on 1060.		
,, *23,*	*do.*	on 921; not very developed.		
,, *23,*	*do.*	on herself.		

Inoculation of the Vaginal Discharge of No. 1185.

June 27,	do.	on herself.	From this, June 27, on 1167.
„ 27,	do.	on 921.	
„ 27,	do.	on 911.	
„ 29,	do.	on 1169.	
„ 30,	do.	on 1189.	

Inoculation of the Vaginal Discharge of No. 1140.

June 7,	do.	on herself.
„ 7,	do.	on 1112.
„ 15,	do.	on 1138.

Inoculation of the Vaginal Discharge of No. 1220.

July 6,	do.	on herself.
„ 7,	do.	on 1193.
„ 7,	do.	on 921.
„ 7,	do.	on 1000; not very vigorous.
„ 7,	do.	on 988; „ „
„ 7,	do.	on 2075; „ „

No. 1118 proves the power of this source to become the head of a series, or a chancroid, if not a truly syphilitic "scolex."

No. 988 proves the auto-inoculability of the auto-inoculated sore itself.

The remainder show the *auto*-inoculability and *re*-inoculability of this contagion source.

Mr. Lee remarks, as before quoted, that "in certain states of the constitution in syphilitic subjects a pustule and sore is produced with great facility." Now, in the above-noted cases, twenty-eight of them, taken throughout the wards and in various stages, some having been before inoculated, and others not, the pustule and sore were always similar, and, in some series (for instance, No. 1220,) the results were not as active perhaps, but were as marked as in No. 1185. "The certain state" I found a uniform one, and the only cases where there were inferior developments were those that had been *previously* inoculated. This is a very remarkable fact, and it would be an interesting addition to our knowledge if we were acquainted with, or could prove, "the certain state" where this pustule is easily produced, if, indeed, such a state absolutely exists; it would go far in explaining the phenomena of soft sores, or the possibility of specific pustular sores occurring in syphilitic patients from contamination or accidental inoculation with *any* form of genital pus (as with the pus from the knee-joint above referred to); but this occurrence I must look on simply as an accident, until it is proved that the resulting sore was specifically re-inoculable and auto-inoculable.

Allusion is made to the co-existence of the two forms of sores, and that the super-addition of the virus of the simple sore may overwhelm the appearances of the sore characterized by "adhesive action" around it; and when artificial inoculation is performed from this, the "characteristic simple sore with the sharp edge was produced." Mr. Lee says that it is the secretion of the super-added simple sore virus, which is alone transmittable; but he mentions elsewhere that when he had irritated the sore characterized by adhesive action (or the hard sore) by savine, &c., and thus procured *pus*, he then could inoculate successfully. It is surely reasonable to conclude that the pus-producing virus will have acted just as the savine did, and gave the fertilising secretion, the cells of which seem, by Rollet's experiment (of filtering the pus, and then finding it inert,) to carry the morbific element perhaps in the shape of "microzoons," such as have been described in the vaccine fluid. The remarks of Sperino, where a lancet, which had been charged with chancroid pus and left to dry for seven months, and thus had thoroughly disturbed the constitution of the pus-cell, and yet produced inoculation, proves that the morbific atoms do not consist of the perfect pus-cell, although filtration of them checks contagion—they must exist *independently* of the pus-cell, but have some relation to them not as yet determined. The mixed sore is a pleasant half-way house on the road to the syphilitic Parnassus, but that the virus of induration and adhesive inflammation and of ulceration and suppurative action should co-exist seems contrary to our practical observation.

With regard to this question, it is said that, "supposing it to have been an indurated sore, upon which another secretion was inoculated, it would be the latter only which would be communicated by inoculation," though one sore is characteristically auto-inoculable, and the other, as *soon as it furnishes* pus, is also admitted to be so, it is said that it was the superadded secretion which is alone inoculable. Under such circumstances, it is, I think, very problematical as to which poison would furnish the inoculative power, and there can be little doubt as to the infecting operation of such a sore, if communicated. What its aspect would have been, or under what form re-inoculations or auto-inoculations appeared, if tested, is not shown, but would be interesting. The cases of inoculation on healthy subjects occurring in the thumb and also on the breast, and illustrated lately in this journal, were undoubtedly from congenital patches, yet the primitive lesion was of the nature of the chancroid or soft pus-secreting sore, and yet was followed by secondary affections. The vast majority of sores occurring in *female* patients in the Lock Hospital here, are pus-secreting and chancroid in appearance, yet are almost invariably followed by constitutional signs of various forms. I have auto-inoculated such sores frequently, I have opened the suppurating bubos accompanying such sores, and I have auto- and re-inoculated from the vaginal discharge of such patients, and produced true inoculative pustules and chancroid inoculable sores, the patients themselves offering, at the same time, intense syphilitic phenomena; this is, no doubt, not conformable with the dual theory, but it is a substantial fact, and the records of the hospital amply corroborate the statement.

Mr. Lee lays down as a rule that, "if from any depressing influence, etc., the primary affection has ulcerated to any extent, or suppurated for any length of time, then the secondary affection will have a tendency to do the same." The cases above alluded to suppurated freely, but the great majority of them had no ulcerative secondaries, and every day we see papules and pustular rashes co-exist, and one exchanging with the other, as it appears to me, quite irrespective of the appearence of the primary. I hope to recur to the matter at a future period, I wish at present to draw attention to it as a startling enunciation.

I have no doubt but that the great source of the chancroid or soft sore in men is from secondary syphilitic secretions, the vaginal discharge being obviously the most copious, and the most obnoxious, is capable of auto-inoculation and of re-inoculation in an indefinite and intensifying series, and though the sore may, and very frequently does, exhaust itself in mere local phenomena, it may, at the same time, carry with it sufficient of its parent virus to contaminate constitutionally. It is impossible that females suffering from soft sores, which speedily become intensely painful and intolerable, could permit coitus, whereas how large a percentage of the male population suffer from soft sores is too well known to practitioners; some other source must exist in the female than the direct effect of a local or soft sore communicating by contact.

At the present moment I have in hospital a patient whose entire penis has been carried away by ulceration, with the only sore he ever had, and he is now thoroughly broken down with constitutional syphilis; a young man of twenty-one, covered with stains, the result of a sore and suppurating bubo of two years ago; a boy of sixteen with sore and suppurating bubo still unhealed, having secondary rash and pains; a boy of seventeen, with a large, soft sore, equalling a crown piece, yet with well-marked indurating glands. On the other hand, very many cases will escape the tendency of recurrence to the original source, however constant; just as we see in nature, from the most luxuriant apple grafted on the "crab," the seeds will produce "crabs," but the graft will produce grafts of equal, if not improved quality; nevertheless, the tendency of recurrence to the parent source will persistently show itself in the seed *if sown, and* the "crab" will again develop itself.—*Medical Press and Circular, May 8, 1871, p.* 369.

63.—SYPHILITIC ERUPTION RESEMBLING RUPIA.

[The following is from a report of Mr. Hutchinson's Clinique at the London Hospital.]

A curious case of syphilitic disease of the brain was called into service to demonstrate a syphilitic eruption on the body. This eruption was very abundant, serpiginous, and multiple, and hence resembling rupia. It was stated to be not rupia, however, because (1) it was a tertiary, and not a secondary symptom; (2) the ulcer healed in its centre, whilst it spread at its edges; and (3) it was not symmetrical. The patient was taking twenty-five grains of iodide of potassium daily, and Mr. Hutchinson pointed out the necessity of commencing with small doses of this drug, but steadily pushing it on to get the best effect, there seeming really no limit in some cases to the advantageous pushing.—*Medical Times and Gazette, July* 1, 1871, *p.* 8.

64.—ON THE MODERN TREATMENT OF SYPHILIS.

By Edgecombe Venning, Esq.

[In the year 1864 the Lords Commissioners of the Admiralty appointed a Committee to enquire into the pathology and treatment of syphilis. This Committee published its report in 1867, and it is upon this report that the following paper is chiefly founded. Fifty-six of the most eminent members of the Profession were examined before this committee, and their evidence demands close attention. The writer gives us a concise statement of the conclusions to which the evidence brings us upon this interesting subject. He observes that we may best ascertain what conclusions may be drawn from this evidence by enquiring:]

1. Are there two forms of venereal sores, depending upon different poisons, and whose effects on the constitution are essentially different?

2. Can one form of sore always be distinguished from the other?

On the first question forty witnesses gave evidence. Of this number, thirty-five were of opinion that there are two different forms of sores; and only five took a contrary view. And I believe that most surgeons in the present day will at least admit this much; as it must daily fall to the lot of most of us—who have a fair amount of experience in such cases—to see the two forms well and clearly defined: one represented as a soft sore, secreting pus plentifully; and the other characterised by an indurated base, and not as a rule discharging pus, but only the *débris* of lymph. But when the fact of the two sores depending upon separate poisons is investigated, we do not find such unanimity of opinion. Sixteen witnesses only coincided with this theory; twenty-two were decidedly against it; and two were doubtful about it. It is for us to enquire into the causes of this disagreement and try if possible to clear up the existing doubt. In order to do this as effectually as possible, I propose to examine into the *pros* and *cons* that were put forward in evidence.

Those who stated decidedly that there were two different poisons, always producing different sores and sequelæ, were very few in number; but there were some, and among them was Dr. Byrne, surgeon to the Westmoreland Lock Hospital, Dublin, who stated that "the soft sore is never followed by constitutional symptoms, unless there be some modification of it." The majority of the witnesses who were in favour of the double-poison theory stated, in addition, that they had not infrequently seen the soft variety of sore followed by constitutional symptoms. I account for this discrepancy in the following manner. The soft sore which has been followed by secondary symptoms has been a soft sore with something added to it; and that something has been the true syphilitic poison; in fact, the local and constitutional disease have existed together. I believe that in the majority of these instances has there been a certain amount of induration accompanying these sores, though it may have been so slight as *to escape anything* but most minute inspection. But I admit that in some instances the induration upon which so much stress is laid is altogether absent. I cannot help thinking that too much importance has been attached to this one symptom. That when present it is a very valuable one, I frankly admit; but its absence does not negative the fact of the sore being a true infecting one. Mr. Henry Lee, in his published lectures on *Practical Pathology*, vol. ii. p. 8, makes some very sensible remarks on this point. "A physician," he says, "may be unable at once, or at all, to form a diagnosis of a particular kind of fever or disease from the presence or absence of one symptom; but he can, and does daily, form a correct diagnosis by the consideration of a group of symptoms, and the conformity or nonconformity of his patient's illness to it." The infecting chancre is no exception to this rule. We must be guided by a series of symptoms; and I hope to be able to show that it is quite possible by carrying out this rule, to diagnose each form of sore with certainty.

I am led to the conclusion, that it is the mixing of the two poisons in one sore which has led to so much disagreement, by personal observation of a large number of cases. And I cannot call to mind any case of soft chancre which was purely such (and by this latter I mean unattended by any of the signs of an infecting sore,) that was followed by constitutional symptoms.

Twenty-two witnesses, I have stated, were decidedly against the theory of the duality of the poison; and we must inquire into their reasons for believing thus. Here, again, we shall see that their great reason was, that they had so frequently seen the soft sore followed by constitutional symptoms. Our answer to this must be the same as before—viz., that they have been dealing with mixed poisons. But supposing, as is the case with many, that they will not admit of this solution of the question. How, then, do they account for such very different results from one poison? Some would lead us to believe that the soft sore is only a modification of the genuine syphilitic poison. By some means it has been diluted, and therefore its effects have become less potent. Others state that the matter with which the system has become inoculated has been old and thus become degenerated; and a third lay all the stress upon the constitution into which the virus has been received. "As your soil, so will your fruit be," say they. I cannot agree with any one of these theories, nor do I think that recent investigations tend to confirm them. That a diluted poison should produce such very different results, is hardly conceivable. Surely the constitution would be tainted to a greater or less extent, and would give some greater evidence that so noxious an element had been received into the system, even though diluted, than a small sore, which heals in a given time, and gives no farther trouble. With regard to the degeneration of the poison; if it has become so degenerate, it can no longer be looked upon as the same thing, and must be classed among other decaying and effete matters. Nor do I think that the third theory is more tenable. That the condition of the constitution shall determine whether such a viralent poison as syphilis is to produce only a local or general contamination, is, I think, unreasonable. We do not find this to be the case with other poisons. If the poison of variola is inoculated into a system which has not previously been infected with it, we see definite results, and it runs a known course, whatever the state of the constitution; and so with other diseases depending upon a specific poison. We do not see them at one time producing a local result, and at another a general one. Why, then, is syphilis to be a great exception to this rule? I do not believe that such is really the case. In other systems, "as your soil, so will your fruit be," does not hold good. If wheat be planted in a field, it will not depend upon the soil whether the fruit it yields will be wheat or oats; into whatever soil it is placed, the result is always the same; and such is the case with the human soil.

With regard to the treatment. Here, again we are met with scarcely less diversity of opinion than we have found to be the case with regard to the pathology. We have to contend with mercurialists and non-mercurialists, including under the latter title those who advocate syphilisation as a means of cure, and those who recommend that the disease should be allowed to run its *natural course without* interference. In order to ascertain what conclusions may be drawn from the evidence given on these several points, we will consider the following questions :

1. Should mercury be administered in all forms of primary sores ?

2. Is mercury beneficial in any particular form of primary sore ? If so, in what form should it be administered ?

3. Is mercury beneficial in the secondary evidences of the disease, or is there another therapeutic agent which gives better results either alone or in combination with mercury ?

4. Are the results of the non-mercurial form of treatment as successful as the mercurial ?

On the first of these questions, forty witnesses gave evidence. Of this number, ten were of opinion that mercury should be administered in all forms of primary venereal sores. Most of them recognized but one form of sore, and treated it accordingly. On the other hand, eighteen stated that it was not necessary to give mercury in all primary sores. These recognised the two distinct chancres; one (the soft) constituting the local, and the other the infecting or constitutional disease. Four witnesses stated that they never gave mercury in any form of primary sore, and three were doubtful whether they should not give it in both kinds. Mr. J. R. Lane stated, "that though he did not always treat the soft form of sore with mercury, still he believed that it would be always safer to do so." Five witnesses never gave mercury in any form of the disease from beginning to end; three of the latter number treating all their cases by syphilisation.

From what has already been stated with regard to the pathology of the disease, it will be surmised that I hold to the opinion that mercury ought not to be used indiscriminately in all primary sores. In the true local disease it is not only unnecessary but hurtful, as it appears to us to retard the healing of the sore. We have not in these cases to deal with the poison of syphilis, and therefore should not treat it as such. We have seen that this disease runs a definite course, and terminates without infecting the constitution. Our treatment, therefore, should be based on this principle. If there be no contrary indication, I believe the best practice to be at once to destroy the sore with strong nitric acid; keep the patient in bed so as to prevent irritation of the parts, which would tend to increase the chances of a suppurating bubo ; support the constitution with a generous diet and ferruginous tonics; and in the very great majority of cases the patient will get well in about a fortnight or three weeks.

We must now pass to the inquiry into the evidence given on the second question. On this point forty witnesses also gave evidence. Twenty-one were of opinion that it was especially in the indurated form of chancre that mercury was most beneficial. The impression made on the minds of those who had thus treated the disease was, that it healed the sore, postponed the secondary infection, and modified it when it made its appearance. Very few were of opinion that it gave exemption from secondaries, but some were of this opinion. Among these was Mr. Acton, who stated that "he was sure that mercury gave exemption from syphilitic disease." Dr. Marston, of the Royal Artillery, believed that mercury and syphilis were antagonistic. Mr. J. R. Lane stated that he believed mercury tended in a hard sore to the absorption of the induration, and to diminish the liability to secondary constitutional affections ; and he mentions a case where there had been well-marked induration for weeks, and mercury had been given, and no secondary affections had appeared for a very long time. Mr. Paget stated that "he regarded mercury really as a specific for syphilis in this sense, that, provided the patient is one who can safely take mercury, it will very materially shorten the duration of the indurated sore ; and if it can be favourably received into the system, will prevent the occurrence of secondary symptoms." Nine witnesses did not recognise any particular form of sore in which mercury should be given, as they gave it in all. Five never gave mercury at all in primary sores ; and four were non-mercurialists. One recognised only an ash-coloured form of sore in which mercury was useful ; this was Dr. Stewart, Surgeon-Major in the Bombay

Army, who stated that in India secondary affections most frequently accompanied this form of sore.

From the above statements it would appear that the majority of the witnesses recognised a sore in which mercury proved useful as a therapeutic agent, because, in addition to the eighteen who were decidedly of this opinion, the nine who did not recognise any particular sore in which it should or should not be given, nevertheless were decidedly of opinion that it was beneficial in indurated chancres.

My own experience in a regimental hospital, where the opportunities of watching cases for almost any length of time are very great, has taught me that in the infecting form of sore there is but one safe practice, and that is, the continued administration of mercury in some one form or another. I indorse again Mr. Henry Lee's statement in his lectures on *Practical Pathology*, vol. ii. lecture xxxiv., where he states, "that it may be safely affirmed that the general experience has proved that no remedy exists possessing so great a power over the syphilitic poison as mercury." My strong impression, after watching a large number of cases, is, that in some instances (though I am far from saying in the majority) mercury does prevent the occurrence of secondary symptoms. At the same time, I believe I can state with perfect confidence that it does postpone the appearance of the secondary affection, and mitigates it where it does do so.

Having decided that there is a syphilitic sore in which mercury is decidedly beneficial, we must next inquire into the evidence given with regard to the form in which it should be administered. Twenty-six witnesses stated their opinions with regard to this. Eleven gave the preference to blue-pill, six to inunction, three to fumigation, two to mercury and chalk, two to Plummer's pill, one to the bichloride of mercury, and one to the protoiodide. The remainder of the witnesses either did not give any opinion on this question, or considered it immaterial in what form it was given. Of all the preparations of mercury that I have mentioned, I find that blue-pill has the greatest number of supporters; and my own observation leads me to give my vote in favour of blue-pill in the primary infecting chancre. I believe that in this form its effects are most easily managed. As a rule, it is well borne by the system, and it is a clean and convenient mode of administration. At one time I greatly preferred administering mercury by fumigation, according to Mr. Henry Lee's method, and which he so strongly advocates, and with which he has had such good results. Of late I have discarded it in the primary sore, as it appeared to me that the results were not so satisfactory as they are when mercury is given in the form of blue-pill. I believe that the ill results spoken of as the result of this drug are in a great measure due to want of care in its administration. While taking it, the patient ought, if possible, to be kept in bed, or at least in a perfectly equable temperature. The weight of the patient should be registered before commencing the treatment, and this should be repeated every third or fourth day. I am indebted to Mr. Cutler for this latter suggestion, and it has proved to be a most valuable one in the treatment of syphilis. As soon as the mineral begins to have any pernicious effect on the system, it will be indicated by loss of weight, even before other well-known symptoms present themselves. The mercury should at once be remitted for a short time; a purge be given; and the patient will almost invariably regain the lost weight. So valuable an adjunct in the treatment of syphilis do we consider this, that in our regimental hospital a patient who is admitted with a primary infecting chancre is weighed at once, and the weight is registered every third day.

There is a third condition that I would strongly insist on, and that is, the combination of some tonic with the blue-pill; and I have found the sulphate of quinine to be most beneficial. My usual mode of administration is, three grains of blue-pill with two of the sulphate of quinine. If these precautions be carefully adopted, I feel convinced that the treatment by blue-pill will prove both safe and effectual.

We next pass to the consideration of the evidence given on the question,—Is mercury beneficial in the secondary evidences of the disease, or is there another therapeutic agent which gives better results either alone or in combination with mercury? The evidence given was decidedly in favour of good results being obtained by the judicious administration of mercury in the secondary sequelæ. Thirty-five witnesses gave their opinion on this point. Thirteen were of opinion that mercury alone should be administered; nineteen were in favour of a combination of mercury and iodide of potassium; and three only considered that iodide of potassium alone was sufficient to cure the disease. Of the thirteen witnesses who were in favour of mercury being administered alone, many considered that it was only in the tertiary stages of the disease that the iodide of potassium was of value as a remedial agent. The preparations chiefly advocated by these witnesses were the bichloride of mercury and fumigation. Those who recommended the use of iodide of potassium alone had but few followers, as experience had shown that, though a valuable adjunct to mercury, yet when given by itself it was disappointing in its results.

Mr. Henry Lee, in his Pathological Lectures, strongly advocates the calomel vapour-bath in secondary syphilis; and here I cordially agree with him. I believe, from my own observation, that in the eruptive form of syphilis there is no remedy like calomel fumigation. It cures quicker than any other mode of treatment, and the patient's system does not seem to be impaired by its use. With regard to the iodide of potassium, I believe that it does assist the action of mercury in this stage of the disease; but it is in the tertiary stages that it becomes such a valuable curative agent in eliminating the poison. When given alone, it certainly does seem to get rid of the symptoms for the time; but they return in a short period, and are as troublesome and severe as ever.

It would appear, then, that mercury is the medicine to be administered in the secondary disease, and that there is no medicine which alone gives such good results; but that the iodide of potassium, in combination with it, assists its action very much.—*St. George's Hospital Reports, Vol.* v., *p.* 77.

65.—THE TREATMENT OF GONORRHŒA BY THE URETHRAL DOUCHE.

By Thomas Windsor, Esq., Manchester.

Injections have been used from a very early period, and many and most contradictory have been the opinions promulgated about them; they have been recommended for the very commencement, and again have been restricted to the very latest stage of the disease; some practitioners have advised that they should be very strong, others very weak; stricture has been attributed to them, and they have also been thought its best preventive. The usual experience seems to have been that strong injections, generally of nitrate of silver, have not, on the whole, proved satisfactory; they are extremely painful, often fail to do good, and have sometimes been followed by serious complications. On the other hand, in order that weak injections may act efficaciously in the earlier stages of the complaint, it is necessary to continually repeat them, every hour, even every half-hour—a frequency of application to which few patients can submit; the repeated introduction of the syringe, the distension of the inflamed urethra, and the mixture of pus with the vehicle can scarcely fail to do some harm.

Such reasons led me to consider whether some better mode of applying injections could be found than the syringe, and whether it might be possible to run a stream of cold water through the urethra without risk of inoculating its deeper portion. A few trials showed that there was no difficulty in effecting this so far as the three or four anterior inches of the canal are concerned. I used at first a vulcanised India-rubber tube with a piece of lead at one end and the glass cylinder of an ordinary urethral syringe at the other, the piston, of course, being removed; the tube was filled with water, and inverted into a jug full of a weak solution of permanganate of potash, so as to act as a syphon; the force of the current was regulated by the height at which the jug was placed. Thus the solution was allowed to run in a steady stream through the urethra for as long a time as seemed desirable; at first, after the glass nozzle had been introduced, only just sufficient pressure was used to keep it in its place, the fluid being allowed to flow freely back; in two or three minutes the glans was gradually compressed, so that the fluid passed further along the urethra before it was allowed to escape. The douche was continued for five or ten minutes at a time, and repeated a few times daily. The effect was to me satisfactory; the swollen penis soon collapsed, and the patient felt a sensible relief on each application.

In the few trials that I have had the opportunity to make, half a grain of permanganate to the ounce of water appeared to be the best strength. I have also used various amounts up to six grains to the ounce, but when increased beyond two or three grains, the application caused pain, and seemed to take less effect on the discharge. As a general rule, the more acute the inflammation, the weaker should be the solution.

As the syphon is somewhat troublesome to use, I have latterly replaced it by the ordinary India-rubber enema ball and tube, the ivory end being replaced by a glass cylinder. This modification renders the process very easy. Of course, if the douche is frequently wanted, still simpler apparatus may be employed, such as a tube with stop-cock fastened to the bottom of a can, or in towns a pipe simply fastened to a water-tap would answer for water alone.

I have made a few trials to wash out the membranous portions of the urethra by using a catheter in place of the glass nozzle, but without success. Probably it could be effected by a double tube, but I have not thought it necessary to proceed further, for if the symptoms of acute inflammation in the anterior portion of the urethra are overcome, those in the deeper parts rapidly disappear as the rule.

After a time, when the discharge is much diminished, and unattended by pain, stronger injections of lead, sulphate of zinc, &c., are called for, and the ordinary urethral syringe answers every purpose.—*Manchester Medical and Surgical Reports, Oct.* 1871, *p.* 54.

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

66.—ON THE PROPER MANAGEMENT OF TEDIOUS LABOURS.

By Dr. G. Hamilton, Falkirk.

[Dr. Hamilton published a paper in 1853, in which he stated, that out of 317 consecutive labours attended by him, all the children had been born alive with the exception of one. Rather more than one-eighth of these had been delivered by the forceps. The one case was a breech presentation. Since that period Dr. Hamilton's experience has been still more favourable, having had 781 children born alive successively, the 732nd case being still-born, and that a footling case.]

Looking back on my practice for forty years in this department, one of the most striking of its late characteristics has come to be, it appears to me, its simplicity. A mere enumeration of some of the negations that have occurred in it will show this well. For example, 1st. I never now use bleeding or antimony to relax the rigidity of the os uteri. 2nd. I never now, except in special cases, use secale cornutum to hasten labour. Only a few times during the last thirty years have I required its assistance. 3rd. I now rarely, if ever, interfere with the first half of labour. 4th. I now never require my patient to have supports for her feet, or a pillow between her knees. I simply ask her to lie on her left side, and keep her knees well drawn up; and I have only occasionally, even in applying the forceps, to ask her to shift her position to the front of the bed. 5th. For thirty years I have not I think once, in my own practice, had occasion to use the catheter. 6th. During this period I have not, in forceps cases, had occasion to ask the assistance of any of my professional brethren. This I mention, not certainly in a spirit of self-sufficiency, but for the purpose of showing how comparatively easy my practice has been. 7th. I never grease the forceps before introducing them, as I think this tends to make them slip. On the contrary, I strongly recommend that the inner surface of the blades be slightly smeared with India-rubber paste, which soon dries, and enables them to lay firmer hold of the scalp than does the bare metal. 8th. I never now use forceps with a double curve. All my cases, during the period alluded to, have been delivered with Ziegler's straight forceps. 9th. I never now, in my own cases, see the parts of the mother injured more than in an easy natural labour. As for rupture of the perinæum, it never happened to me in all my practice. 10th. Although I do not absolutely refuse the use of chloroform, I use it as seldom as possible, and in certain cases decline to give it as inadmissible. I generally require in tedious labour the full power of the uterus, and I have found that chloroform often deprives me of this. 11th. I never, if I am able, apply the first blade of the forceps otherwise than over an ear. 12th. My forceps have no notches on the handles, for tying them, as I entirely disapprove of the practice. 13th. And finally, it is pleasant for me to be able to say that, as a result, I almost never have still-born children.

On several of these points I shall have to speak more fully as I proceed.

Having premised these negatives, it will be found, as I have said, that the management of the two stages of labour is thus considerably simplified. By the first stage, I mean from the commencement of the pains till the full dilatation of the os uteri, and the entry of the head into the pelvis; by the second stage, from this till the completion of labour. As I have said, I now rarely attempt to interfere with the *progress of the first* stage of labour, even when this is protracted for some days. Indeed, when I can, I keep as much as possible out o the way of my patients; recommend them to walk about or lie down, as the may incline; to take a little sherry and water to support the strength; and, i fact, I get over it the best way I can, without interference. As is well known the late Professor Hamilton laid great stress on not allowing his first stage t last more than from twelve to fourteen hours; and I can recollect we, hi students, were particularly active in following out his directions, never goin to a case without a supply of ergot in our pockets, and pestering our patient with our officious endeavours to hurry on the labour. All this I now believe t have been unnecessary and hurtful, and I imagine most of the profession ar agreed with me.

Professor Hamilton and Dr. Burns approved of supporting and dilating th uterus; Dr. Ramsbotham did not; and Dr. Murphy, as far as I know, sti holds the same opinion. I have in this matter always acted up to the instructions of my old teacher; and from the time I began to use the forceps more fr quently than was usual, I saw that the importance of assisting the ascent of t uterus was increased, for until this obstacle has been removed, the forceps never be used with freedom and safety. It may therefore properly be said to (although most necessary I think independently of this) a preparatory ste their application. I have, in thus dilating, encountered many rigid os uteri, have rarely failed in accomplishing my object, by first giving plenty of tim the first stage for the parts to become properly prepared; and, in the sec place, by using, when necessary, pretty determined force in the second st In doing this I have never met with a case where any unpleasant accident curred to the uterus, from tearing or otherwise, and the little extra pain inflic ed on the patient I have usually found well borne, where she has been assure that this was necessary to help on the labour. Where the os uteri has been forced back towards the promontory of the sacrum, and has been kept there from the head pressing the uterus on the pubes, which latter in such a case may be found to project more or less inwards, or where, on the contrary, the head and promontory of the sacrum catch the uterus, or where, again, this is done—as sometimes happens—by both pubes and promontory, then I set myself to find out what is the exact cause of the detention, and introducing the hand more or less fully, determinedly push the uterus over the head. When this has been effected, I generally consider the rest of the case comparatively simple. I was called to a labour not long ago where a midwife had worked at the case for twelve hours without getting the uterus over the head. When I saw the patient the os was perfectly expanded, but the uterus was caught by the head on bot the pubes and promontory of the sacrum, and I had to work hard for two hour and a half more before I succeeded. As time was precious after such long detention and hard work, I immediately applied the forceps and delivered—mothe and child being perfectly well next day. Now let us for an instant reflect wha might have been the consequences had the active measures used been neglected and the labour had been protracted some eight or ten hours longer. The uterus thus caught, must infallibly have become swollen and inflamed, and hav blocked up the passage; and this in all probability becoming what has bee called a "long forceps case," the instrument would have had to be applied unde the most disadvantageous and difficult circumstances, the child very likely bein lost, and the life of the mother endangered.

The more I investigate this point the stronger is my conviction that it is i many cases second in importance only to the use of the forceps. I am quit convinced, from my constant experience, that the excessive dread of many practitioners of injury to, or subsequent inflammation of, the uterus, in doing so with caution, and yet with firmness, is unfounded. At all events, I have not seen even a single case which has made me doubt its safety and propriety; and I can join with those practitioners in its condemnation only when it is used in the *first* stage of labour, which I have no doubt was formerly too much the case. I am inclined to believe that it is now becoming so clear as almost to be axiomatic, that while the safety of the child demands a more frequent application of the forceps than has been customary, the safety of the mother as decidedly demands *that* these should be applied as seldom as possible *within* the uterus.

Convulsions, or some other exceptional occurrence, may imperatively require this; an ordinary labour, if it has been well managed, I should say hardly ever does.

It is very interesting and important for me to state, as having a connection with this subject, that I have never, in all my own practice, had a case of laceration of the neck of the uterus; and I believe the reason of this has been my constant and anxious endeavours to get the uterus over the head. In looking over the former numbers of this journal, I find, in 1851, a notice of Dr. Roberton's "Essays on Practical Midwifery," in which it is stated that he himself had met with ten cases of this kind, in which seven of the females died; and he has collected from different sources seventeen other similar cases. The reviewer says, "The signs of impending danger in these cases, which Mr. Roberton thinks the most pronounced, is a feeling of crampy pain, and tenderness on pressure, on some particular part of the lower abdomen; and he explains the cause of the crampy pain by referring to a case in which it occurred as the result of the cervix being held by a 'vice-like grip' between the head and the brim." The practice which Mr. Roberton counsels, if there is space for the head to pass, is "to watch the case attentively, to apply a binder, and, *perhaps*, to raise the caught lip of the uterus," &c. In such cases of catching of the uterus I have had no hesitation as to the practice to be pursued. I *must*, if possible, get the uterus over the head, and, as I have stated, I have very rarely indeed failed in doing so.

It is with the commencement of the second stage of labour, therefore, that our active interference should generally begin, if we be called on to interfere at all. If the membranes have not been ruptured, and the head is presenting fairly, I then do so at once, and gently but firmly continue to press up the uterus with one or two fingers, or with the whole hand if necessary, until I have got it pushed over the head; and, as previously observed, I like to use the forceps as seldom as possible before this has been effected. As this is specially necessary should ulterior help be needed, I never neglect it, and work assiduously at it till it has been accomplished, if possible. Sometimes I find myself unable to do so in a moderate time, and I then make up my mind to use the forceps, or other means for delivery, under comparatively disadvantageous circumstances. In my first paper in this Journal I gave it as my opinion that it is not safe for the child especially, to allow this half of labour to continue much more than two hours, and I still adhere to that rule, as being of the very greatest importance. Much, more or less, as to interference, may depend upon the kind of labour we have to treat, but, as an average, I am convinced more than two hours cannot be trusted to; and in special cases, to secure a favourable result, we must shorten this time very materially.

It is curious to note how Dr. Murphy tries to evade the necessity for interference with the forceps in relation to this point. He says, in this "controversy a new and very important question has been raised by Professor Simpson, which, if true, would decide in favour of interference in all such cases. He has shown from statistics that the mortality is increased in direct proportion to the length of the labour; that a labour of four hours' duration is more fatal than one of two hours, one of eight hours than one of four, and so on. Hence the inference that protracted labours are dangerous because of the *time* they occupy. We have given this important question the reflection it so justly merits, but confess we cannot coincide in the conclusions drawn from it. It seems to prove too much, that not only are the longest labours the most dangerous, but that the shortest are the safest; neither of these propositions has the support of our experience. The danger of protracted labour depends upon many causes; and if the constitution be good, *time alone* is the least injurious. Rapid labours are attended with risks from which those of moderate duration are free. We do not think therefore that the shortest labours are the safest, or the longest *in time* the most dangerous. The question must be determined by individual experiences." No doubt Dr. Murphy is correct to a certain extent in this, and Sir James was clearly wrong in holding, from his statistics, that the risk to the child is in the ratio of the length of the labour *as a whole*, as I endeavoured to show in my paper in this Journal in 1853. But, granting [illegible] experience [illegible] will [illegible] than the larger increase will [illegible] very [illegible].

[illegible] have stated [illegible] with them when [illegible] they are likely to be [illegible] patient, and [illegible] effected immediately [illegible] in the more difficult one I take a good while [illegible] The forceps will sometimes [illegible] and the patient encouraged to take a few [illegible] several times before I [illegible] the head. My plan always is to assist and not to [illegible] may mention the [illegible] of nature as one of [illegible] the use of chloroform. One of the other difficulties I have [illegible] in getting a good hold of the head from its being too high up [illegible] other pains are of great importance in bringing [illegible] the forceps being already introduced and ready to lay hold of [illegible] the pains and in the very presents an obstacle [illegible] operation, in general, therefore, I [illegible] For exactly the converse [illegible] that I have used the ergot [illegible] to prevent flooding. [illegible] effect the purpose [illegible] when the [illegible] great practice [illegible] in the first stage of [illegible] very few [illegible] now be inclined to dispute.

[illegible] with the forceps I would divide [illegible] the head is well down in the pelvis, [illegible] with the face to the same side. [illegible] remarkably easy to manage. [illegible] down, and the face passes [illegible] the child is delivered. [illegible] hospital practice [illegible] in this class that the principal [illegible] will be found. The case probably is [illegible] the patient is not exhausted, and valuable [illegible]

[illegible] a part of the subject for the present le[illegible] *from my notebook, dated* May 2, 187[illegible]:

[illegible] at [illegible] to Mrs. B., [illegible] Th[illegible] [illegible] had been with her sinc[illegible] *moderate*, but was now making l[illegible] *[illegible] I found the head* [illegible] *down* towards the perinæum, an ear e[illegible] and a [illegible] The patient had become uneasy and *I applied the forceps and delivered* with the assistance of three pains, *being found round the neck and short."* On the above notes I mak[illegible] *mark*, that I should have liked *some* of those practitioners who used, *ago, to apply the forceps once in 500 or 600* times, to have been pre[illegible] *seen* the case, with which this really trifling operation was gone through *safety and smiling comfort* it at *once* brought into a household. The[illegible] *indeed, in such a case*, in my opinion, have been "obstetrical reprobat[illegible] phrase is not mine, but Dr. Murphy's, which, he tells us, he quotes from Dr. Blundell, *with approbation*, which I certainly do not) if they had failed to see and admit the vast benefits which the more frequent use of the forceps has conferred, and is likely in the future to confer, upon humanity. By no other operation that I know of, except, perhaps, vaccination, can such a saving of human life be effected, and in the great majority of instances I have found the performance of the one operation almost as simple as the other. If we take the annual births in the United Kingdom to be about 1,000,000, a lessening of the infantile mortality in these by only one per cent. would give us a saving of infant life in each decennial period of no less than 100,000.—*British and Foreign Medico-Chirurgical Review, Oct.,* 1871, *p.* 449.

67.—ON RESTORATION OF THE PERINEUM.

By Dr. J. Matthews Duncan, Edinburgh.

[The term restoration of the perineum is not altogether free from objection, inasmuch as the operation has sometimes to be performed when the perineum is anatomically intact, but functionally imperfect. The cases in which this operation is absolutely necessary are those in which the whole sphincter is destroyed; but if the sphincter is only partially injured its action is impaired, and entails a loose condition of the bowels, almost amounting to diarrhœa.]

In cases of procidentia, whether the perineum be anatomically entire or not, it is always functionally imperfect. It does not prevent the passage of the procident organ or organs over it and through the vaginal orifice, as it might do. It is gradually distended, the vaginal orifice is dilated, the perineum or its remnant is pushed backwards, and its antero-posterior length is more or less completely annihilated. In this way, cases which begin with an entire perineum are at length brought, not anatomically, but functionally, into the same condition as those in which the perineum was previously extensively ruptured. When the procident parts are pushed back, and the perineal region is exposed, there is found a very unnatural condition of parts. Instead of the labia majora being in contact, or nearly so, there is seen a large rounded gaping vaginal orifice, generally closed by the rugous anterior vaginal wall, and extending from the clitoris in front to the narrow transverse strip of perineum behind. This condition cannot but cause symptoms, and accordingly the woman describes a feeling of disagreeable openness. This gaping orifice the surgeon closes by restoring the perineum, making a new and long perineum anatomically, with the hope that it will efficiently assume the function of closing this orifice, so as to prevent the return of the procidentia. After the operation is finished, and again after the cure is complete, the surgeon finds, when he exposes the perineal region, a quite different appearance. There is now no gaping orifice; no mucous membrane is visible; the labia majora are in contact—indeed they are extensively united.

The operation which I now propose to describe is very generally successful. Success, indeed, may be counted upon. It is rarely that a second operation is required. I have operated on cases for the second time, but not in any case which was from the first under my own care. Previous failure does not prevent future success.

When the operation is performed for injury of the sphincter, the woman feels her renewed power to restrain flatus and fæces however liquid. This renewed power is not in every case complete; for it occasionally happens, that although there is great improvement, and consciousness of regained sphincteric power, yet the recovery is not quite perfect; the control over feculent and flatulent evacuation is not in its pristine perfection.

When the operation is performed for the cure of procidentia, the cure is complete or the failure is complete. This does not imply that a bandage is not a judicious and advantageous application to aid the restored perineum to do its work of restraining the advance of the organ which tends to fall out. The cure is complete, I have said, and it is a cure of which the surgeon may be proud.

The cure is not only often complete, but also permanent. In illustration I may cite a case, and it is not of a rare kind. A multiparous woman, whose employment keeps her almost constantly standing, walking about, stooping, and lifting weights, had a large procidentia. She was operated on and cured by a colleague. The cure lasted a long time. She had again a child. The cure still lasted. Again she had a child and soon after this the womb came down as badly as ever. The restored perineum was still anatomically entire; the cicatrix of the first operation could be traced; but its restraining function was lost: it was thin and relaxed. The operation was repeated. About three weeks afterwards she returned to her arduous employment, and was and now is quite cured. As her husband is dead, it may be hoped that the cure is for life.

What is the proportional number of failures and of successes after this operation I cannot tell. It is a proceeding to which I frequently resort, but I have found it impossible to trace the subsequent histories of the great majority of my patients. The sufferers from aggravated procidentia are for the most part very poor women, whose hard lives and frequent changes of residence form some excuse for their failing to fulfil promises to keep the surgeon informed as to how they are "getting on."

I have just mentioned a case in which the restored perineum continued entire after two births at the full time. The retaining function of the perineum was, in that instance, destroyed, not by laceration, but by distension. I have repeatedly seen cases in which the birth of a mature child did not extensively or injuriously lacerate a restored perineum.

The operation runs great risk of failing from disorder of the healing process, the opposed raw surfaces secreting pus instead of uniting, if the subject of it is very old, if she is of syphilitic constitution, or if the parts are the subject of recent or of chronic inflammation. Of course, under these conditions, it should not be attempted.

In cases of laceration of the perineum the operation may be undertaken at the time of the accident or after the injured parts are quite healed. I have done it successfully at both times, and if circumstances are all favourable I would recommend its performance at the time of the accident. Yet I think a successful result can be more securely predicated of a delayed operation than of an operation done at the time of the injury. It is to be remembered that the perineum often appears to the inexperienced to be extensively injured during labour, and so as to demand operative interference, when truly very little harm has been done; as is made apparent by examination after the parts are healed.

The instruments required for the operation are the following:—a sharp bistoury, a dissecting forceps, a catch forceps, ligatures for arteries, needles armed with silver wire for stitching, and a scissors. Besides these, the ordinary appliances for all operations must be at hand, as sponges, &c.

The operation should be done soon after a monthly period has passed; and in preparation for it, the bowels should be freely evacuated. On the morning of the operation the patient should have only soup or some light nourishment in order that the vomiting so frequently accompanying and following the induction of anæsthesia, which is maintained during the operation, may be kept within the narrowest bounds.

In describing the operation, I shall suppose the case to be one of laceration of the perineum, the fissure extending through the sphincter ani. It will be unnecessary to go over the steps of the operation for procidentia, because, *mutatis mutandis*, they are quite the same as in a case of laceration.

The patient is placed and held in the position for lithotomy, and the surgeon is placed as he is during that operation. With his fingers or with forceps he seizes the fourchette, or that part which corresponds to it, transfixes it with the bistoury, and then continues to cut, first on the one side and then on the other, upwards from the fourchette or its representative as far as he deems necessary. In cases of procidentia it is usual to make the raw advance as far forward as to be nearly on a level with the orifice of the urethra. The operator thus removes a long tape-like piece of integument, which is about half an inch broad, rather less than more; and he leaves a horseshoe-shaped wound in which the point of the shoe is at the fourchette. It is important that the proper piece of integument should be removed, and it is at some parts a matter of care to secure this. At the fourchette there is little difficulty. It is quite easily made out, or may be made to project by separating the labia. Farther forwards on the sides of the vaginal orifice, the junction of skin and mucous membrane is sought for as the line of the wound, and it is not always quite easily found. The anterior margin of the wound is at or involves the posterior extremity of each nympha, and the line of the wound runs between this and the fourchette, its course being sometimes marked by the opening of the duct of the vulvo-vaginal gland on either side.

Generally two, sometimes four, arteries require ligature. Two of these are in front of the sphincter, one on either side; the other two are generally further forwards near the nymphæ. A few minutes' delay is now caused by waiting for the complete or nearly complete stoppage of oozing from the raw surface. Then the wound is closed.

A series of silver wire sutures is passed about one-third of an inch apart. The wire sutures, after being placed, are observed to pass through the wound near its deeper margin, and emerge on the skin or rectal mucous membrane about a third of an inch distant from the outer margin of the wound. Beginning posteriorly, each suture is tied with some firmness, the edges of mucous membrane and of skin being carefully adjusted to one another. Now the bladder is evacuated, the vulva is washed and dressed with some wet lint, and the operation is finished.

The alvine evacuations are stopped for about ten days by daily use of opium in some form. I use solid opium in one-grain pill at bed-time, or oftener if there appears need for it. The patient is fed on light food and sparingly. The urine may be drawn off twice or thrice daily, or the patient may herself make it while lying. The wound requires to be kept clean by daily dressing. Care should be taken that discharge does not accumulate in the vagina.

On the seventh or eighth day after the operation, the stitches are removed. Twenty-four or forty-eight hours afterwards the bowels, if they do not spontaneously move, are acted on by castor-oil.

The removal of the stitches is done as in the operation for vesico-vaginal fistula. A dissecting forceps is made to seize the projecting end of the ligature, and to tighten the loop by traction; then one blade of a sharp-pointed scissors is insinuated within the loop, and the scissors is made to cut it; after which it comes away by the traction of the forceps. But in a case of extensive perineal laceration it is sometimes difficult to get at the deepest or farthest back sutures. They may be half an inch or more within the margin of the new anus, and require considerable care in dealing with them.

Very many different plans for performing this operation have been proposed and practised; and of these many involve proceedings of which I disapprove, such as removing large portions of vaginal mucous membrane, and incising the sphincter ani. The plan I have rapidly sketched has the recommendation of being as simple as possible, and I have had such considerable experience of it as to justify my preferring it on the best grounds as being at the same time very successful and satisfactory.—*Edinburgh Medical Journal, November* 1871, *p.* 385.

68.—PREGNANT SICKNESS.

By METCALFE JOHNSON, Esq., Lancaster.

In considering the effects of remedies upon the human body, the possibility of error in some form or another presents itself so frequently as to induce great hesitation to accept coincident results as necessary consequents. There are, however, some drugs, such as opium, whose effect in sleep is seldom doubted. The point to which I desire to direct attention is one which is beset with difficulties. My object now in writing is to invite consideration for the phosphate of lime as a means of relieving the sickness consequent on the pregnant condition. But when we consider the double relation of mind and body, through the ganglionic nerves and the disturbed state of their functions, in all cases in which th control of the ganglionic nerves, or the great sympathetic, is interfered with b an abnormal condition of organ or organs under its especial rule, we shall s that it requires especial watchfulness to be sure that we do not mistake a "*po* *hoc*" for a "*propter hoc*" in those instances where relief of symptoms has follo ed the exhibition of the remedy. Before proceeding to remark on the theory *modus operandi*, &c., the simple use of the drug may be described. For som years past I have been in the habit of prescribing the simple hydrated pho phate of lime of the Pharmacopœia in doses of from three to ten grains eac three times a day, suspended in water, and flavoured according to the taste the patient. I have tried the remedy dissolved in hydrochloric acid, as also t powder in the dry state, besides having had it made up into biscuits; but none of these forms have the same agreeable results followed so frequently when the simple hydrated phosphate has been used suspended in water.

One remark may here be made respecting disorders of the great sympath and its subjected organs, that they are generally characterised by a dislike of sweet flavours. This has been noticed in the case of persons whose ganglio system is disordered through the stomach by the abuse of alcohol; for I th it is Coleridge who says there is always hope for a man so long as he is fond his pudding. But in those other forms of female ganglionism which, for wa of a proper diagnosis, we designate under the generic term hysteria (though many cases the ὑστερον has nothing to do with it,) the taste not only takes a aversion to sweet things, but has apparently, a depraved tolerance of the flavou of fetid preparations and the alkalies, together with an ability to receive th stimulus of both alcohol and the carminatives, such as lavender, ammonia cardamoms, &c., with advantage. The relation of this sympathetic nerve t certain conditions of the circulation is a subject worthy of more attention tha it has at present received; more especially since the *Saturday Review* has, wit a one-sided view of the matter, taken up such a raid against "alcoholism."

With these complex considerations we approach the subject of pregnant sick ness or vomiting. Here we have, of course, an enlarged uterus, which physical ly bears a relation to the various organs of the body, such as the stomach, larg and small intestines, liver, gall-bladder, kidneys, &c., different from that i health, pressing upon each, and producing a state of things with reference t each organ which, if brought about by traumatic means, would in any cas bring on nausea and vomiting. But, in addition to this, the very relation t the nerve is altered, and in many cases this great change is attended with no only bodily suffering but temporary mental aberration. I have not unfrequentl seen temporary insanity of a few hours' duration attend both the act of concep tion and the act of quickening. Everyone is of course familiar with the puerp ral insanity as well as the frequent insanity which is associated more or les directly with ovarian or uterine disease.

A short time since, Mrs. A. B., aged 24, second pregnancy, during the la month has had violent spasms all over the body, with strabismus. Mouth an hands clenched. Says she has pain all through the womb. Has great sensibi ity in the nipples of the breast. Bowels generally confined. Has had bleedi at the nose for the last few days. Her mother is a very excitable, clever w man and one other close relative a somnambulist. The spasm was relieved b subcutaneous injection of morphia. I emptied the bowels by an aloetic enem and gave her the phosphate of lime, which she took for three weeks, after whic she was delivered of a very small child, the parietal bones of whose head co sisted simply of two centres of ossification. Since her confinement she h been well. The spasm *never* returned after using the phosphates. I have ofte had this proof of the efficacy of the phosphates in arresting the sickness: th patients have been sent to me for "some of that medicine that relieves the sic ness."

I had a patient a few weeks ago, who had been complaining for some wee of an irresistible vomiting after every meal, who no sooner took the phosphat than all sickness ceased. This of course might be the effect of expecting to *relieved*; but the cases have occurred too frequently for me to think other th *that relief has* been most frequently the result of the use of the phosphat

As such, I trust that some of your readers will be induced to give the remedy a trial, for which I now proceed to give a physiological reason or *ratio medendi.*

As we have seen, the altered shape of the uterus, the altered nerve relations, the control of the ganglionic nerve to supply the new arterial system to be established, make a demand upon nervous influence which is very unusual. Nervous power cannot be expended without harm unless the supply of new neuric elements makes up the deficiency. Neuric force derives much of its nutrition and source from phosphates. Moreover, the child in its formation requires more phosphates for its new bones, and if these are supplied at the expense of brain and ganglionic nerve, it follows, as a matter of course, that debility, nervousness, and all the concurrent train of symptoms must inevitably be brought about; and hence arise those feelings of depression, peevishness, and irritability so frequently associated with the pregnant state. Nor is it to be wondered at, if we consider that we take no steps to supply the new demand made upon the blood.

This view of the case is again supported by pathological evidence, when we see how that fractures in pregnant females are more frequently liable to non-union. These considerations induce me to believe that the remedy is really the cause of the relief so constantly expressed by the patient after its use for a few days. I have also for some years been in the habit of using this form of phosphates for the relief of rickety children with great success, which further confirms me in the belief that phosphates administered through the stomach do become used by the blood. I have used for children the saccharated wheat phosphates supplied by the druggists, but in the case of pregnant females I have not found these so useful, and chiefly I suspect, owing to the sugar which they contain.—*Medical Times and Gazette, July* 1, 1871, *p.* 7.

69.—ON THE MODE OF INTRODUCING THE MIDWIFERY FORCEPS; WITH NOTE ON THEIR MODIFICATION.

By Dr. James Cappie.

[In the following paper the writer draws attention to a single point in the management of the midwifery forceps. In works on the subject the instructions given are almost stereotyped, so much so, that it might be supposed there could be very little opportunity to suggest anything novel.]

The instruction I have to criticise is, that in introducing either of the blades, it is to be held lightly in the right hand, and that two or more fingers of the left, introduced into the vagina, should guide the point of the instrument, and guard the parts of the mother; and the amendment I have to submit is, that the task of introducing, guarding, and directing, should be trusted entirely to the fingers of the right hand, and that, in the first stage of introduction, the left, crossed over the right wrist, should have little more to do than to support the shank or handle of the instrument. The advantages of this plan are that the ordinary obstetric position does not require to be disturbed, that in many cases the instrument can be used positively with greater facility, and, at all events, that in this way it can be applied in instances where it would be almost impossible in the ordinary way.

At the outset, I must mention that my remarks are intended to apply to the double-curved instrument. My experience of the straight forceps has been limited, and, so far as it goes, decidedly unfavourable. In leverage power, and in the thorough command we have over it, it is greatly inferior to the long forceps.

The first objection I have to the ordinary instructions is the awkward position in which we are directed to place the patient. She is to lie as much as possible right across the bed, and, according to some writers, the nates are to be made to project over its edge. To put her in this position, of course involves trouble to the patient, and all the anxiety to her and her friends that attends preparations *for what is* regarded as a serious operation. It will not be denied that, when it has been determined to use instruments, the less formidable the procedure can be made to appear to the patient and her friends the better. Other things being equal, that plan should be adopted which can be gone about in the quietest manner. The position across the bed, however, is quite necessary if two or more fingers of the left hand are to be introduced into the vagina. When the perineum is relaxed the ostium vaginæ dilates, not from before backwards, but more nearly in the direction of a line drawn in continuation of the symphysis pubis. The twisting of the left wrist, therefore, would be extremely awkward if the body of the patient is not kept well forward.

With the plan I have followed, and would now advocate, the ordinary position does not require to be interfered with. The patient simply requires to be conveniently near the side of the bed. In very many cases it is a matter of indifference to myself whether she be lying on her back or on her side. If she is not under chloroform, and is anxious to facilitate procedure, I prefer a posture between the two. The right thigh can thus be more readily managed than when she is lying fairly on her side.

In regard to immediate management, we may take one of the simplest cases, where the head is at the outlet and the perineum is relaxed. An examination being made in the ordinary way with the right hand, the blade to be applied first—that is, in most cases, the lower or left iliac blade—is held in the left hand (which, as I have already said, is crossed over the right wrist), and the point directed to the margin of the outlet. If the perineum be well relaxed it will at once slide within the vagina, along the side or inner surface of the finger. Or, allowing the middle finger to remain within or at the edge of the vagina, the forefinger is made to catch on the inner rim at the rounded end of the fenestrum, and to draw the point cautiously onwards. If the patient be stout and the perineum thick, a little care is necessary to prevent any fold of skin or mucous membrane being caught by the edge of the instrument. One or other of the fingers feels cautiously round while the point is being coaxed onwards; for, of course, through all the stages of introduction and adjustment any great amount of force is out of the question.

The more firm the perineum is, the more necessary is it for the handle of the instrument to be directed well forwards. If the outlet of the pelvis be contracted, the handle may with advantage be even inclined to some extent across the left thigh, so that it is made to describe a complet half-circle as the blade is being made to sweep round into its proper position on the head.

When the point of the instrument is fairly within the vagina, its progress and direction are at first to be trusted almost entirely to *the forefinger pushing on the rim of the fenestrum.* I believe those who have never tried this plan will be surprised to find how easily it can be urged onwards, and how safely the proper direction can be given.

Of course, the direction for the first blade is, as a rule, first backwards along the perineum towards the sacrum, and then downwards and forwards, so that the outer surface of the instrument is toward the left ilium ; and for the second blade, it is first backwards and then upwards and forwards, so as to lodge it between the child's head and the right ilium.

As long as the rounded point of the instrument is within reach it is pushed onwards by the forefinger, and the left hand has scarcely anything more to do at this stage than to allow the handle to rotate, and to be elevated or depressed according to circumstances. When the finger has pushed it onwards as far as it can reach, the left hand becomes the propelling force. The forefinger, however, still acts at any convenient point as a fulcrum to assist in giving the proper direction to the blade, while the hand gives the rotating and forward movement. If the os uteri has previously been slipped over the child's occiput, there is usually no difficulty now in at once adjusting the blade to the proper position on the head.

In this process there is wonderfully little risk of any injury being done to either mother or child. Ramsbotham indeed remarks, that if "a circular sweep *of a* portion of the pelvis is made, the maternal structures might be endanger*ed.*" *But it is really* difficult to conceive how such an accident could happen, except by the grossest carelessness. When the perineum is at all distended, the surface of the vagina is so smooth that it would be difficult, even intentionally, to make the edge of the instrument to catch on any fold of the mucous membrane. If the head is high in the pelvis the probability of doing so is still small, and may be avoided by the most ordinary care. I have applied the forceps in the way now described for twenty years, and have never met with any accident to the maternal passages by the sweep of the instrument. In regard to the progress of that part of the blade which is out of reach, the operator has first to have a clear conception of the direction he wishes it to take, and then to proceed with the caution, but, at the same time, with all the confidence that the surgeon has in passing the male catheter.

If the os uteri is still felt all round on the head, the *tactus eruditus* of the forefinger comes to be of great assistance. The instructions usually given to introduce the fingers of the left hand within the os, and to feel for the child's ear, appear to me to be not only needless but to be cruel to the patient, and indeed the attempt must often be difficult for the operator. The forefinger by itself, being intimate with the topography of the localities can at once determine the position of the instrument relatively to the passages and the child's head. It not only takes up greatly less room, but it is positively more efficient. It is usually a very simple matter to introduce the point of the blade within the os, especially if the latter has been pushed low into the pelvis. When it is not easily reached, the method of procedure resembles what I have detailed for introducing the blade into the vagina. First, the finger tilts on the os, while the left hand cautiously advances the point of the blade till it rests on the child's head. Then the finger hooks itself into the fenestrum, and, if possible, keeps the point sliding along the scalp till it catches the lip of the os and is pushed within it. The part at which I prefer to enter the instrument within the uterus is, for both blades, at the poster or or sacral portion of the os uteri.

If the point has passed the os, and we are uncertain whether it is outside or within the lip, all that is necessary is to draw back the instrument slowly while the finger is resting on the os. If it be within the lip, it will be caught on the tip of the finger, and no hesitation need then be felt to urge it steadily onwards, and to give the necessary rotatory impulse to bring it into position.

It may sometimes be an advantage to be able to apply the forceps although the os uteri is not completely dilated, as in cases of severe convulsions, when uterine action is completely in abeyance, and the only hope for the mother is in the speedy termination of the labour. Now in the manner I have described the forceps may be passed into the uterus when the os is scarcely more dilated than just to admit the breadth of the blade. On this point, however, I shall not at present dilate, as I have brought it before the Society on a previous occasion.

In regard to the introduction of the second blade, the same general management is to be attended to. Introduced in front of the first blade, it is directed backwards along the perineum, and then upwards and forwards towards the right ilium. Here, as before, if the head be low in the pelvis, no difficulty is experienced in introducing it within the vagina, and in adjusting it on the head. If the outlet is contracted, the handle may be inclined across the right thigh, at the first stage of introduction, and a complete half-circle be described in the adjustment. If the perineum is thick, or the patient stout and with sturdy muscular thighs, a convenient mode of introducing the second blade is to project the handle so well forward that the left hand can support it in front of the thighs. When the patient is lying on her back, it can thus be passed with great facility. The forefinger again acts as the propelling agent in the first stage; and, according to circumstances, it does so either behind or before the shank of the first blade.

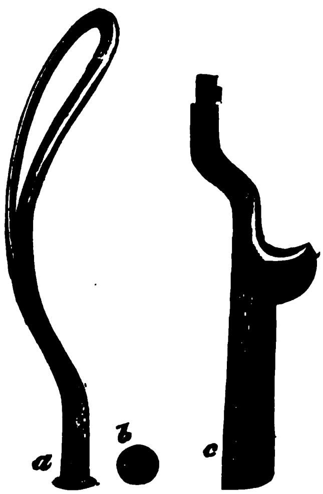
There is one point which requires a little study in the process of adjusting the second blade. Very often, when it is being made to sweep round to its position on the head, the handle is apt to be caught on the mattress; and this may sometimes be to an extent to make it appear to be impossible for the sweep to be completed. Usually a little coaxing will overcome the difficulty, but if this is not sufficient, all that is necessary is to get the patient to lie a little more on her back, and the handle may then be made to go round without interruption.

In regard to the locking of the blades I have nothing new to suggest. In most of the cases they lock readily with the first attempt. When they do not do so easily, an examination must be made to ascertain whether one of them cannot be better adjusted. I believe it will be found that if one of the blades inclines to fall easily out of position, that is the one which requires to be further introduced or rotated. It is seldom necessary altogether to withdraw either blade.

If the adjustment cannot be improved, some force may be employed to complete the act of locking. The shanks are brought into contact as near the lock as possible. Then each handle being held firmly, the one in the right and the other in the left hand, the thumbs are made to press steadily on the one side of the lock, and the forefingers firmly to support the anterior shank on the other side. The point of contact is the fulcrum, against which each handle, as a lever, is made to press from opposite directions. Considerable force can thus be applied, especially if the handles have transverse rests below the lock. Of course, the hands are ready to complete the locking as soon as the twisting or compression is sufficiently accomplished.

This paper might be considerably extended by considering what may be done in particular contingencies, but this is unnecessary. I wish my remarks to be taken as hints rather than elaborate details. I believe, indeed, it would be impossible to lay down rules that must apply to all the cases that one meets with in practice. A great deal must be left to the common sense of the practitioner in adapting general instructions to the peculiarities of individual cases. Where there is pelvic deformity, or mal-position of the head, or strongly developed pelvic ligaments, more depends on the mode of traction and general management than on the mere act of introducing the blades; but these are points on which I do not enter in the present communication.

Note on Modification of the Midwifery Forceps.—A few years ago I showed an instrument at this Society intended to be such a modification of the long forceps, that the shank being cut at the middle, a joint was contrived by which the pieces could with facility be joined or detached. The Simpson forceps could thus be made as portable as the common short forceps. In contriving the joint, there was a rather intricate mechanism of spring and catch, to prevent the pieces becoming too readily disjoined. After some experience, however, I have discarded this mechanism as quite unnecessary, and for several years have found the simple instrument, which I now show to the Society, to answer admirably.



The two portions are joined by what is known as the bayonet joint. On the blade portion, *a*, there is a socket, in the rim of which is a notch, *b*, which leads to a horizontal slit extending about one-third round the circumference of the socket. On the handle portion, *c*, is a nipple which fits into the socket, and on it there is a small knob which enters at the notch. When the nipple is fairly inserted, and a quarter turn is given to the handle, the knob slides along the horizontal slit, and the blade and handle portions have then exactly the same relations to one another that they have in the solid instrument.

In making the socket, it is very necessary to attend to the direction of the slit. In both blades the notch for receiving the knob of the nipple is opposite the concave *edge* of the blade, and the slit itself terminates opposite the concave *side*. It will then be found that, with either blade, the rotation necessary to join the pieces is in the same direction as the rotatory movement that must be given *when the* blade is to be adjusted to its position on the head.

On looking at the instrument as held in the hand, I dare say the first imprestion may be that the joints are insecure, as the tips of the blades appear to fall readily inwards. But it is to be remembered that in practical use the child's head is between the blades, and when they are locked the whole instrument is as immobile as the solid-stemmed long forceps. It is then an impossibility for either handle to slip away from its blade portion. The whole of the strain in compression and traction is thrown on the solid part of the socket.

In using the instrument, I first introduce and adjust the blades and then join the handles. Sometimes, however, as when the head is at the brim of the pelvis, I may join the handle on the second or upper blade, to assist in the rotation necessary to place it in position on the head. I do not find that the blade, when once adjusted, tends to slip out of its place more readily than that of the ordinary instrument. After introducing the first blade, I have purposely left it alone for some minutes without finding it displaced. Indeed, one would naturally expect that the absence of the weight of the handle would be rather an advantage in this respect.

The act of joining the handles is a very simple process. The notch in the rim of the socket being felt for, the nipple is introduced with the knob opposite the notch. When fairly inserted, the handle of the lower blade is turned upwards, and that of the upper blade is turned downwards. The instrument now resembles the ordinary forceps, and locking is made in the usual way; of course, care is to be taken that the shank of the upper blade is in front of that of the lower when the handles are joined.

As, in my former paper, I narrated some cases to illustrate the advantages of this modification of the midwifery forceps, I shall not now detain the Society by further remarks. I may only mention that, after other nine years' practice, I have never experienced any serious difficulty in regard to the speculative objections that were urged against the instrument.—*Edinburgh Medical Journal, August*, 1871, *p.* 117.

70.—ON USING LONG FORCEPS WITH THE PATIENT IN THE LATERAL AND SUPINE POSITIONS.

By Dr. Andrew Inglis, Professor of Midwifery in the University of Aberdeen.

In a former paper* I showed that, in short forceps cases by adopting the supine position and by introducing the blades from the front, as is done when using the catheter on a man who is in bed, the otherwise inevitable preliminary disturbance, and the consequent exposure of the patient are entirely avoided, while the instruments following during their introduction the antero-posterior curve of the pelvis, meet with the smallest possible resistance. As to extraction in that position of the patient, I showed that the operator had more power to act, and also that by using it, more relaxation of the soft parts at the outlet was obtained than by any other posture.

This position, cannot, however, be adopted from the first in long forceps cases, for, when the instruments are properly adjusted on a head at the brim, the handles project backwards in the line of traction, and would be buried in the mattress were the patient lying supine in the middle of the bed, so that, to adopt the supine position at all in such cases we must bring the nates *over* the edge of the bed and double up the limbs as in lithotomy. This is the method universally adopted on the Continent but is not satisfactory. It is offensive, demands more assistance than is required in any other position, and, moreover, it puts the perineum more on the stretch than need be.

The plan I have adopted for some time for the high operation is as follows:—

The patient is laid on her left side diagonally in the bed, but not with the breech projecting over the edge, her head low, the limbs not drawn up much, only slightly flexed, the knees being kept a little apart by a pillow.

Introduction of Blades in Occipito-anterior Presentations.—The occiput having been ascertained to be near the right groin, the two first fingers of the left hand are introduced along the back wall of the vagina till the head is reached. Their points resting firmly on the head, are to be pushed up to the most posterior part of the presentation and retained there, keeping back the os and folds of mucuous membrane. After this, the left half of the forceps is introduced, at first it is held horizontally, the handle projecting forwards between the limbs and the convexity of the blade lying in the palm of the examining hand. The blade is then made to slide in along the anterior surface of the fingers, observing the mesial line of the pelvis, till it arrives at the point where they touch the head. After this a partial rotation by pronating the right hand, is made in order to send the blade laterally, which results in a slight raising of the handle towards the patient's right hip. It is then pushed on, the handle passing in its progress backwards and downwards till it rests firmly against the perineum. The blade in its course through the vagina, traverses over the front of the pelvic portion of the rectum, and as it gets into the uterus, passes more forwards, and when finally adjusted, lies immediately in front of the transverse bi-section of the pelvis at the brim, with its tip resting on the head behind the left ear. The second blade is next introduced. The fingers of the left hand rest on the head as before, the right blade is passed along their palmar surface horizontally, keeping the mesial line of the pelvis till it reaches the head where the fingers touch. It is then rotated partially, by supination of the right hand, the handle is slightly depressed, and then pushed onwards, upwards and backwards, till the locks meet at the perineum. The second blade will lie in the posterior half of the right side of the pelvis at the brim, opposite the sacro-iliac joint, and should cover the right zigomatic region of the head.

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* *Medical Press and Circular*, June 8th, 1870.

In the second position of the head, the introduction is exactly similar in all respects, excepting that the blades must be made to grasp the head, while they lie in the reverse oblique diameter, the left blade being sacral, and the right pubic.

Traction.—When the patient is placed diagonally in the bed, with the breech far from the edge, the traction can be applied during the first part of the proceeding, in a most effective manner. Her body sinks into the mattress, and is not easily displaced, while the operator stands straight before his work in a good position for exerting force with accuracy. As the head progresses, the power of pulling and guiding gradually diminishes, for the handles get forward between the limbs; but this allows the patient to be turned on her back, and the case becoming a short-forceps one, is much more easily terminated in the supine position. Apart from theoretical considerations, proofs of this may be met with in practice—for I have more than once seen it happen, that where the head could not be moved while the patient was on her side, it passed through easily after she was turned on her back.

Occipito-posterior presentations.—With regard to the third and fourth presentations of the head at the brim, no such definite rules as the foregoing for using forceps can be laid down, for the course of the head through the pelvis in these presentations, varies considerably. For instance, it may turn as it advances into an occipito-anterior presentation, and may then be treated as such, or the occiput may descend into the hollow of the sacrum, so as to oblige the head to pass out of the pelvis with the face under the pubes. In the latter case the handles sometimes project so far backwards at the end of the extraction that it would be impossible to put the patient on her back in the bed, and the head may also emerge in the oblique diameter of the outlet, the occiput passing over the sciatic notch of one side. This oblique course of the head is by no means rare in such cases, and were the patient on her back, the handles could not be diverted sufficiently to one side.

The conclusions I would point at, in connection with this and the previous paper, are as follows:—

1st. In all long forceps cases, introduction of blades can be effected from the front.

2nd. Therefore, the patient can lie in the middle of the bed during introduction.

3rd. Consequently, traction during the first part of operation is more effective.

4th. After the head fills the pelvis, the case becomes a short forceps one (except in the rare cases quoted), which can be more advantageously treated by using the supine posture.

Before leaving the subject, I would again refer to the pernicious custom of doubling up the patient, and thus tightening the soft parts concerned with the outlet of the pelvis; even in ordinary labour in primiparæ advantage ought to be taken of this supine position to slacken the perineum, and the relief which is given by doing so, is often very marked—most women, if left to choose their own position at the end of their first labour, turn on the back of their own accord, and if they do so, they can hardly be persuaded to turn again on the side. —*Medical Press and Circular, June* 21, 1871, *p.* 526.

71.—ON DECAPITATION AS A MODE OF DELIVERY IN SHOULDER PRESENTATION, IN WHICH VERSION CANNOT BE SAFELY EFFECTED.

By George H. Kidd, Esq., President of the Dublin Obstetrical Society.

[In cases of shoulder presentation in which turning is impracticable, or highly dangerous, owing to the shoulder being wedged in the pelvis, and the uterus firmly contracted upon the child, we have only the choice of two modes of delivery, evisceration and decapitation. The former is the most modern, and, in some cases, the easiest, but more frequently it is difficult to perform, and dangerous to the patient. Decapitation is a much older operation; it was described by Celsus, and many writers since his day. Sir James Simpson considered it preferable to evisceration in cases of the class in question, as being safer to the patient and easier to the practitioner.]

The instruments that have been used for dividing the neck of the fœtus, in utero, may be arranged in three classes:—1st. We have hooks with variously devised cutting blades. The first of these, and the model on which the others of this class have been formed, is the decapitating hook of Ramsbotham, senior, which is, in fact, the ordinary blunt hook, with a cutting blade fixed in the concavity of the curve, and which resembles much the instrument described by Celsus. Davis, Scanzoni, Jacquemier, and others have modified this hook by the introduction of a guard or sheath, or of cutting blades, made to move by screws and levers in the handle, for dividing the neck. Dubois uses a pair of strong scissors with which he cuts through the neck, bit by bit, from below upwards. Professor Heyerdahl, of Bergin, adopted a different plan. In 1855, he invented an instrument consisting of a handle and hollow stem, together, 14 inches long. The extremity of the stem is curved and forms about a third of a circle. The instrument is hollow, and a strong wire passes up the centre. The wire is pushed up by means of a button sliding in the handle, and at its extremity there is a little knob which closes the open end of the stem. The instrument is used thus:—After it has passed round the neck the internal wire is pushed upwards, and this projects the knob round and beyond the other side of the neck. A loop of string can now be passed round the knob, and, on withdrawing the instrument, a cord is thus left round the neck, with which a chain or wire rope is drawn round, with which the neck is sawn through, and, to avoid mutilation of the soft parts of the mother, the rope or chain is crossed in the operation.

Without appearing to have known anything of Heyerdahl's method, Pajot and Tarnier have adopted modifications of it. In the Archives Générales for September, 1865, Pajot published a paper on presentation of the shoulder, where there is extreme narrowing of the pelvis, and on a new method of embryotomy. This new method is virtually Heyerdahl's, and consists in sawing through the neck with a cord. To get this cord round the neck and at the same time, to avoid the multiplication of instruments, he makes use of the handle of his forceps, which is very much bent, and the extremity of which he has perforated, and through this perforation he passes a cord, on the end of which is a leaden bullet. The cord is pulled till the bullet is closely applied to the end of the bent handle of the forceps, which is then passed up into the uterus and hooked on the child's neck. The cord is now relaxed so as to allow the bullet to fall and roll down into the hand of the operator; but if the compression of the parts should prevent the ball from finding a passage, a simple pressure on the fœtus with a finger or the bent hook will immediately determine the formation of a sort of gutter in which the ball will at once engage itself. As soon as the cord is fixed in its place the instrument is withdrawn, and the ends of the cord are passed through a speculum, which is introduced into the vagina to save it from injury, and by drawing the cord rapidly backwards and forwards the neck is cut through in a few seconds. If the neck should be inaccessible, the cord may be placed round the body, between the crest of the ilium and shoulder blade, but it will occupy four or five minutes to effect the division here. Pajot's colleague, Tarnier, seeing Pajot's experiments, suggested the use of an instrument such as is used for plugging the posterior nares (Balloc's Sound), for carrying the cord round the part of the child to be divided, which was the one feature wanting to make it virtually, and to all intents and purposes, Heyerdahl's operation.

During a visit to Paris, in November, 1869, I procured Pajot's instrument, and determined to give it a trial on the first suitable occasion, but it was not till the 8th January last that I had an opportunity of doing so. On that day I was asked by Dr. Harley, of Baggot-street, to see a poor woman, of whom he gave the following history. She had had six children, and always with difficult labours, owing to a narrowing of the brim of the pelvis. When he first saw her she had been three days in labour, and the hand and arm had been lying outside the vulva for 24 hours. He had passed his hand into the uterus, laid hold of a foot and brought it down into the vagina, but by no effort, that he thought justifiable, could he get the child to turn. When I saw her I found the foot in the vagina, where Dr. Harley had left it, and seized it, and tried to turn, but also found the uterus was so tightly moulded to the child that I could not complete the version; and as the fœtal heart could not be heard, I determined to divide the neck. I placed the handle of Pajot's forceps on the neck without very much trouble, but failed to make the bullet roll, and had to pass my hand behind the neck till I met the instrument, when I drew down the bullet, and so got the cord into its place. I now passed a speculum as directed by Pajot, and proceeded to cut through the neck, but at the end of one minute and a half of the sawing motion the cord broke, and I feared the operation had failed. On examination, however, I found the greater part of the neck, including the spinal column divided, and I now tried to bring down the foot and effect version, which I did without any difficulty whatever, and the woman made a rapid and satisfactory recovery.

This is an example of the second class of cases in which version cannot be affected; those in which the hand can be introduced and the foot seized, but the body of the child cannot be turned. I think we may say these are the cases in which evisceration is most difficult and dangerous, and decapitation is most easily performed. Certainly, when I contrast the safety, ease, and rapidity with which the operation was performed, with the difficulty I have myself experienced and seen occur with several experienced operators in performing evisceration, I am most favourably impressed with the value of the operation of decapitation.

On reflecting on the case, it seems to me that the operation might be very much simplified and also rendered safer, and still without infringing Pajot's principle of not multiplying instruments. The cord might be very easily carried round the child's neck by means of an elastic catheter, mounted on a firm stylet, or, what would be better still, mounted on a uterine sound. The firmness and broad handle of the sound would enable us to direct more easily the point of the catheter. The catheter should be a good deal curved; it should form about the fourth of a circle, whose diameter would be three inches. The first step of the operation is to hook this on the child's neck; then the stylet *being held steadily* in its place, the catheter is to be run forward, when the *curve of the stylet* will cause it to pass round the neck till its point can be easily reached with the fingers, or may come into view. The cord may now be attached to the catheter, and drawn back with it, or it may have been previously passed through the catheter, and brought out at the eye of the instrument; so that it can be seized when the catheter comes within reach of the fingers. The ends of the cord are now drawn through a speculum, the catheter having been first removed, and the neck is divided as Pajot directs. If no speculum be at hand, the soft parts of the mother may, as Pajot suggests, be protected by the broad handles of two spoons introduced into the vagina, and used as retractors.

It will be seen that this suggestion differs from Heyerdahl's only in that the internal wire is held steady, and the outer tube is the part pushed forward. It is probable a soft elastic catheter would find its way more safely than the firm wire; but the chief object of the suggestion is that it does away with the necessity for a special instrument, which might not always be at hand when required.

The operation might be further improved by using an écraseur for dividing the neck, by which the accidental interruption of the operation by the breaking of the cord would be avoided, as well as the risk of injuring the soft parts of the mother by its friction. Once the cord is placed round the neck there would be no difficulty in attaching the chain of an écraseur to it, and drawing it into its place. The use of an écraseur invented by Dr. Ritchie for this purpose was, according to Dr. Barnes, mentioned at a discussion at a late meeting of the Edinburgh Obstetrical Society by Dr. Keiller, but I have not been able to meet with a report of this discussion.

As soon as the neck has been divided, the body may be easily extracted by drawing down the arm, and if the uterus act well it will very probably expel the head; but if not, it may be seized with the forceps or cephalotribe. In using the cephalotribe in this case it will not be necessary to perforate in the first instance, because when pressure is applied the brain will escape through the foramen magnum; and, in avoiding the perforator, we avoid one great source of danger and difficulty in this part of the operation. But it is very probable that the best way of completing the operation would be to aim at the production of the condition that arose in the case under consideration from the accidental breaking of the cord—that is, to stop as soon as the spinal column is divided, and to leave the head attached to the body by a portion of the soft tissues of the neck; then to deliver by the foot, and let the head be drawn down still attached to the body. It is obvious that the division of the spinal column would do away with the difficulty of turning, and the tissues attached to the head would afford us a ready means for its extraction.—*Dublin Quarterly Journal, May,* 1871, *p.* 383.

72.—TREATMENT OF HEMORRHAGE ARISING FROM RETENTION OF THE SECUNDINES AFTER ABORTION.

By Dr. J. G. Swayne, Physician-Accoucheur to the Bristol General Infirmary.

An abortion about the middle of utero-gestation may become one of the most troublesome, difficult, and dangerous cases which can fall to the lot of an accoucheur. The reason is, that at this time it is peculiarly liable to be complicated with retention of the secundines. During the first two months of pregnancy, the aborted ovum is usually cast off entire, and there is seldom any further trouble. During the last three or four months, the membranes generally are ruptured, and the fœtus and placenta expelled separately; but the latter is sufficiently bulky to stimulate the uterus to expel it; and, even if this should not happen, the uterus is large enough to admit of the introduction of the hand for the purpose of removing it. During the middle of pregnancy however, or rather, I should say, during the third, fourth, and fifth months, the ovum is very likely not to be expelled entire: the placenta and membranes are not sufficiently bulky to induce expulsive efforts of the uterus, and are, therefore, very liable to be left behind; and the uterus is not large enough to admit the hand for the purpose of extracting them. Under favourable circumstances, the placenta and membranes may be cast off with, or immediately after, the fœtus; but it is quite as usual, I think, to find that this is not the case, and that the secundines, if let alone, may be retained for an indefinite period. For instance, on making an examination, the accoucheur finds the uterus contracted in all its dimensions, and the os uteri tightly encircling the filiform funis, which will inevitably give way if any attempts are made to remove the placenta by its means. In fact, circular contraction of the fibres of the os uteri internum, or what is commonly called hour-glass contraction of the uterus, is a much more frequent occurrence after abortion than after labour at the full term, and is accompanied with similar dangers, although in a minor degree. As long as the placenta and membranes remain behind in the uterus, the patient is subject to attacks of hemorrhage, which may be very frequent and very profuse; or she is liable at a later period to septicæmia from the absorption of putrid matter caused by the decomposition of the retained secundines.

To prevent such untoward results, the obvious indication is, of course, to remove the exciting cause; but this is a matter of so much difficulty in practice, that obstetric authorities have for a long time been divided in opinion as to the advisability of attempting it. The older authorities—for instance, Denman, Davis, Dewees, Burns, Ingleby, and Blundell—are in favour of the expectant plan, and disapprove of manual attempts to extract the placenta. Denman, who always relied so implicitly on the *vis medicatrix naturæ*, is especially opposed to manual interference, and at the same time very much underrates the risk arising from retention of the secundines, for he goes so far as to say, "Much less mischief may be expected from the retention of a putrid placenta than from attempts to force it away by the medicines usually given, or by manual assistance." I have myself in many instances tried the expectant plan of treatment, and can by no means give a favourable report of its results. As long as the placenta remains in the uterus, the patient is liable to sudden and alarming attacks of hemorrhage; and so greatly am I impressed with the danger of this occurrence, that during the first three or four days after an abortion I would not on any account leave a patient with the placenta in the uterus, unless I had previously guarded against this accident by plugging the vagina. In all cases of miscarriage accompanied with much hemorrhage, the plug is a most valuable remedy, and may always be employed when the placenta cannot be extracted. It is a very safe remedy also, when certain precautions are taken. These are, to watch carefully that it does not occasion accumulations of blood, or retention of putrid matter, in the uterus. To fulfil the first indication, firm counterpressure should be made over the fundus uteri, so as to prevent the expansion of that organ. This is unnecessary during the first three months of pregnancy, as the uterus is too small to hold much blood; but during the fourth, and especially the fifth month, it becomes necessary, because the uterus is then capable of containing a considerable amount. I once attended a woman who miscarried during the fifth month, and who nearly died from internal hemorrhage into the uterus, although the vagina had been plugged, and there was not the slightest escape of blood externally. She had lost a large amount of blood previously, and the quantity which then escaped into the uterus was nearly enough to turn the scale against her. Her case so impressed the danger of internal hemorrhage on my mind, that I now never use the plug in the fourth or fifth months of pregnancy without applying over the fundus uteri a large cup-shaped piece of padded metal made for that purpose, and just adapted to the size of the organ. This is secured in its place by a strap and buckles, and over all an abdominal bandage. To fulfil the second indication—viz., to obviate the danger of collections of putrid matter in the uterus—the plug should be removed frequently, and reapplied if necessary. As a general rule, it is well not to let a plug remain *in situ* more than twelve hours. By far the most efficient method of plugging is to introduce into the os uteri a tent of compressed sponge before plugging the vagina. This not only prevents all escape of blood from the uterus, but most effectually dilates the cervix, and facilitates the extrusion of the placenta. The vagina should then be plugged; and for this purpose I generally prefer an old silk handkerchief, which has been previously torn into two or three long strips, and well oiled. It is much more convenient to introduce the plug through a full-sized tubular speculum. When the plug is pushed up by the fingers without using a speculum, the operation is much more tedious and painful. The plug should be removed after an interval of about twelve hours: and it is a good plan to administer a full dose of ergot about two hours previously. The result of this will probably be that, on removing the plug, the retained placenta will come away also; the stimulus of the plug and the ergot having induced expulsive action of the uterus. But yet, in many other cases, the action of ergot is very uncertain, and one's hopes are doomed to disappointment. If the placenta does not come away, plugging may be continued for two or three days, if there be much danger of hemorrhage, but if this is not the case, it is better not to persevere with it so long. When putrefaction of the secundines has come on, and the vaginal discharge has become very offensive, the plug should be taken away; and this is above all necessary if fever and constitutional irritation have set in. The plug, by confining putrid discharges within the uterus, renders the danger of absorption more imminent. To obviate this danger the vagina and even the uterus should be syringed out twice or three times a-day with warm water and antiseptic lotions, and this should be done quite as much with the view of detaching the placenta as of neutralising and washing away the putrid discharges. An ordinary Higginson's syringe with an elastic tube is the best instrument for washing out the vagina, and the nurse or other attendant may be directed to throw up about a pint of fluid each time. To syringe out the uterus, however, requires much more care and caution, as it is a proceeding by no means free from risk. I have seen very alarming symptoms, such as sudden acute abdominal pain, dyspnœa, and great prostration produced by injections into the uterus, especially when the fluid has been thrown up too forcibly and in too great quantity; and there is no doubt that in this way, not only some of the injection, but even some of the putrid uterine contents may be forced through the tubes into the abdominal cavity. On this account no more fluid should be injected than the uterus can easily contain; and the tube used for the purpose should not be so large as to prevent a free escape of the fluid by the side of it through the os uteri as soon as the uterine cavity is filled. A small India-rubber bottle, to which an elastic catheter has been adapted, will be much better for uterine injection than a Higginson's syringe. I need scarcely say that this operation should not be entrusted to a nurse. Besides antiseptic lotions, astringents and styptics may be injected in the manner just described whenever hemorrhage comes on after plugging has been discontinued. For this purpose an injection consisting of four ounces of the liq. ferri perchlor. id. fortior of the *British Pharmacopœia* with twelve ounces of water, as recommended by Dr. Barnes, will be found very serviceable. I am in the habit of using a lotion consisting of one part of Bird's styptic (a peroxide of sulphate of iron and alumina) with four parts of water, and I prefer it to any other. Besides these local means for checking hemorrhage cold may be applied, and ergot of rye administered, together with various astringents. The recumbent posture should be strictly enjoined.

It must be confessed, however, by all who have had much experience of these cases, that the expectant treatment is very unsatisfactory; and that, in spite of plugging, styptics, and other palliatives, the patient is never safe from hemorrhage, septicæmia, and other less dangerous consequences, so long as the *fons et origo mali* in the shape of the whole or any portion of the secundines is left behind in the uterus. She may suffer for weeks from severe hemorrhages, frequent rigors, constant fœtid vaginal discharges, hectic fever, night sweats, great prostration of strength, tenderness and swelling of the abdomen, and complete loss of appetite; and these symptoms may go on to a fatal termination unless the putrefying portions of retained placenta are either expelled naturally or removed by art. On this account most obstetric authorities in the present day have abandoned the expectant plan, and are in favour of active interference.

In 1861, Mr. Priestley communicated to the Obstetrical Society of London an excellent paper on "the Treatment of Cases of Abortion in which the Placenta and Membranes are retained." In the discussion which followed, the weight of opinion in the society was decidedly in favour of manual interference: Dr.

Priestley, Dr. Hall Davis, Dr. Tyler Smith, and Dr. Tanner, all strongly advocating this plan of treatment, in which opinion I myself fully concur.

The propriety of manual interference being decided in the affirmative, the next question will be as to the best method of effecting it. Most authors maintain that the safest means is by the hand alone, and condemn (without as I think, sufficient reason) the use of any kind of instrument for this purpose. As I remarked before, the uterus at this period of gestation will not admit the entire hand, and the most that we can do is to introduce the hand into the vagina so as to pass the fore and middle fingers into the uterus. Any attempts to extract a retained placenta, by two fingers, in the vagina only, will almost certainly be futile. Under the most favourable circumstances, the tips of the fingers may thus be able to touch the placenta just within the os uteri, but have little or no power to bring it down, and, in all probability, it will again and again elude their grasp. It becomes necessary, then, to introduce the whole hand into the vagina. But this is always a very painful operation at this period, even in a multipara, and in a primipara is accompanied also with considerable difficulty, and not free from danger. It is, therefore, always best to place the patient first under the influence of chloroform, as much for the purpose of relaxing the tissues as of abolishing pain. The *modus operandi* is thus described by Dr. Priestley: "Whilst the woman lies on her back, with the thighs flexed upon the abdomen, I have introduced one hand into the vagina, the other hand being placed over the fundus uteri, externally, to steady and depress it. If the os uteri was closed, it was gradually dilated by passing first the forefinger into the orifice, and then the second, with as little force as possible. With time and caution, the os uteri gives way to gentle pressure, and the introduction of the finger is in most cases not difficult. When the placenta is reached, it is separated in the ordinary way, and pushed downwards towards the palm of the hand, until it is partially or wholly beyond the os uteri. It is important, certainly, to ascertain that the placenta and membranes are entirely loosened from their attachments, and quite movable in the uterine cavity before the retraction of the hand, so that a second introduction of the fingers is not needed." Dr. Priestley further remarks: "In scarcely any case can it be necessary to introduce more of the hand into the uterus than the first two fingers—one may occasionally answer the purpose, but works at great disadvantage alone, both in separating and extracting the placenta. The two fingers together constitute the best form of forceps for seizing what it is intended to withdraw, and are the most serviceable in all respects." With respect to the time that ought to elapse after the birth of the fœtus, before the placenta should be extracted, Dr. Priestley is inclined to suggest that six hours may be fixed as an approximative limit, unless hemorrhage have occurred to any extent during the interval. In such a case their removal should be effected earlier.

As far as my own experience goes, I cannot bear out the statements which have been made as to the efficiency of two fingers, either in dilating the os uteri, or extracting the placenta and membranes; I have several times failed, especially when the placenta has been at all adherent, in effecting these objects. This I attribute to the small powers of abduction and adduction which the fingers possess, even under ordinary circumstances. It is surprising how little power there is of grasping anything between the distal phalanges of the two first fingers, compared to that which is possessed by the forefinger and thumb. To test the relative powers of each, I have tried what amount of weight I could raise by the forefinger and thumb only, used as a forceps, and by the two first fingers. I found that, while I could raise twenty-eight pounds by the former, I could not raise more than four pounds by the latter. The reason, no doubt, for this great difference in power is, that when anything is grasped between the points of the thumb and forefinger, it is held by means of the flexor muscles; whereas, when it is grasped between the points of the fore and middle finger, it is held solely by the adductor muscles, which are much weaker and act much less advantageously. To show how little power the two first fingers have of grasping anything between them, let any one place the handle of an ordinary dining-room *poker* between their two distal phalanges, and he will find it no easy matter to lift it off the ground. One can readily understand that these two fingers, enclosed in a contracting uterus, would form but a very inefficient forceps.

On this account I have endeavoured to supply the deficient powers of abduction and adduction possessed by the fingers by means of a modification of the ordinary forceps which is used for extraction of the ovum. The extremities of the instrument form an obtuse angle with the shanks, so as to correspond with the angle formed by the axis of the uterus with that of the vagina. The extremities are fenestrated, and resemble in shape the blades of an ordinary obstetric forceps in miniature, except that they are more hollowed on their inner surface, so as to fit with tolerable accuracy over the forefinger of the left hand when it is placed between them for the purpose of introduction. Attached to the handles is a screw, by means of which they can be powerfully separated when necessary. The instrument may thus be made a most efficient agent in dilating the os uteri.

The cases most adapted for the use of this forceps are those in the os uteri which will just admit the last phalanx of the index finger, and where the detracted placenta can just be felt at the os uteri internum, but is found to be retained in that position by the contraction of the circular fibres of the os uteri. The forefinger of the left hand enclosed between the blades of the forceps, but with the tip advanced a little beyond them, should be passed through the os uteri until it touches the placenta. The blades of the instrument should then be passed in a little beyond the finger, and separated very slowly by the screw, until the contraction of the circular fibres is gradually overcome. The placenta will then probably be extended until it can be grasped by the blades of the forceps, and if the cord is unbroken, this process may be facilitated by a little gentle traction upon it. In order to grasp the placenta, the screw must be turned in a reverse direction until the handles can be brought together.

Much has been said about the danger of passing instruments of this kind into the uterus beyond the guidance and protection of the fingers. I can readily understand that, with such an instrument as the wire crotchet of Dr. Dewees it would not be difficult to injure the inner surface of the uterus when groping about for a retained placenta; but I cannot see how it would be possible to grasp any portion of the uterus with such a forceps as I have described when used with ordinary care; and the blades are so smooth and rounded externally, that they would not be likely to inflict injury in any other way. For my own part, I should not hesitate to employ this forceps, even when the placenta is still adherent to the fundus uteri. The advantages it possesses are that it dilates the os, and grasps the secundines better than the fingers, and that it renders the introduction of the entire hand into the vagina unnecessary.—*British Medical Journal, August* 19, 1871, *p.* 201.

73.—ON A CASE OF PUERPERAL CONVULSIONS TREATED BY CHLOROFORM AND VERSION.

By Dr. J. Harris Ross.

I was sent for on Sunday, Dec. 5th, 1869, at 6 p.m., to see Mrs. C., aged 22 who had fallen from her chair in a fit. She was lying on her bed, and had a flushed face and bounding pulse; and expected daily to be delivered of her first child. I was informed that the fit was sudden, and came on whilst sitting at tea; that she struck her head against the table in falling; that she had complained of headache all day, but had been about her household duties. She had most violent paroxysms of convulsions, which came on about every ten minutes, leaving her during the intervals perfectly unconscious.

On making a vaginal examination, I found the os extremely high up, so as hardly to be reached with the finger; it was not at all dilated, though very flaccid and abounding with secretion. There was no "show." I at once sent for a medical friend of mine, and, after consultation, we decided to give chloroform, and try to evacuate *the uterus as quickly* as we could. After placing a cork between her teeth, my friend gave her chloroform, whilst I proceeded to dilate the uterus with my fingers. After gradual extension of the os for about half an hour, I was enabled to introduce my left hand into the uterus, and had no difficulty in turning, and bringing the legs beyond the vaginal opening. My friend then took my place, and speedily completed the delivery. After the removal of the placenta there was a considerable escape of blood; but this was easily controlled by external manipulation over the uterus. After a little artificial respiration, I was pleased to find the child was alive.

Our patient had but one attack of convulsions after the administration of the anæsthetic, and that was during a slight interval when we had removed the lint from her face. Half an hour after delivery, her pulse had diminished from 140 to 120, and she had no convulsions. We then left the house, and on again visiting her an hour afterwards she had had no return of the convulsions, but was still unconscious.

Dec. 6th. Seen three times during the day, in a semi-conscious condition, and when spoken to sharply replied, "Don't;" "Let me alone." Has neither eaten nor drunk anything since delivery; urine dribbles away involuntarily; has one hand constantly between her legs; pulse 126. She is very morose when offered nourishment, and on one occasion knocked a cup out of my hand.

7th. Took a cup of beef-tea freely during the night; cerebral symptoms and moroseness much the same as yesterday. I ordered clean linen to be applied, and the administration of beef-tea, eggs, milk, and brandy, &c. I saw her again in the afternoon, and found that none of my directions had been carried out, and that the nurses were both in a state of intoxication. I at once had the services of these dipsomaniacs supplanted by that of a temperate woman. In the evening I found the patient had taken nourishment liberally, had passed water at will in a vessel, and would answer a question when asked; still she was not quite conscious.

8th. Asked questions of the attendant during the night, who showed her the baby, and the patient laughed at the idea of its being her own. Talked quite rationally, and ate a chop for dinner.

From this date she gradually recovered her strength. She sat up for two hours on Dec. 15th, and was discharged quite well on Dec. 27th. She states that she remembers nothing from the morning of the 5th (when she had a severe headache) until that of the 8th. I am sorry I did not have an opportunity of examining the urine during the first two or three days, for I was about introducing the catheter on the occasion of her being first placed under chloroform, when the sphincter gave way and all the urine escaped, and for the next two or three days it kept dribbling away. She had no sign of lactation. On Jan. 16th, 1870, I met her in the street, carrying her baby. They both looked hearty and well.

This case is interesting at the present time owing to the diversity of opinion which exists as to whether we ought to bleed in these cases. On the one hand, we find Drs. Churchill, Johnson and Sinclair, Ramsbotham, Tyler Smith, and others, all strongly recommending bleeding. In fact one of them says: "Bleeding is our great reliance; the lancet is our sheet anchor; and blood may be taken to a large extent." On the other hand, we have the late Sir J. Simpson, Dr. Braun of Vienna, Dr. Barnes, Dr. Tanner, &c., all equally averse to bleeding. Dr. Tanner, in his last work on "Diseases of Pregnancy," says: "I have no hesitation in expressing a decided belief that bloodletting, as a general rule, without exerting any valuable effect on the symptoms, is calculated to produce irreparable mischief."

Since writing the above I have read in the last volume of "Guy's Hospital Reports" a paper by Dr. Phillips, in which he relates an interesting series of cases all treated without bleeding.—*Lancet, August* 12, 1871, *p.* 215.

74.—TWO CASES OF PUERPERAL CONVULSIONS, TREATED BY MANUAL DILATATION OF THE OS UTERI AND INSTRUMENTAL DELIVERY.

By Dr. E. Holland, Regent's Park, London.

In the course of the year 1869 it was my professional fortune to be called upon to treat two cases of severe puerperal convulsions in the duration of one week, and as there were certain peculiarities attached to each, some novelty in the treatment adopted, and in each instance a good recovery, I regard their brief announcement to be as desirable as it must be interesting.

Case 1.—A tall, full-fleshed, rather ruddy, and tolerably vigorous rural primipara, the subject of aortic regurgitation and some hypertrophy of the left ventricle, with habitual constipation and albumen in the urine, was suddenly seized with epileptic convulsions, about the middle of the seventh month of her pregnancy, after a few hours' premonition of hazy vision, headache, and incipient labour pains. The convulsions were very severe, recurred at intervals of ten, fifteen, or twenty minutes, and were followed by insensiblity and coma. The rectum was full, the os uteri massively thick, and open to the extent of a shilling, but the presentation could not be decided upon. The pulse was enduringly quickened, and forcible. An enema of gruel, containing an ounce of castor-oil and a flavouring of turpentine, was prepared with despatch, and administered after about a pint of blood had been drawn from the arm; the bladder was emptied by a catheter, the hot head had cold water applied to it, and the legs were wrapped in turpentine stupes. After the bleeding there was no fit for twenty-five minutes, and during this interval a piece of deal was placed between the teeth. The fits now recurred every ten or fifteen minutes with the pains, and were equally severe and prolonged, whilst the intermediate coma, if anything deepened, and left scarcely a shadow of hope that she would recover. An anxious hour or so still passed, during which the convulsions were decidedly severer and more prolonged, and something was to be done. I now determined to introduce my hand (a small one) into the vagina, and to dilate the os uteri (now but little altered) by introducing finger after finger, until I could work in the hand, turn, and extract. After about an hour's manipulation of the os, I succeeded in getting in the hand, and no sooner was it there than it came upon the fœtal feet, which were seized, and the extraction cautiously proceeded with, both during the pains and their intervals. As the shoulders came along, much caution was used to sweep the arms from the grasp of the uterus, and when all but the head was released from the uterus the latter became forcibly contracted upon it and round the fœtal neck. As the convulsions recurred, the uterine action was more powerful than I ever experienced; however, in the intervals of the seizures I contrived to insinutae my fingers and perforate the head behind the ear and then, by very protracted efforts, effected its delivery by the conjoint use of the blunt hook and the fingers of the right hand. As soon as the child was delivered the convulsions ceased, and never returned. A comatose state continued for some ten or twelve hours, but yielded to a large blister on the nape; and, excepting hazy vision and occasional headache, she made an uninterrupted and speedy recovery, with no indication of uterine injury, and, quickly becoming pregnant again, miscarried about the eighth month, without convulsions.

Case 2.—A short, slim, and pale multipara, aged 42, with mitral reflux and albuminous urine, was suddenly seized, about the seventh month of pregnancy, with epileptic convulsions, after having been reduced to a state of incipient collapse by a severe attack of diarrhœa. When visited, the head was cool and perspiring, the legs and feet cold, and the pulse quick and feeble. There was a convulsive seizure about every ten or fifteen minutes, and a semi-conscious state in the intervals. A vaginal examination revealed the fact that labour had commenced. Altogether the case was as disheartening as any case could well be; but, fortified by my recent experience of the preceding case, I ordered hot bottles to the feet, turpentine stupes to both legs, and brandy-and-egg mixture to be cautiously smeared over the interior of the mouth. I watched an hour or so, and matters got worse. I then resolved to dilate the os uteri, as in the preceding case, and deliver by perforation or otherwise as quickly as possible.. After an hour's digital manœuvring, I effected an entrance of the hand, and found a head presenting. I then immediately perforated; but after the escape of the brain, I had to remove the scull-cap piecemeal before I could get a firm hold. At last, however, after many a slip, I got a purchase on something, which turned out to be the clavicle, and by it I extracted a small female child of seven or eight months, drawing the uterus down, however, to the very outlet, where its retirement over the fœtus could be seen. The placenta followed; but, to mend matters, I now found there was a second fœtus in the uterus. However, as there had been no fit for a good twenty minutes, and no additional complication, I let well alone (having always derived advantage from that maxim), and watched. Presently there was a strong pain or two, and the second child, enveloped in its membranes, and with its placenta, was expelled *en masse*. The patient had no more fits, took an opiate, and made a very rapid and complete recovery.

Both these cases were severe. Both were treated on general principles as far as the bodily health was concerned. In both the fits recurred with increasing violence, and their intermediate coma deepened, despite every general measure. In both cases the os uteri was dilated from a very small size by the introduction of finger after finger; and in both instruments were freely used during an operation of several hours. In neither case was there any consideration to be given to the child, it being premature, and well known in the majority of such cases to be born dead. In neither case was there any fit after complete delivery of the child, or any indication of uterine injury; but, on the contrary, a rapid and good recovery. And, as each case formed the opposite end of an associated series, they will bear to be reflected upon, notwithstanding the sweeping condemnation some might feel disposed to inflict on every procedure that deviates from the groove of *ex cathedrâ* practice.—*Lancet, Sept.* 2, 1871, *p.* 322.

75.—ON THE MODE OF EXAMINING THE UTERUS IN CASES OF SUSPECTED DISEASE.

By Dr. Lombe Atthill, Fellow and Examiner in Midwifery, King and Queen's College of Physicians; and Obstetric Physician to the Adelaide Hospital, Dublin.

[Nothing is more important than care and skill in making an examination of the uterus. The trouble involved, and the natural repugnance of the patient, too frequently lead to incomplete, and consequently, misguiding examinations, resulting in loss of credit to the practitioner.]

In examining nearly every case of uterine or vaginal disease, we require the aid of both touch and light, to arrive at a correct conclusion as to the condition of the affected organs. To use the speculum without a previous examination by the finger and hand, is not only wrong, but fails to convey to us anything like an accurate knowledge of the case. Thus a patient suffers from leucorrhœa with pelvic pain and pains in the thigh. You make an examination with the speculum, and find the os uteri healthy, and may hastily come to the conclusion that no abnormal condition of the genital organs exist; and perhaps assure the patient that the womb is healthy. But nevertheless she is dissatisfied, for her sufferings continue, and by and by she consults another practitioner who detects the existence of a retroflected or anteflected uterus—a condition which an ocular inspection of the os uteri failed to detect. I could easily multiply examples, but let this one suffice to impress on you the necessity of making a manual examination before using the speculum. Now in speaking of a manual examination I mean more than a digital examination of the vagina. I include also under that term the investigation of the pelvic viscera through the abdominal walls, and if the symptoms seem to demand it, through the rectum also. I shall make a few remarks on the mode of conducting these investigations. First then as to *the ordinary digital* examination of the vagina or uterus. The patient is placed *on her left side*, the knees should be well drawn up, and the hips pushed out to the edge of the couch. These preliminaries effected, the index finger previously well oiled should be introduced slowly upward in the axis of the outlet of the pelvis; the tip of the finger being kept in contact with the posterior wall of the vagina. By adopting this course the finger reaches the posterior *cul de sac* of the vagina and by carrying it from this point round the cervix uteri, we are enabled at once to ascertain the condition of the lower segment of the uterus. Thus we learn whether it be moveable or fixed; whether it be of the normal size and shape; or if on the other hand elongated or hypertrophied. Then by drawing the finger down along its surface you reach the os uteri and discover its state, whether it be patulous with everted lips, or small and contracted. While thus engaged in investigating the condition of the uterus, you should not fail to attend to that of the vagina, and to satisfy yourself whether it be of the natural temperature and moisture, or unduly hot and dry. But there is more yet to be ascertained before you have gained all the information possible from a digital examination—the position of the uterus itself is to be made out, for the organ may be retroflected or anteflected, or possibly under certain circumstances completely retroverted.

As a rule you should not be able to feel the body of the unimpregnated uterus through the posterior *cul de sac* of the vagina. If therefore on sweeping the finger round the cervix you feel a firm globular mass above you, you can at once pronounce that the organ is in an abnormal condition. Then immediately follows the question, which you are called upon to solve, namely, on what does this enlargement depend? But I must defer the consideration of this question to a future lecture, for a mere digital examination though of importance, is frequently insufficient to enable us to decide this point; and in a large number of cases you must not remain content with it, or you will fall into grave errors. To make your examination complete you must have recourse to the use both of the speculum and of the uterine sound. I name them in the order in which as a rule they should be used. You see on the table there are three kinds of speculum; they are all of them admirable instruments, and, as I am about to explain to you, each possesses certain advantages which the other wants, and certain disadvantages which renders the use sometimes of one and sometimes of another preferable. It is, therefore, essential that you should be acquainted with the respective merits of each. There are no doubt numerous other kinds, but for ordinary purposes these are sufficient, and of these for general use I without hesitation recommend the one known as Fergusson's. It is as you are aware, a glass cylinder silvered externally. This again is protected by a layer of gutta-percha, which answers the double purpose of affording a very smooth surface, and serving as a protection to the vagina, should the glass by any mischance crack or break. Through a full-sized one of these speculums you can see the parts very distinctly, and it also possesses this great advantage, that it is uninjured by the action of acid, a class of remedial agents which are frequently used in the treatment of uterine disease. It is not however so easily introduced as either of the other speculums which I exhibit. If therefore the vagina be narrow or if much inflammation be present, the attempt to use a full-sized one will give so much pain that you will have to desist, and should you with the view of avoiding this, have recourse to a smaller one, you will find much difficulty in bringing the os into view; and even when you succeed in doing so, the position of the cervix exposed to view will be of such limited extent as often to afford but little information. Still the number of cases to which it is inapplicable will prove to be comparatively few. When from the narrowness of the orifice of the vagina, or from the amount of inflammation present you find Fergusson's speculum to be unsuitable, I recommend you to make use of a plated bi-valve. It is very easily introduced, but it does not reflect the light nearly so well as the glass one does, and moreover the lateral folds of the vagina fall to a considerable degree into the space between the blades when they are expanded, and intercept your view. The duck-bill speculum affords you one advantage which neither of the other possesses—namely, that it permits you to see the os uteri, and at the same time to touch it, a matter of the greatest importance in many cases. We therefore use it when introducing sea-tangle or sponge tents into the cervix uteri or when having withdrawn these we proceed to examine the condition of, or to make applications to, the canal of the cervix or body of the uterus, and also in the case of all operations about the vagina or uterus. Its disadvantages are that the forcible drawing back of the perineum, which is necessary to permit the os uteri to be seen, causes pain; while if the instrument be not held very steady the os slips out of view. Next, that an assistant is absolutely necessary to take charge of it, and thirdly that difficulty is often experienced in keeping the anterior wall of the vagina from intercepting the view unless, indeed you seize the os with a hook or vulsellum—the reason for, and the mode of doing which, I shall on a future occasion explain. I shall now give you a few directions as to the mode of introducing the speculum, for if you use the instrument in a bungling unhandy way not only will you give your patient much unnecessary pain, but you will also most likely leave an unfavourable impression as to your skill on her mind, and I therefore feel that I am not wasting time in dwelling on these minutiæ. First then you should dip your speculum into warm water to bring it up to the temperature of the body, and oil it; then, your patient lying on the left side with the hips well out, you should with the index and middle finger of the left hand raise and draw up the right labium and nympha, while with the thumb and index finger of the right hand you hold the speculum, and bring its point to the orifice of the vagina. You should at the same time, with the middle finger of that hand, depress the soft parts on the left side; for if this be not done, and if the labia or nymphæ be turned in before the edge of the speculum you will cause your patient much unnecessary pain, which a little care on your part would have obviated.

When once the point of the speculum has fairly entered within the vagina, its further introduction is a matter of no difficulty; but still it is very possible for a person inexperienced in its use to fail in bringing the os uteri into view, and therefore you should be careful to keep the point of the instrument pressed well back against the posterior wall of the vagina, for the os uteri should look downward and backward, and by keeping the point of the instrument in the direction I have indicated the os should without difficulty come into view. If this be not the case, the speculum should be withdrawn a little way and its direction slightly altered, when the desired object will most likely be attained. The foregoing directions hold equally good, whether you use Fergusson's or the expanding speculum, for though the latter, on account of its shape, is introduced with greater facility, yet it is not easier with it to bring the os into view; indeed the reverse is the case.

The duck-bill speculum requires special directions for its use. The following are those given by the inventor, Dr. Marion Sims, and should be carefully attended to whenever this speculum is used—"The thighs are flexed at right angles with the pelvis, the patient lying in a semi-prone position on her left side; her left hand being drawn backwards under her, and kept in that position; the chest rotated forward, bringing the sternum very nearly in contact with the table or couch, the head resting on the parietal bone; the head must not be flexed on the sternum nor the right shoulder elevated; the patient is thus rolled over on the front, making it a left lateral semi-prone position. The nurse or assistant at her back pulls up the right side of the nates with the left hand, while the surgeon introduces the speculum, elevates the perineum, and gives the instrument into the hand of the assistant, who holds it firmly in the desired position." These directions are admirable, and should be strictly attended to.

When with either speculum you have exposed the os uteri, you are able to judge of its state. You see first of all what may be the condition of the lips; if they are covered with healthy mucous membrane, and present the normal light mother-o' pearl coloured appearance, or whether they be congested, abrased, or in a state of granular ulceration and bleeding on the slightest touch; you see also whether the os be a small opening, free from discharge, or whether it be patulous, and plugged with a string of thick, glairy mucus, the sure indication of an unhealthy condition of the cervical canal. Then while withdrawing your speculum, you have an opportunity of satisfying yourself as to the condition of the vaginal mucous membrane; thus by touch and sight you are enabled to pronounce with positive certainty as to the state of the os, of the lower segment of the cervix uteri, and of the vagina; but should you stop here, you will in many cases have failed in your duty. Many a sufferer has been told after having submitted to such an examination that the womb was perfectly healthy, because the os and cervix appeared to be free from disease, and has consequently been looked upon as a complaining hypochondriac by her friends; while in reality she was a suffering invalid—the physician having failed to detect the actual ailment, either because he omitted to carry his investigation further, or because he was ignorant how to do so. For myself I lay down the following rule, which I advise you to follow, in the investigation of all cases of uterine disease which come under your observation:—1st. To make a digital examination of the vagina and cervix uteri; 2nd. If that fails to satisfy me as to the cause of the patient's suffering, then to use the speculum; and 3rd. If still in doubt, to introduce the uterine sound, unless its use be clearly contra-indicated. You are aware that the sound is an instrument of recent invention; but even so it is surprising how little it is used, and how few appreciate its merits. I look on it as at once one of the most useful and at the same time, if carefully and judiciously handled, safest of obstetric instruments. In my own practice I am indebted to it for most important information, which could have been obtained by no other means, and this too without having ever known it to produce the most trifling injury. Doubtless I am aware that if roughly and unskilfully handled, or used in an improper case, the most serious consequences may follow its introduction; but the same may be said of the catheter, or indeed of any other instrument requiring skill in its use. I again repeat that, if carefully used and skilfully handled, it is a harmless instrument, and may be used as safely or as freely as a catheter. Before explaining to you the mode of introducing it, I wish to call your attention to the instrument itself. It is as you see a metallic staff, not unlike the sound used by surgeons in the male. The best are made of copper plated, the advantage of which is that you are able to bend it at pleasure, a matter of no small importance, as you are frequently obliged to alter the curve when flexions of the uterus exist. At a distance of two and a-quarter inches from the extremity of the instrument there is a little knob, which marks the depth to which it should usually penetrate into the uterine cavity; and at this point you observe the instrument is curved, so that it may correspond with the axis of the uterine cavity. The entire length of the instrument is marked at intervals of an inch by notches, which enable you at once to decide to what depth the instrument has penetrated, for when withdrawing it you keep the point of your finger on the notch nearest to the os, and with the aid of the figures marked on the handle see at a glance what the depth of the uterine cavity is. It is not a matter of any great difficulty to introduce the sound into the cavity of the uterus; still it requires tact and practice, just as the use of the catheter does. The following directions will aid you in acquiring the requisite skill. I recommend you to introduce the index finger of the right hand into the vagina, and to keep the tip in close contact with the os uteri, then to guide the point of the sound held in the left hand up to the os, slipping it along the inner surface of this finger, the concavity of the instrument being turned towards the rectum. A little manipulation and gentle pressure will now make it enter the canal of the cervix. This being fairly accomplished, a fact you can always be sure of because your finger is still in contact with the os, you are to rotate the handle of the sound, a manœuvre exactly similar to that practised by surgeons when introducing the catheter in the male, and termed "tour de maitre." This has the effect of changing the direction of the point of the instrument, which will now look upwards in the direction of the axis of the uterus; steady but very gentle pressure should now be made, and the point will in general pass on without difficulty till it reach the os internum, and here some slight difficulty is generally met with. This if it occurs should be overcome by gentle continuous pressure; force must not on any account be used, lest injury be done to the uterine walls. As the point of the instrument passes through the os internum, the patient nearly always complains of pain and sometimes of nausea; but this goes off in a few minutes, though I have met with instances of the pain lasting for several hours, and I have on one or two occasions known a patient to feel faint; this too never lasted for more than a few moments, and was never sufficiently severe to prevent my finishing the examination. *In some instances* an obstruction to the introduction of the instrument is met with low down in the cervical canal. This is not due to any contraction, but to the point of the sound becoming caught in a fold of the mucous membrane which in this portion of the intra-uterine canal, is not smooth but plaited. Should this occur you must withdraw the point a little, and, altering its direction somewhat, again press it onward. This difficulty is more likely to occur when the os uteri is patulous, and the cervical canal relaxed from the effects of disease, than when it is in a healthy condition; but a little patience and careful manipulation will always overcome these obstructions. I have dwelt at some length on the mode of introducing the sound, because the difficulties of the operation have been much exaggerated, and I am satisfied that these difficulties are mainly due to want of skill on the part of the operator. The method of using the sound which I have described, is that which I always adopt; but there are other modes doubtless equally as good. Thus Dr. Graily Hewitt following the plan recommended by Sir J. Simpson, introduces the index finger of the left hand, guiding the sound along it up to the os uteri. While Dr. West recommends introducing two fingers of that hand for the purpose, the instrument being held in the right hand. But whichever method you adopt, you will speedily with a little practice become adepts, only remember never to use force; better far that you should never introduce the instrument than you should run the risk of injuring the uterus, and perhaps cause a fatal result, in doing by force what should only be accomplished by tact. But you will frequently meet with cases in which the use of the sound is entirely forbidden. Thus, if there be any possibility of pregnancy existing, it would be most improper to introduce it, and you should wait till you are satisfied on this point. In cases of cancer too, and as a rule, during an attack of any form of acute inflammation, your own judgment will warn you against it. But with such exceptions as these I can confidently recommend it to you as a safe and useful instrument—so high is my opinion of the uterine sound, that I make it a rule to introduce it in all doubtfu cases unless its use is contraindicated by the possible existence of pregnancy, or by some equally valid cause, and I am satisfied that this will at no distant time be recognised by all well-informed obstetric practitioners as the established rule. Now as to the information to be obtained from its use. We learn three things which it would be impossible to ascertain by any other means. Firstly, we ascertain with positive certainty what the depth of the uterus is. If the sound pass beyond the nodule, at the curve of the instrument, we know that the cavity is unduly elongated, and we can measure accurately the extent to which it is elongated. Secondly, we ascertain the position of the uterus, and determine whether it be in its normal position, or flexed anteriorly or posteriorly. Lastly we learn whether the entire organ be fixed or moveable—a matter of the greates moment when we come to decide on the all important question of the nature of some abdominal tumour, the sound, and the sound alone enabling us to decide whether the uterus is engaged in that tumour or not.

But our means of obtaining information are not yet exhausted. Our examination hitherto has been carried on through the vagina. We have ascertained what the condition of the os uteri is. We have measured the depth of the intra-uterine canal with our sound. We are satisfied that the uterus has retained its natural position, or is displaced. But we know nothing of the condition of the external or peritoneal surface of that organ. A fibrous tumour for instance, of any conceivable size may be developed from any portion of the uterine wall, and yet the examination I have hitherto described may fail to detect it. Never omit then in all doubtful cases, to pass the hand over the abdomen, and by the aid of both hands, to satisfy yourself as to the shape and size of the uterus. This method, termed by Dr. Marion Sims the bi-manual method, often affords valuable information. To carry it out pressure is made with the left hand over the pubes while the index finger of the right is kept in contact with the cervix uteri, the patient lying on her back should be made to expire deeply, and at this moment the fingers of the left hand should be pressed firmly down into the pelvis, immediately over the pubes while the index finger presses the uterus upward from the vagina. It will thus, to use Dr. Sims's words, "be easy to measure the size and shape of the body of the womb, for it will be held firmly between the fingers *of the two hands*, and its outline and irregularities will be ascertained with as much nicety as if it were outside the body." In thin subjects the results here enumerated as attainable can be obtained; but in fat or very muscular women we sometimes fail in our efforts to feel the uterus at all through the abdominal parietes. Still even with these exceptions, the bi-manual method of examination is often of great value.

I have already told you that in order to make an accurate diagnosis it is generally necessary to make a digital examination of the condition of the uterus and vagina, and to use both the speculum and the uterine sound. But in many cases, the two latter modes are not only unnecessary, but positively forbidden. Thus, if on introducing the finger into the vagina you detect cancer of the os uteri, the introduction of the speculum becomes unnecessary, and may be injurious, while the use of the sound is altogether prohibited; or, if on using the speculum we find the os and cervix uteri to be in a state of ulceration, the symptoms the patient is suffering from will probably be accounted for, and the introduction of the sound into the uterine cavity uncalled for, and therefore to be avoided. So your examination in all cases is to be progressive, the finger always being used in the first instance. Any departure from this course, I deprecate strongly.—*Medical Press and Circular, June* 14, 1871, *p.* 501.

76.—ON DILATATION OF THE OS AND CERVIX UTERI.

By Dr. Lombe Atthill, Fellow and Examiner in Midwifery, King and Queen's College of Physicians, Dublin.

Whenever you meet with a case of menorrhagia *in an otherwise unhealthy woman* which a careful vaginal examination proves not to depend on ulceration of the os and cervix uteri, on an extra-uterine polypus, on cancer or such evident cause, to dilate the cervix and os internum with the view of determining what the condition of the interior of the womb may be, I hold to be your manifest duty.

I cannot refrain from quoting the judicious remarks of Dr. Tanner with reference to this subject. He says, speaking of menorrhagia (vol. ii., p. 301),—"When a woman suffers from repeated attacks of uterine hemorrhage which can only be partially or temporarily relieved by rest, nourishing food and proper astrigents, we may be sure that there is some organic disease of the ovaries or uterus; and though the cervix and body feel healthy to the touch, we can be certain that the bleeding is due to some actual disease, that it is not functional." And further on after enumerating what these causes may be he adds, "There is only one plan of treatment which can be adopted with a reasonable hope of success, and that is to dilate the os and cervix thoroughly so as to permit the removal of the source of evil." I fully endorse these observations. There are two methods still practised of accomplishing this object the one with sponge tents, the other by means of sea tangle. The former can be made of any required size, it is merely necessary to cut a fine clean sponge to pieces conical in shape and of various size and lengths, for you should always be provided with several tents of different sizes before commencing the process of dilatation. You should then wrap each piece as tightly as possible with fine twine commencing at the narrow extremity and winding it on till it reach the thick end. The pieces of sponge should then be immersed in a strong solution of gum arabic, left in it till thoroughly saturated, and then hung up to dry slowly. Before these are used the surface should after the removal of the twine be rubbed smooth. A small-sized tent is to be first inserted, a larger one being introduced on its removal after the lapse of from six to twelve hours. I have entirely given up the use of sponge tents myself: they are troublesome to prepare, give rise to a very fœtid discharge, and are further objectionable because the mucous membrane lining the cervix sinks into the cells of the sponge which is consequently lacerated as the tent is withdrawn, and thereby the risk of inflammation occurring is greatly increased; besides sponge tents from their conical shape necessarily dilate the os externum far beyond what is required before the os internum is opened even to a moderate extent. In fine, sponge tents should never be used if sea tangle can be obtained. Tents made of this substance technically called *laminaria digitata* have been in use for some years for the purpose of dilating the cervix. The method first adopted was to introduce one which after the lapse of some hours was withdrawn and another of greater calibre introduced in its place, the process being repeated till the os interum was sufficiently dilated. This process was necessarily very tedious and besides objectionable in other points of view. It is now given up and a modification of it introduced by Dr. Kidd of this city adopted in its place. Dr. Kidd's method possesses these three great advantages—that it is comparatively rapid, that it is cleanly, and lastly and most important of all that it dilates the canal equally throughout its whole length, except in some cases of rigidity of the os internum to which I shall allude presently.

Having decided to dilate the cervix the first step is to expose the os uteri by means of the duck-bill speculum, next to seize the anterior lip with a small hook and with it to draw down and steady the uterus. You should previously measure the depth of the uterus and have ready several pieces of sea tangle bougies, each piece being at least the length of the uterine cavity. These you now proceed to introduce, the main difficulty is nearly always with the first, and this difficulty is greatly increased if the uterus be retroflected. The short lengths not being so easily manipulated as longer ones I sometimes when difficulty occurs take an entire bougie and pass it through the os internum as I would the sound. I then slip pieces of the proper length in beside it, for when we have inserted one piece it straightens the uterus and serves as a guide to the others. When several pieces have been introduced you can withdraw the long one, or if before passing it you nick it round at a point corresponding with the length of the othe pieces you may be able to break it there, and so avoid the trouble of having to substitute another length in its place. The number of pieces you should inser varies in each case. If the patient have never been pregnant and the cervix rigid you will not be able to get in more than three or four, but if she have borne children or if the cervix be relaxed you may succeed in introducing double that number, or even more without difficulty.

If a small number only have been introduced, it is better to withdraw them after the lapse of nine or ten hours and introduce a larger number; but if seven or eight pieces have been inserted they may be left for twenty-four hours before any further steps be taken. The sea tangle rapidly absorbs moisture from the vagina and uterus, and swells, and by swelling forcibly dilates the cervix. This of course causes pain which however is seldom very severe and generally passes off after a few hours. If it continue I usually direct a morphia suppository to be introduced into the rectum, or 20 grains of the hydrate of chloral to be administered at bed-time. Dr. Graily Hewitt who still advocates the use of the sponge tents in preference to the sea tangle, states, as an objection to the latter that it is liable to slip out. This certainly is true if you use the short tents which are sold in boxes, but if you use pieces of the bougie of the length already specified and take care that they pass up to the fundus, there is very little chance of them being expelled; on the contrary I have on two or three occasions experienced some difficulty in removing them. This has been the case when the os internum was so rigid, that it prevented the sea tangle expanding as freely at that point as it did in the cavity of the uterus and in the cervical canal; and the pieces of tangle being thus constricted in the middle, it was necessary to press the index finger of the left hand firmly against the lip of the os uteri, while with a pair of long forceps, held in the right hand, one piece is seized and slowly extracted. These are the cases in which, as just mentioned, the whole extent of the canal is not equally dilated, and then fresh pieces of the tangle must be introduced, and time given to allow of them to expand, before proceeding to explore the interior of the uterus. You will, however, from time to time meet with cases in which, although the sea tangle has expanded to its fullest extent, still, from the size of the tumour or some other cause, the os internum is not as large as you would desire. Under such circumstances, I usually complete the process by the introduction of one of Dr. Barnes's dilators. These are India-rubber bags, of a somewhat hour-glass, or rather fiddle shape. They are made of three different sizes.

One end terminates in a long slender tube, the extremity of which is furnished with a stop-cock. The dilator is introduced in a flaccid state into the uterus on the point of a staff or sound, the terminal bulging part being carried through the os internum, and air or water being then gradually forced into the dilator through the long tube just alluded to it is left for an hour or two, and by that time has generally distended the canal to a considerable extent. The peculiar shape of the dilator prevents it, when once it has been distended, from slipping out of the uterus. Dr. Barnes originally introduced these bags into practice for the purpose of dilating the os uteri in cases in which it was desirable to induce premature labour, a purpose which they often serve admirably, but their use is now further extended, and we use them occasionally for the purpose of completing the dilatation of the cervix in the unimpregnated uterus.

You have had frequent opportunities of seeing the process I have described carried out—and must have noticed the entire absence of unpleasant symptoms, after a proceeding so apparently severe as the forcible dilatation of the cervix uteri—I have, therefore, no hesitation in recommending you to adopt this course in your future practice, as being one which you have seen productive of such good results in this hospital.

I have now explained to you the mode by which dilatation of the cervix is to be accomplished. It remains for me to direct your attention to the way in which you are to proceed, when having withdrawn your sea tangle or sponge tents, you desire to clear up the doubt which exists, and satisfy yourself as to the cause of the menorrhagia. Attempts have been made to examine the interior of the uterus by means of the endoscope; but as yet with negative results, the hemorrhage which in these cases is always present to at least some extent, and the mucous discharge which is continually being poured out, prevent anything being distinctly seen. We are therefore obliged to rely on the sense of touch alone, and must therefore pass the index finger fairly through the os internum till the tip reaches the very fundus. To accomplish this by no means easy matter, it is necessary, in the first instance, to draw down and fix the womb, this you effect by seizing the anterior lip of the os uteri with a vulsellum, which you intrust to an assistant to hold, while the fundus should be at the same time pressed down by your left hand, or, better still, by another assistant, the finger well oiled is now introduced slowly through the os internum, and swept round the entire cavity of the uterus. You will thus detect the existence of a polypus, or a tumour, no matter how small, should either be present, while the educated finger will recognize the rough uneven feel which the mucous membrane, if in an unhealthy granular condition, conveys to the touch.—*Medical Press and Circular, July* 26, 1871, *p.* 72.

77.—ON MECHANICAL DYSMENORRHŒA.

By Dr. Lombe Atthill, Fellow and Examiner in Midwifery, King and Queen's College of Physicians, Dublin.

[There are three varieties of mechanical dysmenorrhœa. Firstly, that in which the cervical canal is so flexed as to obstruct the escape of the menstrual discharge; secondly, that in which inflammation or congestion of the lining membrane exists to such an extent as to cause temporary closure of the canal, or of the os internum; and thirdly, that in which there is some congenital narrowing of the os internum or cervical canal.]

Painful or difficult menstruation is frequently met with in women in whom the uterus is flexed; but though flexions of the uterus may and certainly do sometimes interfere with the exit of the menstrual flow, they certainly seldom do so unless the flexion be complicated by the existence of chronic inflammation, or the presence of a fibroid. In such case we should certainly endeavour to relieve the flexion, and see if by replacing the fundus in its normal position, and supporting it there by a pessary, we can relieve the patient, before having recourse to surgical means, which are less suitable in this than in any of the other forms of mechanical dysmenorrhœa. I have already so fully explained my views as to the chief cause of the dysmenorrhœa in cases of inflammatory swelling of the lining membrane of the uterus, that I have but to repeat that though not in my opinion of very frequent occurrence, cases are met with in which the os internum or some portion of the cervical canal becomes so narrowed in consequence of the tumefaction of the parts as to present a mechanical impediment to the discharge of the menses; in such cases I have no hesitation in having recourse to surgical treatment with the view of procuring relief; indeed it is obvious that an operation which divides the cervix so freely as does that introduced by Sir James Simpson must be calculated to give permanent relief to the congested organ. I only say again that the operation should not be had recourse to till other means have failed, including the dilatation of the cervix by means of sea tangle tents. I may here take the opportunity of saying once for all that I unhesitatingly condemn the use of any of the metal instruments which have been suggested for the purpose of dilating the cervix; their use is attended with danger, as they act too rapidly and sometimes rupture the uterine fibres, several cases of severe inflammation and even of death are on record as following their use, while the sea tangle is perfectly harmless. A contracted os, looking almost like a pin hole and leading up to a narrow cervix uteri, is not unfrequently seen; this condition is almost invariably associated with sterility, and very often with dysmenorrhœa also. You saw last week a very good example of this in the case of the young woman who sought relief for the latter affection Menstruation is with her both painful and scanty, the os uteri so small as hardly to admit the point of a probe; there is no doubt the cervical canal is in this case unduly contracted. I think such cases as hers are fair subjects for operation, for no other treatment will be productive of permanent benefit, but beware of holding out hopes to your patient that, by submitting to the operation sh will gain more than relief from the suffering caused by the dysmenorrhœa. Th operation has been proposed and performed for the relief of sterility. As far a my experience goes it has resulted in failure. In other words it is in my opinion a legitimate proceeding, if performed with the view of curing cases of dysmenorrhœa, in which other treatment has failed, or is inapplicable, but that it is not warranted in cases of sterility, because the narrow os and contracted cervical canal are not the cause of the sterility, but merely an index of some congenital condition or defect in the uterus itself which hinders conception; what that defective condition may be we do not at present know.

But the patient I have first alluded to is averse to undergoing any operation, and I have therefore introduced a slender and short stemmed galvanic pessary. She has worn it for a fortnight and now tells you that she feels easier. You saw that I had some difficulty in introducing it mainly because the uterus is slightly ante-flected. I had accordingly to expose the os with the duck-bill speculum, then to seize and draw down the cervix with a fine hook, and while the womb was thus fixed to slip in the stem of the pessary. You must always adopt this method when difficulty occurs in the introduction of these instruments. I have known much good to result in such cases as the foregoing from this simple treatment, it is at least worth trying before advising that an operation should be performed.

Now with respect to the operation itself, we are indebted for its introduction to Sir J. Simpson, who for a time practised it very extensively, though I believe that before his death his views on this point were considerably modified, and that he did not perform it nearly so frequently as he had done at an earlier period of his career. His method of performing the operation was by passing an instrument termed a bistourie cachée through the canal of the cervix and within the os internum. It contained but one blade, which, when the instrument had penetrated to the requisite depth was made to protrude, the extent of the protrusion being regulated by a screw; the incision commenced at the os internum, and as the instrument was withdrawn it incised gradually and more deeply the substance of the cervix, till it divided the vaginal portion quite through; the instrument had then to be turned, reintroduced, and the other side divided in like manner; this re-introduction is very objectionable, and consequently various knives have been invented with the view of obviating it. Those pro*posed by Dr. Savage* and Dr. Greenhalgh are both good instruments. I give the preference however to the former. It is furnished with two blades, the cutting edge of each being directed outwards, but as the back of each blade when the instrument is closed projects beyond the cutting edge of its fellow which it thus overlaps, its introduction into the cervix can be safely effected, but it is generally necessary to dilate the cervical canal before this step can be effected. This indeed is generally necessary no matter what instrument is used, two pieces of sea-tangle will however open the canal sufficiently for the purpose; you should then, having exposed the os by means of the duck-bill speculum, proceed to introduce the knife slowly, taking care that it does not pass unnecessarily far into the uterus, the blades should then be expanded slowly and only to a very limited extent at first, for if this precaution be neglected you will divide the os internum too deeply, a proceeding which may cause alarming hemorrhage, and is nearly certain to be followed subsequently by such excessive eversion of the lips of the womb as to leave the neck patulous and gaping to an extensive degree; this condition exists in a patient at present under my care who was operated on more than ten years ago. When the blades are clear of the os internum they are to be expanded more widely, but I think it safer not to divide the vaginal portion of the cervix with them, but to complete this part of the operation subsequently by means of a pair of curved scissors. Dr. Marion Sims varies the operation by dividing first one and then the other side of the vaginal portion of cervix with a pair of bent narrow-bladed scissors, he then passes a narrow-bladed knife through the os internum and cuts from within outwards. The operation of dividing the cervix uteri is not devoid of danger; it has, though not in my practice, been followed by fatal results. I have known very alarming hemorrhage to occur, both at the time and also some hours subsequently. You should therefore be always prepared for this contingency, and be provided with a solution of the perchloride of iron in glycerine; with this you should invariably brush over the divided surface, and should hemorrhage occur, insert a pledget of cotton saturated with it into the cervix, and then plug the vagina. The operation itself seldoms causes pain, and if the woman be healthy the chance of inflammation following is not great; still extreme care should be taken to guard against such occurring, and she should be kept in bed for several days. There is often a great tendency in the incisions to unite. To prevent this Dr. Coglan has suggested the insertion of a thin roll of lead; this answers the purpose very well. It is sometimes necessary to introduce and leave in the cervix an expanding spring tent, but not unfrequently these precautions may be dispensed with. Dr. Graily Hewitt recommends with the view of preventing contraction, and at the same time of keeping the canal straight, that the patient wear for some time subsequently an ebony stem pessary, a proceeding which in many cases would doubtless be useful.—*Medical Press and Circular, July 5,* 1871, *p.* 3.

78.—OBSERVATIONS ON THE TREATMENT OF SOME FORMS OF MENORRHAGIA.

By Dr. Lombe Atthill, Fellow and Examiner in Midwifery, King and Queen's College of Physicians, and Obstetric Physician to the Adelaide Hospital, Dublin.

[The subject of the etiology and treatment of the various cases of menorrhagia is a most extensive one. In the present paper Dr. Atthill confines himself to the consideration of the treatment most suitable to cases of menorrhagia when occurring in connexion with, or dependent on, subinvolution of the uterus, on granular ulceration of the cervix, or on an unhealthy condition of the mucous membrane lining the body of the uterus.]

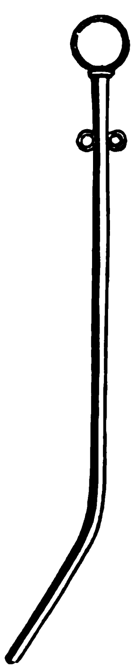
Defective involution of the uterus after labour or abortion occupies a prominent place among the causes giving rise to excessive menstruation. That this should be the case is but natural; for not only is there, when subinvolution exists, an undue amount of blood present in the organ, but also the relaxed condition of the uterine tissue favours its exudation, and, therefore, when the periodic determination of blood to the uterus takes place, as it occurs at each menstrual period, the moderate flow which should relieve that congestion become a profuse discharge, and often an exhausting drain. But the mischief resultin from subinvolution does not end here, for that abnormal state of the uteru predisposes to that unhealthy condition known as "granular ulceration" of th os and cervix uteri: a condition in which the mucous membrane of the cana of the cervix is hypertrophied, becomes exceedingly vascular, and is ofte everted to a considerable extent: a condition which increases the previousl existing tendency to hemorrhage. Thus, in not a few cases, do we find th two causes present in the same patient. The following case affords a well marked instance of this.

Mrs. F., aged 48, the mother of twelve children, presented herself among th extern patients of the Adelaide Hospital nearly a year ago. She stated tha ever since the date of her last confinement, five years since, menstruation ha gradually become more profuse, the flow continuing for a longer time than usual the intervals between the periods being correspondingly shortened. During th intervals she suffered much from profuse leucorrhœa, and, as a result of thi nearly-incessant drain, became greatly debilitated. On examining the uterus the sound passed to the depth of three inches and a half. The os uteri was patulous; there was extensive ulceration of the os uteri; and, on separating th lips, the mucous membrane lining the cervical canal was seen to be thickene and highly vascular. This was a case requiring energetic treatment, which I di not dare to attempt so long as she continued to follow her ordinary occupations and, for a time, she refused to come into hospital. But, finally, her conditio becoming much worse, she consented to do so. My first step after her admission was to introduce several lengths of sea-tangle bougies through the os internum, with a view of being able to explore the cavity of the uterus, and to mak the necessary applications to the entire extent of the cervix uteri. This proceeding having enabled me to decide that there was not any polypus or fibrou tumour in the uterus, I cauterised the entire of the inner surface of the uteru freely with the strong nitric acid; this did not cause any pain. She was, however, as a precaution, kept in bed for three or four days. On examining he after the lapse of a week, the condition of the ulceration which existed round the lips of the os was found to have improved considerably, and she was discharged in a short time perfectly cured.

In the foregoing case, subinvolution was manifestly the primary cause of the menorrhagia, the ulceration being altogether secondary. But, in many cases, subinvolution exists alone, or on the other hand, ulceration may exist alone, either condition being fully sufficient to give origin to severe menorrhagia. As an instance of the former, the following serves as an example. F. L., aged 24, married about a year, was a delicate young woman of lymphatic temperament. Menstruation had always been profuse, especially if she took walking exercise, or exerted herself during the flow. She became pregnant after the occurrence of the second menstrual period following her marriage, but, having imprudently taken a long and fatiguing walk, she aborted at about the eighth week; the two subsequent menstrual periods were so profuse as to reduce her to a state of extreme debility. Ergot, gallic acid, &c., failed to do good. On examining her after the termination of these periods, the uterus proved to be considerably elongated, the sound passing to the depth of three inches and a half; there did not exist any ulceration. The history of the case being altogether against the supposition of the existence of polypus, I came to the conclusion that the menorrhagia depended on subinvolution; in fact, that the uterus had never regained its normal size and tone since the miscarriage, which had occurred two months previously. I therefore decided on carrying out a plan of treatment, the value of which I have repeatedly tested: I mean the introduction up to the fundus of the uterus of ten grains of the solid nitrate of silver, and leaving it to dissolve there. This I accordingly did. The application produced considerable pain, which lasted five or six hours, but no further unpleasant results followed. I confined this patient to bed for several days, but then allowed her to go about. Menstruation appeared at the regular time, and was mode*rate in quantity*; and she became pregnant immediately afterwards.

I wish to call attention especially to this case: first, as illustrating the occurrence of subinvolution as a result of abortion—a fact which is overlooked by many; next, as showing the dangerous menorrhagia that may depend on this condition of the uterus; and, thirdly, as proving the excellent results which follow the treatment adopted. Ergot, gallic acid, and, indeed, all other medicines will frequently fail to check menorrhagia depending on subinvolution; and we must have recourse to treatment directed to the uterus itself; we must stimulate the organ to set up that healthy action by which it regains its normal size after pregnancy has terminated—a process to which Sir J. Simpson has applied the term "involution." With this view, I unhesitatingly advocate the adoption of the treatment practised in the preceding case. I know no other so efficacious.

The mode of carrying it out is simple. The instrument known as Sir Jas. Simpson's "*uterine porte caustique*" is introduced into the uterus just as an ordinary uterine sound. This little instrument consists of a hollow silver tube, in size and shape closely resembling a sound; it contains a flexible stilette, which it fits accurately. As soon as its point is found to have reached the fundus of the uterus the stilette is withdrawn, and through the instrument is pushed up, by means of the stilette, a piece of solid nitrate of silver reduced to the requisite size and weight, till it is fairly lodged in the cavity of the uterus. In doing this there is but one caution requisite to be attended to; namely, that as soon as the piece of nitrate of silver has reached the extremity of the *porte caustique*, and before it is finally pushed out of the instrument—a point of which we can always be certain by observing how much of the stilette remains still unintroduced—the instrument should be withdrawn to the extent of about half an inch; for if this precaution be not observed, it is possible that the nitrate of silver might be forced into the substance of the uterine wall, instead of being left free in its cavity, an accident which, though possible, is very unlikely to occur. I have dwelt at some length on this plan of treatment, because I am satisfied that its value is far from being appreciated. It is looked upon by many practitioners as heroic and dangerous. I believe, and I have practised it for several years, that it is simple and safe. I do not say that it is always sufficient, and that a cure must always result; but in my hands it has been productive of marked success, and in no single instance have I known of its producing serious symptoms. The application of solid nitrate of silver to the interior of the uterus is by no means a novel practice. Dr. Evory Kennedy used to introduce it into the cavity of the body of the womb by means of an ordinary female catheter several years ago, but he did not leave it there to dissolve. To Sir J. Simpson we are indebted for the further development of the practice, and for the invention of the *porte caustique*. Dr. Kidd, I think, was the first of the Irish obstetricians who adopted this practice.



Menorrhagia resulting from ulceration of the os and cervix uteri is also of frequent occurrence. Mere abrasion of the lips of the os uteri is not sufficient to produce menorrhagia; but that unhealthy, spongy condition of the os and cervix, in which the mucous membrane lining its canal becoming hypertrophied and thickened, and bleeds on the slightest touch, the os being patulous and the lips everted, is quite capable of originating severe menorrhagia.

Mrs. B., a young married woman, aged 24, who had never been pregnant, stated that she had become greatly debilitated by the excessive loss which occurred at each menstrual period. Ergot and astringents were exhibited by the mouth, and astringent lotions injected into the vagina, without producing the least effect. The use of the speculum proved the existence of extensive granular ulceration of the os and cervix uteri. Now, in severe cases, s the one I am referring to, the unhealthy condition of the mucous mem extends at least as high as the os internum, and we will fail to effect a cu less our treatment reach every portion of the diseased tissue; therefore the view of permitting the necessary applications to be made to the who tent of the cervical canal, I commenced my treatment by introducing tw of compressed sea-tangle; two pieces being sufficient for the object I h view, which was not to open the uterus to such an extent as to enable examine its cavity, but only to permit me to treat the entire of the c canal. I left these pieces *in situ* for twenty-four hours, and on withdr them after the lapse of that time, cauterised freely the whole diseased s with fuming nitric acid. This did not cause any pain. On examining t uteri a few days subsequently, I found it in a much healthier state. *Th norrhagia was entirely checked, and never returned;* and, although a derable time elapsed before the uterus regained a healthy state, still th gress of the case was rapid and the cure perfect; the only treatment sequently necessary being the occasional application of a twenty grain so of nitrate of silver to the os uteri, and, at a later period, of a small blister sacrum; finally, not the slightest trace of the ulceration remained, and struation became in all respects normal.

The foregoing case illustrates perfectly the mode of treatment which I rule, adopt in cases of granular ulceration of the os and cervix uteri.—*I Medical Journal, June* 24, 1871, *p.* 666.

79.—ON THE VARIOUS METHODS OF TREATING THE PEDIC OVARIOTOMY.

By Dr. D. LLOYD ROBERTS, Vice-President of the Obstetrical Society of London, and Ph to St. Mary's Hospital, Manchester.

[It is now universally conceded that the first *bonâ fide* operation for the val of an ovarian tumour was performed by Dr. Ephraim MacDowell, of ville, Kentucky, U.S., in December, 1809. Mr. Duffin, of London, w first to recommend and practice the plan of bringing the pedicle throu wound and fixing it with sutures to the abdominal parietes. Mr. Duffin' case occurred in 1850.]

Mr. Erichsen introduced an important improvement into our practice in In a case operated on by him, an account of which is given in the *Lancet* vol. ii, p. 611), the pedicle was tied in two portions, and the abdominal i brought together by means of sutures closely applied, with two harelip the lowest part of the incision, the ligatures of the pedicle being twisted the harelip pins in such a manner that the pedicle was secured outside t domen between the lowest pin and the inferior angle of the wound. I Dr. W. L. Atlee, of Philadelphia (*Gardner's Scanzoni*, p. 255), first em the écraseur in the division of the pedicle, and applied the perchloride a sulphate of iron to all bleeding vessels, thus doing away with the necess the use of ligatures. The next and perhaps the greatest advance in the ment of the pedicle we owe to the ingenuity of Mr. Jonathan Hutchinson anxious to avoid the use of ligatures and the risks thereby induced, devi *clamp*, an instrument for the permanent compression of the pedicle un tached by a process of sloughing. The first clamp used by Mr. Hutc was a pair of carpenter's callipers; this he used in two cases, both of were successful. He was soon after followed by Mr. Spencer Wells eagerly adopted the new method, and in whose hands it has led to such b results.

The introduction of the clamp dates from 1858, since which there hav numerous and varied modifications of the original form: to a few of th must now refer. Shortly after using the callipers Mr. Hutchinson devi *clamp*. About the same time Mr. Spencer Wells devised his improved which is composed of two parallel blades provided with two screws for fixing, and which is figured in his paper entitled Cases of Ovariotomy, (Dublin, 1859, p. 36); this he continued to use until 1869, when he devised his circular clamp, which is figured in the *Medical Times and Gazette* (Oct. 30, 1869, vol. ii, p. 530), as made by Mayer and Meltzer. The chief objection to this clamp is that it is liable to cut; I found this in one of my own cases. There is nothing to prevent its edges being rounded off, however. He has since discarded the circular clamp, and now uses a modification of the original calliper type, with the upper and lower surfaces of its blades bevelled and rounded off, which is made by Krohne and Sesemann. This is an excellent clamp, and possesses many advantages over his former one. It lies more comfortably on the abdomen, and takes up less space, from its being curved with the concavity upwards; the pedicle is thus more easily dressed and the discharges are prevented from accumulating round it.

[The next method introduced in the treatment of the pedicle was that of Mr. Baker Brown, which consists in a combination of crushing and cauterization.]

Sir James Simpson proposed acupressure for securing the ovarian pedicle in January, 1860 (*Edin. Med. Journ.* vol. v., p. 649). He has described his method of procedure in the *Medical Times and Gazette*, 1860, vol. i., p. 285, and also in his work on *Acupressure* (Edinburgh, 1864), in which he reports a case illustrating its successful application (p. 489). A modification of this method was proposed by Mr. John Dix, in 1864 (*Edin. Med. Journ.* vol. x., p. 216), who thought that the disadvantages inseparable from the use of the needle as a compressing agent might be obviated by the substitution of a fine wire of silver or iron for the needle. In 1866, Dr. J. F. Miner (*Buffalo Med. Surg. Journ.*, vol. v., p. 432) suggested the plan of applying "a ligature to the pedicle as a whole, carrying its extremities through the vaginal septum, and in this way secure a drain for any purulent infection that might take place;" this he carried out in September, 1866, but, unfortunately, the case terminated fatally.

Dr. Graily Hewitt (*Brit. Med. Journ.*, 1870, vol. ii., p. 458) has contributed another to our stock of mechanical contrivances for treating the pedicle. His instrument [see woodcut in *Retrospect*, vol. lxiii., p. 292) consists of "a framework of steel, shaped something like a shoe-buckle, measuring 2½ by 1¼ inches . . . and is provided with studs or buttons eight in number, three on two sides, and one on each of the other two sides." It is used thus: the pedicle is perforated by a needle, bearing a double ligature of strong thread or whipcord in two or three places, according to the width of the pedicle; it is then tied in segments, the opposite ends being secured one by one to the framework which is now made to surround the pedicle, the cut edge of which is freely open to inspection and treatment.

The most recent and, apparently, a most effectual method of *dividing* the pedicle is that introduced to the profession by Dr. G. H. B. Macleod, of Glasgow (*Lancet*, 1871, vol. i., p. 108). It may be viewed as a combined process of crushing, compression and twisting. The pedicle is seized with a sort of clamp forceps, with narrow, small, and female blades, which are thus capable of maintaining a very firm hold, their apposition being kept up by means of a screw. After cutting away the cyst at the extreme distal end of the pedicle, the stump is seized with another pair of stirrup-shaped fenestrated forceps, also provided with quadrant and screw; this latter instrument being slowly turned, while the first-named forceps is maintained at rest, the stump of the pedicle is by this means twisted off close to the portion held by the first forceps. Mr. Hilliard has also made for Dr. Macleod an ingenious clamp of the calliper type.

Dr. Washington L. Atlee, of Philadelphia, has provided us with the most recent clamp, for which he claims the following advantages:—It is much smaller, lighter, and stronger than the older forms of clamp, and by its use the pedicle may be compressed in the smallest possible space, and what is of permanent importance in the *linear* direction of the wound, the direction most favourable for the approximation of its edges. I am indebted to Messrs. Krohne and Sesemann for an inspection of this clamp, and can testify to its great lightness, combined with elegance and strength.

The great objects in the treatment of the ovarian peduncle are:—1, to prevent hemorrhage; 2, to avoid peritonitis; 3, to avoid the risk of purulent infection (septicæmia, pyæmia) from the absorption of dead or decomposed animal tissues; 4, to avoid undue dragging on the uterus and its appendages; and 5, generally to avoid everything likely to interfere with the healing of the abdominal wound or to retard the recovery of the patient. All the methods devised for the attainment of these objects may be ranged under three heads, according as the stump of the pedicle is left within the cavity of the abdomen, or is partly within and partly without ("pocketed,") or is retained outside the abdomen; they may be termed the intra-abdominal, intermediate, and extra-abdominal methods.

The various methods of securing the pedicle so that it may be returned into the abdominal cavity comprise the use of the ligature, compression, torsion, cauterization, or crushing of the vessels, or a combination of two or more of these means, together with "capping" and "enucleation."

The ligature has consisted at various times of leather (N. Smith), hemp, silk, silver or iron wire, or catgut. It may be applied to the pedicle *en masse*, or one or more double ligatures may be employed, or each vessel may be ligatured separately. The ends of the ligature or ligatures may be cut short, or they may be brought out by the abdominal wound, by the fovea inguinalis interna, or by the vaginal septum. The practice of cutting short the ends of the ligature was rescued from desuetude by the energy of Dr. Tyler Smith, who in the year 1861 gave fresh impetus to this method. Certainly this practice is a most attractive one, as if the ligature becomes sacculated or remains innocuous, the case progresses rapidly to a favourable termination. For apart from the immediate shock of the operation and the risk of peritonitis, the greatest anxiety in these cases is felt less there should be excessive suppuration caused by separation of the pedicle, strangulated vessels, and adhesions; this being got rid of, the case becomes one of a simple incised wound in the abdominal wall, which rapidly heals. I recently (June 28th, 1871) removed an ovarian cyst and returned the pedicle with the ligature cut close; the wound in the abdomen healed by first intention, and the patient had not a single unfavourable symptom; she might be considered quite well by the tenth day after the operation. In one case which terminated favorably, Mr. Bryant, a very successful ovariotomist, reports (*Guy's Hosp. Reports*, 3rd series, vol. xiv., p. 228) that the ligatures which secured the peduncle came away through an artificial anus at the lower part of the abdominal wound twenty-five days after the operation; he also mentions two other cases which terminated favorably, the silk ligatures by which omental adhesion had been secured having been discharged externally through the wound. On the other hand, he mentions a fatal case in which "some half-dozen ligatures had to be employed, and were left in," and on post-mortem examination the ligatures were found in the abdominal cavity "resting in their own depôts of pus, having been thrown off from their attachments, and acting as foreign bodies." Notwithstanding these occasionally unfavorable cases, the practice is generally sound and productive of most favorable results. I am informed by Dr. Braxton Hicks that "the preference here (Guy's) is for tying and dropping in the pedicle."

On the other hand, when this method is employed, we know that adhesion takes place between the pedicle and the neighbouring gut, and in some cases circumscribed collections of pus have formed, until at length it finds an outlet either through the abdominal wall, the vagina, or the intestine.

Where the ends of the ligatures are brought out by the abdominal wound, the chief objections are that there is much greater probability of the strangulated portion of the pedicle becoming gangrenous, and of putrid infection as a consequence—there is also greater risk of peritonitis. Another objection is the length of time required for the separation of the ligature—I have known it to be thirty days. This retards convalescence, confines the patient to bed, and is a source of anxious care to the surgeon; moreover, it weakens that portion of the abdominal wall through which the ligatures pass. Another great objection to its employment is the tendency to the formation of fæcal fistulæ; "a sort of canal or sinus is formed by the adhesion together of folds of omentum or coils of intestines, in such a manner as to enclose the ligature and shut it off from the general peritoneal cavity" (Spencer Wells, in *Glasgow Med. Journ.*, Feb., 1868). This occurred in the case of lady upon whom I operated in June, 1866. On the fifteenth day a portion of intestine opposite the lower part of the wound gave way, and fæcal matter passed continuously through the wound for some weeks. The perforated intestine, however, eventually healed, and a good recovery took place.

[*Compression* may be applied to the pedicle by means of acupressure (Simpson), the wire compress (Dix), the serrenœud (Kœberle), and the coil-clamp (Aveling).]

Torsion may be applied to the vessels of the pedicle after division (Beebe). This is a very excellent plan, and has been very successful in the hands of its introducer. It is well worthy of a farther trial.

With reference to the method proposed by Dr. Macleod, if further experience should confirm the report given by him of the remarkable immunity from hemorrhage, and from constitutional disturbance of the patient whose successful case he quotes, it will speedily take a foremost place in our treatment of the ovarian peduncle. I shall certainly make a trial of his method in the first suitable case that presents itself to me. [For woodcut and description of Dr. Macleod's instruments see *Retrospect*, vol. lxiii., p. 296.] I cannot help thinking that it would be an improvement if his clamp forceps were made with parallel blades, as by this means the pedicle would be compressed on all its surface equally; as it is, the clamp forceps acting like a pair of scissors, compresses the pedicle unequally. The chief objections to the use of the *écraseur* are: the time required to separate the tumour, that it frequently fails to arrest the hemorrhage, and that it gives rise to as much suppuration as any of the other methods. Dr. Atlee, its introducer, has now discarded it.

I take it as a fundamental principle that no one method of securing the ovarian pedicle is of universal application; each case must be treated according to circumstances, and the multiplicity of methods furnishes us with ample choice. If I were asked how I should treat the pedicle, I should answer somewhat after the following fashion:—If the pedicle were long, narrow, and could be easily brought outside without traction upon the uterus or making any undue pressure upon the wound at the lower part of the belly, I should most certainly use the clamp; if, on the contrary, the pedicle were short, and I feared dragging or too much displacement of the uterus, and if it were not too voluminous, then I should transfix it with a firm ligature crossed so that it could be tied on one side only, cut the ligature close, and drop the stump into the pelvis. In cases where the pedicle could neither be safely brought out with the clamp nor ligatured for fear of vessels retracting or tissues shrivelling up; or where the pedicle was very short, vascular and fleshy, and I feared the ligature might cut it; or where it was very close to the uterus, I should secure it with a clamp (moderately crushing it), divide it, secure each vessel with a ligature cut short, and apply the actual cautery to the rest of the stump, waiting a few minutes to see if any oozing of blood took place before dropping it into the pelvis.

If the pedicle were very voluminous, the cyst having a very broad attachment to the uterus, so that I could not use the clamp to hold and crush it whilst I applied the cautery, I should divide it with a pair of blunt scissors slowly, taking up each vessel as I went on, and apply the cautery to the remaining portion of the stump.—*Manchester Medical and Surgical Reports*, *Oct.* 1871, *p.* 94.

61.—ON THE USES AND APPLICATION OF HODGE'S LEVER-PESSARY.

By Dr. ROBERT BARNES, Obstetric Physician to St. Thomas's Hospital; Examiner in Midwifery and the Diseases of Women to the University of London, the Royal College of Physicians, and the Royal College of Surgeons, &c.

[Hardly anything in uterine affections is less understood than the value of this instrument in cases of retroflexion and retroversion of the womb, and we are consequently glad of the opportunity, afforded by Dr. Barnes's clinical lecture on the subject, *to bring it before our readers.*]

The morbid condition which the lever-pessary is especially adapted to relieve, is retroversion and retroflexion of the uterus. The more we reflect upon the great frequency of this condition, the distress it entails, and the general efficacy of the instrument in curing it, the better shall we estimate the debt of gratitude under which our distinguished American brother has placed us by putting this admirable contrivance in our hands.

It is right to premise that Professor Hodge extends the application of his lever-pessary to many other morbid conditions besides retroflexions of the uterus. I would not so much as insinuate that the Professor has ever so little given the reins to his hobby as to exaggerate the uses of his invention. I know from experience that it is often of great service in chronic metritis, especially in inflammation with engorgement and hypertrophy of the cervix, in vaginitis, and several other diseases. By counteracting prolapsus, by securing comparative "rest" of the organ, by maintaining it at its proper elevation, it tends remarkably to counteract congestion, to restore freedom of local circulation, and thus to promote the cure. But the special subject of the present lecture is retroflexion of the uterus. I will not now discuss the causes of this displacement; they form the subject of a controversy which will probably be carried on for some time to come. One camp contends that it is a primary condition entailing congestion, inflammation, and other evils; another camp contends that it is secondary, and produced by the congestion or inflammation, which is often found associated with it. The rational course is to look at both sides of the shield. I have no doubt that retroflexion follows labour. The uterus remains bulky from imperfect involution, and the heavy fundus is driven back by the superincumbent pressure of the intestines; it may be the consequence of adhesions, the reliquiæ of peritonitis. The body of the uterus may in like manner be bent backwards by a tumour in its walls, by its bulk being increased by hyperæmia or hypertrophy, or by other morbid conditions. But close observation has convinced me that primary retroflexion is far more frequent. I meet with it frequently in women who have never had children or sexual relations. In fact, it is a frequent cause of sterility. In most, if not in all, of these cases I believe that the retroflexion is congenital. Until the advent of puberty, little or no inconvenience is felt; there is nothing to draw attention to the existence of the displacement. But when menstruation sets in, when the uterus under the ovarian stimulus grows and undergoes periodical engorgement, then trouble begins. The flexion at the neck aggravates beyond the physiological degree the menstrual hyperæmia of the body; the excretion of the menstrual discharge is impeded; a severe form of dysmenorrhœa becomes established; the bladder may become irritable; the enlarged body of the uterus projects into the rectum in the hollow of the sacrum, obstructing its canal like a ball-valve. Hence constipation, and gradual retrograde or ascending difficulty invades the whole alimentary canal; a degree of paralysis of the intestines ensues; coprostasis leads to decomposition of fæces in the intestine, to flatulence; then other dyspeptic evils follow; loss of appetite, attended, perhaps, by vomiting under the pain of dysmenorrhœa, blood-degradation, general impairment of nutrition. These surely entail disorder of the nervous system. Henceforth the patient's life is one of constant, or, at best, of intermittent suffering, under which she may break down altogether. You may exhaust the Pharmacopœia in treating the stomach, the liver, the brain, or the spinal cord; but so long as you leave the cause untouched, the patient will continue to suffer.

How are you to detect this cause? Let us first enquire what are the symptoms which indicate uterine or ovarian disorder? what are the symptoms which justify physical exploration of the pelvis? If you see dysmenorrhœa to a severe degree, attended or not by menorrhagia, followed by leucorrhœa; if you learn that the sexual function is endured with difficulty; if you see the patient's general health becoming impaired under the repeated suffering; and if you learn that ordinary general and local treatment, fairly tried, have failed to bring relief—I think myself, and I know that most patients will be of the same opinion, that you ought to ascertain the condition of the organs whose functions are performed with so much distress. The time has come to give up the vain chase of the disease through its consequences, and to apply our attention directly to the organs which are obviously at fault. I admit that general means, medicinal and hygienic, should have a fair trial. Not seldom dysmenorrhœa is relieved by time and mitigated by medicines; but, on the other hand, more often still, dysmenorrhœa and its consequent ills are intensified by repetition, until the local condition and the remote disorders, working in a vicious circle of action and reaction, become so firmly established as to imperil the success of any treatment. We assume, then, that the case calls for full clinical investigation. The finger and the uterine sound are the instruments of diagnosis. Direct touch necessarily precedes every other mode of examination. You feel, then, for the central point—the os uteri. In retroflexion or retroversion of the unimpregnated uterus this is generally found a little lower in the pelvis than is normal, and nearer the centre. In the natural state of slight anteversion the os uteri is directed backwards; in the abnormal state of retroflexion the os usually points downwards, perhaps a little forwards. Feeling the os, you of course ascertain its form and the condition of the intra-vaginal portion of the cervix. Associated with congenital retroflexion of the uterus, the vaginal portion is often found conical, and the os small and round, barely admitting a sound. Where this complication exists, something more than restoration of the body of the uterus to the normal position will commonly be required. But this subject may be put aside for the present. Your next step is to explore the roof of the vagina *in front of the cervix uteri.* Here, if the uterus is in normal condition, you may feel the solid rounded mass of its body through the vaginal wall; and, by combining the intra-vaginal touch with the extra-abdominal touch—that is, by pressing down with the other hand the abdominal wall above the symphysis pubis, so as to meet the finger inside—you may take note exactly of the position and size of the uterus between them; and if the uterus is not there, its absence will be made manifest.

You next proceed to explore in like manner the posterior vaginal *cul de sac.* If you feel a hard, even, rounded mass *behind the cervix* through the vaginal wall, tender to the touch, you have strong presumptive evidence, strengthened by the absence of the body of the uterus from its normal position in front of the cervix, that this mass is the retroflexed uterus.

Rectal examination affords additional evidence. The finger in the rectum will even travel round the outline of this post-cervical tumour to a greater extent than can the finger in the vagina, and thus help materially the diagnosis. When by long practice you have acquired skill, you may be quite satisfied with this evidence. But this *tactus eruditus* itself is not acquired without a course of cross-testing by other methods; and to arrive at complete demonstration of uterine flexions the sound is necessary. Before passing the sound you must carefully consider the probability of pregnancy. In the great majority of single persons, and of those who have lived a married life for some years without children, the probability is strongly in the negative. The presumption is also in the negative in those who, having had children, seek relief from the consequences of retroflexion. Among these consequences is frequently acquired sterility. Two things help in forming an opinion: the date of the last menstruation, and the bulk of the uterus. If the bulk be sensibly increased and a period have been missed, of course you will not use the sound. At the same time you will remember that increased bulk of the uterus is an almost constant attendant upon retroflexion. To pass the sound you give the end of it such a curve as the idea you form of the degree of flexion of the uterus may indicate. For, example, if you feel the fundus of the uterus falling below the level of the cervix, and feel an acute angle of flexion, the curve given to the sound must be considerable. But in the majority of instances a moderate curve will be enough. And by a little manœuvre—that is, by lifting up the fundus of the uterus by your finger whilst the sound is passing the os uteri internum, the seat of chief flexion—you straighten the whole organ somewhat, and thus wonderfully facilitate the passage of the sound. The first stage of the introduction of the sound is best effected by passing the end into the os externum, as far as the os internum, with the concavity of the curve directed forwards; then turn the point backwards, so as to bring the concavity backwards, whilst the guiding finger lifts up the fundus. At the same time you must carry the handle towards the symphysis pubis.

When the sound has gone the normal length of two inches and a half, as measured by the protuberance, you may very gently—lest adhesions oppose—ascertain the mobility of the uterus by bringing the point forwards again, so as to place the uterus in anteversion. Your finger behind the cervix now loses the firm rounded body, which, carried forwards, may be felt by the hand outside pressed down upon the symphysis. On withdrawing the sound, the tumour behind the cervix is reproduced, that is, the uterus has fallen back again. Then the demonstration is complete.

It was at one time supposed that this restitution or *redressement* of the uterus by the sound, if repeated frequently, would cure the retroflexion; but experience has proved this to be a delusion. Remove the sound, and the uterus commonly falls back again. Lift up the uterus gradually, as the lever-pessary will do, extend the support it requires over a long space of time, and the structural change which is necessary for permanent cure will be effected. The action and mode of applying the lever-pessary we will next discuss.

Selection of a proper Instrument.—In choosing a lever-pessary we must bear in mind the principle of its action. It is a lever, and we must take care to use it as a lever. It is very easy to impair or to destroy this, its essential character, by using too large a pessary. This is the case when the instrument puts the vagina tightly on the stretch. It then acts very much like the old ring-pessary, which prevents the uterus from falling chiefly by expanding the vagina and resting upon the floor of the pelvis. The lever-pessary requires no bearing upon the solid structures of the floor of the pelvis, and ought not to stretch the vagina. A lever must be mobile. It depends for its efficacy upon its responding to the natural movements of respiration. At every inspiration the diaphragm, descending, drives the abdominal viscera downwards, and the anterior wall of the vagina, with the cervix uteri and the base of the bladder closely adherent to it, descends. Now, if a bar or lever be so placed in the vagina that one end rests upon the anterior wall, whilst the other is applied to the upper part of the posterior wall, so as to impinge upon the depressed fundus of the uterus, it is obvious that with every inspiration the anterior arm of the lever, carried down by the descending anterior wall of the vagina, will cause the posterior arm to ascend, and thus lift up the fundus uteri. This is precisely how the lever-pessary works. Its action is in strict harmony with the physiological movements of the organs. It is gentle, gradual, and safe. It preserves the natural contractile condition of the vagina. And this gradual action is the great condition of cure of a retroflexion. At the seat of flexion, if this has existed long, there is change of structure—perhaps some degree of atrophy. Before the uterus can maintain itself erect, this spot of impaired structure must be restored to its normal integrity. This is a process of nutrition for which time is required; hence the lever-pessary must usually be worn for a considerable time, counted by months—perhaps for a year or more; but all the while it is doing good.

The material of the Pessary.—The best is undoubtedly vulcanite, on account of its perfect smoothness, and inalterability under exposure to the heat and discharges of the vagina. It is, therefore, perfectly clean; and when you have determined, by observation, the suitable size and shape, it is well to have one made in vulcanite for continuous use. But in the first instance it is more convenient to use the ordinary gutta-percha ring mounted on copper wire; this you can fashion yourself to the proper shape.

The size is determined by the capacity of the vagina and vulva, and by the degree of descent of the uterus. In single women one of very small size is commonly best; the elasticity and firm apposition of the walls of the vagina maintain the pessary in position. The touch gives you an estimate of the capacity and tonicity of the vagina. A ring 1½ in. or 2 in., or 2¼ in. in diameter is generally large enough. In married women, especially in those who have borne children, a ring of 2½ in., 3 in., or 3½ in., may be necessary. It is important that both ends should have but a moderate curve—should be, in fact, nearly straight. *This enables* the lower end to adapt itself better to the symphysis pubis and urethra, and the upper one to the rounded uterus. The lower arm is bent backwards, the upper one forwards.

The introduction is effected in this way. The patient lies on her left side. The forefinger of the left hand is first passed as a guide just inside the vulva, and presses the perineum slightly backwards. The pessary held in the right hand is then passed with its flat in a line with the vulva—that is, in the conjugate diameter of the pelvis. As soon as the upper end enters the vulva it is directed somewhat backwards, so as to avoid pressing against the symphysis. When it is wholly inside the vagina the guiding finger applied to the lower arch turn the pessary half round on its long axis, so as to bring its flat into the transverse diameter of the pelvis. The effect of this manœuvre is often to carry the upper arch in front of the os uteri. The guiding finger, therefore, passed inside this arch must press it back under the os, when it will immediately rise to its proper position behind the os and below the retroflexed body of the uterus. The riding of the pessary over the os sometimes causes a little pain. *In situ*, the os falls inside the upper loop, and displacement, provided the size and form are right, is nearly impossible. The adaptation of the lever depends upon the structure of the vagina—upon its anterior wall being shorter than the posterior; so that the anterior arm of the lever must be lower than the posterior one, which naturally rises into the *cul de sac* behind the cervix, to which place it is guided by the cushion between the lower part of the posterior wall of the vagina and the rectum, which forms an inclined plane rising towards the fundus of the vagina. The last thing to do is gently to push up the lower or anterior arch behind the symphysis with the tip of the finger. In single women the passage of the vulva is generally painful, and sometimes the displacement of the uterus is attended by such a severe degree of hyperæsthesia that the use of chloroform is indicated alike to facilitate diagnosis and the application of the instrument.

When *in situ* a well-chosen pessary should give no pain. If the vaginal wall is found much stretched, the pessary should be removed and a smaller one tried. Pain sometimes is felt from the pressure of the upper arch upon the tender congested uterus. Where there is any serious amount of inflammation, some days' rest in bed, perhaps weeks, should be used first. But generally the little uneasiness at first experienced soon wears off, and, as the pessary begins to act, sensible relief from the troubles due to the retroflexion is obtained. The pressure upon the bowel is gradually taken off, and after a time, aided by tonics and other means, the constipation is often materially lessened.

To remove the instrument, manœuvres in the inverse order of those used for introduction are necessary. There is no occasion, however, to bring the upper loop first in front of the os uteri. All you have to do is to hook the forefinger into the lower loop, which is directly behind the symphysis pubis, and whilst drawing it downwards and backwards to rotate it on its long axis, so as to bring the flat into correspondence with the vulvar fissure, and then to extract in the axis of the pelvic outlet.

Some few patients acquire the knack of introducing and removing the instrument themselves, but generally it is an operation that requires skill. It is worn continuously. It interferes with no function. Moderate exercise is beneficial, both by improving nutrition and general health, and, also by promoting the specific lever-action of the pessary. It is always desirable to see patients who are wearing the pessary from time to time—that is, once in two or three months. After a while, as the uterus rises, it may be useful to substitute a larger instrument. When endocervicitis or uterine catarrh or hemorrhage, complicates the displacement, as is not uncommonly the case, further local and general treatment will be necessary to accelerate the cure.—*Medical Times and Gazette, July 29 and August* 12, 1871, *pp.* 121, 181.

81.—ON POLYPUS OF THE UTERUS.

By Dr. J. Matthews Duncan, Edinburgh.

[Ordinary polypi are of three different kinds, fibrous, fibrinous, and mucous. Mucous *polypi are the most frequent*. There is no symptom of the disease worthy of description. No doubt various kinds of pain or uneasiness may be caused by them, but there is nothing distinctive in these sensations.]

The great sign of polypus is that loss of blood which is the main source of the importance of the disease. This hemorrhage may be menorrhagic—that is, occurring at the menstrual periods; but it may be incessant, or it may be accidental, occurring at any time. It is also highly important to remark that a polypus may exist without any hemorrhage whatever, and even grow while there is suppression of the menses, or while the menstrual flow is gradually getting scantier. I have never seen a false fibrous polypus, or a fibrinous polypus, which was not at some time or other the cause of bloody discharge. But the frequency, and therefore the importance, of bloody discharge or hemorrhage has been generally very much exaggerated by describers. Among the forty-one mucous polypi to which I have already referred, nineteen, or about a half, caused no extraordinary loss of blood whatever; while in several there was less than ordinary loss, and in some none of any kind.

It is rare to find considerable loss of blood produced by a small polypus, but I have observed it. The largest losses dangerous to life, which I have seen, were produced by true fibrous polypi, or by false fibrous polypi, which were undergoing extrusion from the uterine wall. In only one of the twenty-two fibrous polypi was there no extraordinary loss of blood.

When a mucous polypus causes serious loss of blood or anæmia, it does so, not by a flooding or in a short time, but by constant, or nearly constant slight oozing. Of course, to produce anæmia, the oozing must be to an amount greater than the system can repair as fast as it flows. On the other hand, bleeding from a fibrous polypus is generally more profuse; sometimes it deserves the name of a flooding. I have seen a woman's life in imminent danger from a single flooding in a case of true fibrous polypus, and it is not rare to observe cases where two or three hemorrhages have produced the most aggravated anæmia and all its painful consequences. Mucous polypi comparatively seldom, and comparatively slowly, produce extreme anæmia. Among the sixty-six cases to which I have been referring as mostly occurring recently in my practice, none proved fatal. In more than one it was my opinion that life could not have been much prolonged if an operation had not been performed. My impression, founded on experience, is, that death from bleeding in a case of polypus is much more uncommon than in a case of ordinary fibrous tumour.

Besides blood, mucous or muco-purulent fluid is frequently discharged from the vagina in cases of polypus. Such discharge may be of the nature of a simple vaginal or cervical uterine leucorrhœa, or it may be produced by superficial ulceration of the polypus or of the cervix uteri.

In cases of fibrous polypi, whether true or false, there is frequently a more or less copious watery discharge, of a more irritating kind than the ordinary leucorrhœal discharge, and sometimes to such an extent as to be weakening.

A woman can never be said to have a polypus unless the practitioner has either felt or seen it; in most cases he can easily both see and feel it. A mucous polypus is generally soft and rounded, but it may be dense and have various shapes. To the eye it generally presents a darker red tint than the neighbouring parts, as also does a fibrous polypus. Sometimes a true fibrous polypus is hard and nodulated, and of a very pale or yellowish white colour.

A false fibrous polypus is hard, nodulated, and very pale in colour, except occasionally, when it is much soiled by long lying in the genital passages. A true fibrous polypus is not always hard When it is œdematous, it may be soft, and even deceive the practitioner, causing him to believe that he has to do with a bag of fluid. A fibrinous polypus may be broken down by the finger, disrupting its outer fibrinous wall.

Polypi certainly have no sensitiveness, and at least generally no sensation.

When a practitioner suspects the existence of polypus, and yet, on a vaginal examination, does not find it, he has to consider whether he will further pursue the search for it. If this further search is merely carried the length of dilating the external os uteri, the practitioner may proceed without any anxiety. But *if he proposes* to dilate the whole length of the cervix for the passage of his *examining* finger, he must proceed with the greatest circumspection. For the exploratory operation may itself be injurious, even more so than the disease which is as yet only suspected to exist. Metritis, ovaritis, perimetritis, parametritis, are frequent results of this exploration, and death itself may be a consequence. I entertain a belief—of which, however, I have no sufficient evidence—that the forcible dilatation of the cervix uteri, in cases whose upshot shows that it was required, is not nearly so liable to be followed by untoward accidents as when it is done in cases which turn out not to have required it. Similar statements have been frequently made as to other dangerous remedies, and a so-called law of toleration, founded on such facts or fancies, has been enunciated.

It is very rarely, indeed, that dilatation of the whole cervix is required. In only one of the sixty-six cases was it resorted to in order to discover the disease.

Dilatation is effected by means of a tangle-tent. Sometimes a small one may be first used to begin the dilatation; and after it has done all in its power, or in about twenty-four hours, it is replaced by a larger; and again by one even still larger. The tent, with a string attached to its lower end, is seized by a vaginal forceps, or throat forceps, and passed into the uterus. It is well to introduce a small sponge into the vagina to keep it *in situ*. It is removed by pulling the string attached to its lower end. After the removal of the tent, the index-finger of one hand is passed into and through the cervix, while the other hand, pressing on the abdomen above the pubes, pushes the wound downwards upon the examining finger.

Besides this kind of exploration, very little is required in the way of diagnosing a polypus. A cauliflower excrescence has a rough surface, friable texture, a broad attachment, constant fleshwater discharge, and free bleeding when handled; characters which a polypus does not present. Chronic inversion of the uterus—a very rare affection—may cause a little more difficulty than a cauliflower excrescence. It is to be distinguished by the following points; its history, its occasional sensitiveness, its regularity of form, the largeness of the part which may be mistaken for a pedicle, the absence of the body of the uterus in its natural situation, and the impossibility of passing far up a probe at any part between the cervix uteri and the suspected pedicle. But it is a general rule that a complication or complications are the cause of extraordinary difficulty in diagnosis; and the practitioner must remember that a polypus may complicate an inversion of the uterus.

There is nothing simpler than to tell what should be done in a case of polypus. It should be removed.

In most cases the operation is very easy. A small mucous polypus may be removed by torsion. It, or its pedicle, is seized by an appropriate long forceps, and, by twisting, removed or divided. This little operation may be done with the aid of a speculum or without it. When the polypus is not very small, exceeding in size the phalanx of a small finger, it is best to clip through the pedicle of the polypus by an appropriate curved scissors. When the polypus is not large, and the woman has borne a child, the duck-bill speculum may be used to expose the polypus with a view to the operation; but it is generally convenient to dispense with a speculum of any kind. The operation may be done without an assistant, but it is better to have one. The operator seizes the polypus with a volsella in order to fix it, and in order in some cases also to pull it nearer the os vaginæ. The operator then guides an appropriate curved scissors by the fingers of one hand to the stalk of the polypus. His other hand holding the scissors makes it grasp and cut the pedicle. A pretty long curved scissors is generally used, but most such instruments are badly made, the cutting parts being too long and weak, in consequence of which they yield and disappoint the operator by refusing to cut. This error, however, is easily corrected by any cutler.

Instruments have been invented for cutting through the pedicle of a polypus, but they are illustrations of misplaced ingenuity. There is no difficulty in cutting through the pedicle of any polypus, and nothing is better for the purpose than a bistoury or an appropriate scissors. There is occasionally difficulty in reaching the stalk of a polypus, but no instrument for dividing the stalk will facilitate this part of the operation. A bistoury or a scissors will reach the stalk, if it be accessible, better than any other instrument.

The operator, in the case of a large polypus, may find the operation of removal very difficult. The difficulty lies, not in dividing the pedicle, but in getting sufficient access to it. In the case of an enormous polypus, where the difficulty is greatest, the division of the pedicle, if it could be effected as a preliminary to the operation of removal of the polypus, would make little difference in the difficulty of doing so. In a case of this kind, the difficulty is almost exactly that of delivering a fœtal head through a contracted outlet or over a rigid perineum. In both cases the perineum is likely to be torn, and sometimes the laceration extends even through the sphincter ani.

In cases of false fibrous polypi, the operator not unfrequently finds it unnecessary formally to divide the pedicle at all. Having fixed one or two volsellæ in the tumour to pull it through the os vaginæ, he finds, when the birth of the tumour is effected, that the false pedicle is also divided. I have never seen this happen with the true pedicle of a true fibrous polypus. On examining a false fibrous polypus after it has been removed in the way described, it is easy to distinguish the recently separated connexions from the rest of the surface of the tumour which has been for some time lying bare in the genital passages.

The ordinary operation, in the case of a large polypus, consists in fixing one or more volsellæ in the polypus, dragging it forwards through the orifice of the vagina, and then cutting through the pedicle as soon as it can be reached. If the pedicle be short, this dragging down involves considerable displacement of the uterus; but really the displacement is not very great, if the shortest route for the cervix uteri—that is, following the direction of the anterior wall of the vagina—is pursued. The stalk of a polypus does not require to be cut off; it shrinks and soon disappears.

As already said, the difficulty of operating for an enormous polypus is sometimes very great, and the difficulty consists in getting it out of the vagina. The getting access to the pedicle is a matter of comparatively little importance. Yet when the pedicle is reached all the difficulties of the operation are past, not because the pedicle is reached, but because the bulk of the tumour is extracted from the vagina. Many plans have been proposed for facilitating the extraction —such as cutting out a wedge-shaped portion of the accessible part of the tumour, or cutting the tumour spirally, as it is called. But these plans have an appearance of precision which I have been unable to imitate. I proceed on a simpler plan, removing portion after portion of the tumour, and this, if possible, in such a way as always to leave a convenient bit of the tumour projecting from the remaining part, by which piece a good hold is got for pulling the tumour further down. So, bit by bit, the whole is removed.

A fibrinous polypus may come away spontaneously; or it may be detached and removed by the finger, used in a hook-like manner, as the obstetrician frequently employs it to remove early abortions; or it may be broken up and pulled away by a forceps.

After removing any kind of polypus, no bleeding is to be expected In a case of small mucous polypus, or false fibrous polypus, or fibrinous polypus, I have never seen bleeding follow the operation of removal. After the numerous operations for polypi which I have performed, including those of the sixty-six cases referred to in this paper, I have not once seen an alarming hemorrhage, and I make it a rule not to plug the vagina after the operation, or do anything to restrain, except keeping the patient in the horizontal position. But I have seen a few times—certainly not five—an amount of bleeding which rendered hæmostatic measures expedient. In all of them, the bleeding began at the time of the operation, except in one case of fibrous polypus, where it was delayed till a few hours after the operation. In a case of mucous polypus where bleeding occurred, the pulsation of an artery in the stalk could be easily felt, and the large vessel was subsequently seen in the removed portion of the pedicle. In all of the cases a very gentle plugging of the vagina was sufficient to arrest the hemorrhage.

Fear of hemorrhage after the operation, and anticipated difficulty in severing *the pedicle*, have been for a long time, and to a great extent still are, bugbears of gynæcologists, and have led to considerable sacrifice of life from their inducing operators to resort to bad methods. I lately removed a large true fibrous polypus in the Royal Infirmary from a patient who, immediately after the operation, insisted upon returning home to a great distance by railway, and who never had any further trouble or confinement from the disease. This simple operation was witnessed by a stranger, who had recently seen a similar operation in a large hospital done by aid of Gooch's canula. The woman was a fortnight in bed, and at one time her life was considered to be in danger. The contrast of the results of these cases is not only fair, but might be made infinitely more striking by taking, instead of the second case, one of the numerous cases where strangling the neck of the polypus has proved fatal to the woman. A great variety of instruments has been invented for the purpose. I recommend the disuse of them all. The procedure is slow and clumsy, and should be rejected on these accounts, even were there no danger to life from the sloughing and other processes consequent on the strangulation of the pedicle.

It is scarcely necessary to say anything of the palliative treatment of hemorrhage in a case of polypus, for this is a temporizing which, under any combination of circumstances, can scarcely ever be required. Such palliative treatment I shall dismiss with a very few words; for it is the same as is adopted in menorrhagia, and it is equally efficient—a statement which, however ironical, is yet true. He would be a bold physician who would dare in any case to predict that any palliative would have the desired effect. The mere number and variety of such hæmostatic remedies, recommended as they are in menorrhagia, and in the bleedings of fibrous tumours, is to the thoughtful physician good presumptive evidence that they are all pretty inefficient. Practitioners delight in them, and in changing from one to another. The patients are probably at least as credulous as the physicians, and are comforted.

I have never used in a case of polypus the local application of solution of perchloride of iron. But this, if required, would probably be as efficient as in the ordinary alarming bleeding from a fibroid.—*Edin. Med. Journal, July*, 1871, *p.* 1.

82.—ON FIBROUS TUMOURS OF THE UTERUS.

By Dr. Lombe Atthill, Fellow and Examiner in Midwifery, King and Queen's College of Physicians, and Obstetric Physician to the Adelaide Hospital, Dublin.

[A fibrous tumour may be defined to be a growth composed of fibrous tissue identical in structure with that of the uterine wall, but disconnected from it, being in general surrounded by a capsule of dense fibro-cellular tissue, which is perfectly dry and loose, so that when one cuts on the tumour it almost of itself escapes from its cavity.]

Fibrous tumours are met with of all sizes, from that of a grain of shot upwards; those of large size being by no means of unfrequent occurrence, while cases are on record in which they have attained a size greater than that of the uterus at the full term of pregnancy and a weight of 70 lbs. or even more. Again they may be solitary, but usually two or more are present in the same patient. They may spring from the peritoneal surface of the uterus and can be felt through the abdominal wall; they may grow from the submucous tissue of the uterus; or finally be developed within the walls of the organ: consequently fibrous tumours are spoken of as belonging to one of the three classes—namely sub-peritoneal, sub-mucous, and intra-mural, according as they are found to grow in one or other of the situations I have designated. Of these the extra-uterine or sub-peritoneal being entirely beyond the reach of treatment must be dismissed after a brief notice. They vary in size and appearance in even a greater degree than either of the other varieties, sometimes being numerous, small in size, and sessile, giving the surface of the uterus a nodulated appearance, or on the other hand attached by a pedicle which is sometimes short and thick, or at other times so long and slender as to permit the tumour to float as it were free in the abdominal cavity, and finally even to dissever itself from all connection with the womb, and possibly to become attached to some other portion of the peritoneal surface. When sub-peritoneal fibroids are pedunculated they sometimes descend into the pelvis, and then by their pressure on the neighbouring organs give rise to most distressing symptoms. When this occurs the patient's sufferings are sometimes very severe, incessant desire to micturate or total inability to pass water being frequently experienced. Of course it is impossible to give relief unless the tumour be raised from its position and replaced above the brim. This is always a matter of great difficulty, sometimes an impossibility. The tumour invariably lies in the posterior *cul de sac* between the rectum and uterus, occupying much the same position which the impregnated uterus does when retroverted. With the view of raising it above the brim Dr. Kidd has adapted to this case the method suggested by the late Dr. Halpin, of Cavan, for restoring the uterus when retroverted during pregnancy to its normal position. He introduces one of Barnes's largest-sized India rubber bags into the rectum and gradually distends it with water by means of a syringe, while at the same time steady pressure is made with the finger on the tumour through the vaginal wall. In this way you will often succeed in raising the tumour and making it slip up into the false pelvis. Unless indeed the case be of long standing, and it be bound down by adhesions, should these exist your efforts will be not only useless but injurious.

Sub-peritoneal fibrous tumors do not necessarily give origin to menorrhagia, indeed as a rule they do not seem to influence menstruation at all. These tumours also generally spring from the posterior surface of the uterus; this however is far from being always so.

The sub-mucous pedunculated fibrous tumour is, prior to its removal, in no way distinguishable from, and is to be treated in a manner identical with the ordinary fibrous polypus of which I have already spoken. I shall not therefore allude to it any further but shall proceed to the consideration of the third and most important variety of these tumours.

Intra-mural or as they are sometimes termed the parietal or interstitial fibrous tumours are of frequent occurrence. They differ from the sub-peritoneal in two important features—namely, that they nearly always cause menorrhagia, and that they nearly as invariably stimulate the uterus to enlarge, an effect not often produced by the other form. Thus in Dr. Morgan's case just alluded to, though the tumour weighed upwards of 11 lbs. and was at least 25 inches in circumference, the uterus was of nearly its normal size and shape, while the presence of even a very small intra-mural tumour has been known so to stimulate the womb that it has grown to a length of five or six inches, while its walls have attained a thickness of an inch or more. Dr. West in his work "On Diseases of Women" mentions a case illustrating this fact. The growth of an intra-mural fibrous tumour is sometimes very slow. In a case at present under my observation, and in which the womb has attained a length of five inches, no appreciable change has taken place during a period of two years. On the other hand the tumour sometimes steadily increases in size, and then one of three results must occur—either it will bulge out the peritoneal surface of the uterus and possibly may become a sub-peritoneal tumour, or it may continue to grow in the substance of the uterus, the whole of the organ enlarging as the tumour increases—or it may project into the uterine cavity carrying before it a covering of the muscular tissue of that organ. It is easy to conceive how this process if continued may result in the formation of an intra-uterine tumour, connected with the wall by a pedicle consisting of muscular tissue continuous with that of the uterus, and of the mucous membrane covering it. That this pedicle may in time elongate, and as it lengthens become more slender, till finally it passes out of the uterus, or even spontaneously breaking its attachment is expelled from the vagina, nearly all writers with the exception of Dr. Matthews Duncan admit the possibility of this process. He however thinks that the uterine wall never elongates before the true intra-mural tumour, but that the tumour is expelled *bare* into the uterine cavity, enucleation of the tumour, a process to which I shall have to refer by and by, having taken place spontaneously. However one thing is quite certain, that these growths frequently present themselves as well-defined tumours projecting into the cavity of the uterus.

Very frequently, however, fibrous tumours appear as mere protuberances bulging out the uterine wall. Such tumours as these can hardly be removed with an *écraseur*, and yet you cannot leave them alone, for health is undermined, and life itself frequently endangered, by the hemorrhage arising from their presence. The treatment to be adopted in such cases necessarily divides itself into the palliative and the radial; the former consists of restraining the profuse flow which occurs at each menstrual period, by plugging the vagina, as recommended in a former lecture, and by the administration of hæmostatics, such as gallic acid, alum, &c., while ergot alone, or in combination with the perchloride of iron, is often useful. But this plan of treatment is most irksome to the patient, and can only be looked on as a means of delaying the fatal results which ere long must follow if more energetic means be not adopted.

Medicines without number have been administered with the view of causing the absorption of fibrous tumours of the womb. Prominent among these are the bromides. I have tried them fully and freely, and believe them to be of very little, if any, use. It would be waste of time for me to go through the long list of drugs which have been recommended in these cases. I do not wish to deter you from trying them in your future practice; they will probably do no harm, but I think I can promise that they will effect little good. For myself, I have lost all faith in the resolvent powers of medicines of this class in the disease at present under consideration.

The very limited good produced by medicines has induced obstetric surgeons to adopt energetic treatment, no less than five methods having been recommended and practised with the view to the radical cure of these embedded fibrous tumours. They are—1st, Incising the cervix uteri; 2nd, Incising the tumour; 3rd, Cutting into the tumour and destroying a portion of its tissue, a process to which the term gouging has been applied; 4th, Enucleation of the tumour; 5th, Avulsion or the forcible tearing away of the tumour from its attachment. Incising the os was first practised in this city by Dr. McClintock. This operation has been founded on the theory which, according to Mr. Baker Brown, is "that the division of the os and cervix uteri permits the fibres of the body of the uterus to contract upon the contained tumour, and thereby to compress the vessels and prevent hemorrhage." Whether this be the true explanation or not, one thing is quite certain, that the operation is often followed by good results, and in the case of very large tumours which are contained within the uterus, and when the cervix is thinned and spread over them, the operation is fully justified. The incising of the tumour has been practised by Dr. Attlee, in America, by Dr. Tracy, of Melbourne, and others, with success, a success which is probably due to the fact that the vitality of these tumours is nearly, if not altogether, destroyed by the incisions having divided their capsules, for the fibrous growth itself is endowed with but a very low degree of vitality. I have not met with a suitable case in which to try this treatment, but I certainly should not hesitate to do so were dangerous hemorrhages to occur in a patient in whom an intramural tumour existed, which I could not control by other means.

Enucleation, that is the cutting down on and dividing the capsule, and then grasping the tumour and turning it out of its capsule, is an operation suggested by a consideration of one of the processes by which nature occasionally effects a spontaneous cure, in which the capsule and investing covering of the tumour becoming thinned at one point, either by a process of absorption or ulceration, the contained tumour is then pushed out by the contractile power of the uterus, and is finally expelled. Enucleation is advocated by Dr. Matthews Duncan with his usual ability. He also practises with great success, the operation of avulsion, that is the seizing of a tumour with a strong vulsellum, and forcibly dragging it from its attachments.

Avulsion is practised by Dr. Duncan in cases in which spontaneous enucleation has been already partially begun, or where that stage having been artificially commenced, has proceeded to a certain extent. He considers it to be the proper practice in those cases of fibrous tumours in which the patient's life is in great danger, which medical treatment is unable to avert. I am not able to speak from personal experience as to the value of the operation, but you will find full details of Dr. Duncan's views on the subject in the twelfth volume of the *Edinburgh Medical Journal.* I am equally without experience as to th merits or demerits of "gouging," but am of opinion that surgical means hav been carried rather too far in the treatment of some of these fibrous tumours.

There is a less heroic mode of treatment I would have you bear in mind, anc under certain circumstances, to practise before having recourse to surgical measures. It is the injection, after previous dilatation, of tincture of iodine or c the liquor of the perchloride of iron into the uterine cavity. This practice i warmly advocated by Dr. Routh, of London, and if the cervix and os internur *be first dilated,* so that the injection may have a free and rapid exit, I do nc think that it is likely to be followed by unpleasant symptoms. My friend Di McClintock, informs me that he has recently injected tincture of iodine wit marked success in the case of a lady whom I had an opportunity of seeing wit him, and in whom alarmingly profuse menstruation occurred from time to tim and which he ascertained to be dependent on the presence of a large fibroid.

Dr. Matthews Duncan has recorded two cases in which he successfully r strained dangerous hemorrhage depending on the existence of a tumour in th uterus by the injection of the liquor ferri. perchloridi, one drachm of the flui being in each case injected by means of a hollow sound into the cavity of th womb. In his cases the cervix does not seem to have been dilated, a precautio I should always adopt.

I have now given you an outline of the pathology and treatment of the variou forms of fibrous tumours, but there yet remain two interesting and important phases of their history, to which I must allude before concluding the subject; the one, the increase and subsequent decrease in their size, which is sometimes observed, the other their occasional absorption, transformation, or even elimination.

All fibrous tumours, especially the sub-mucous, when they hang into the cavity of the uterus, are liable to become œdematous, and to this cause many of the recorded cases of enlargement and subsequent decrease in their size is referable. But in addition to this cause, menstruation and pregnancy undoubtedly influence both the condition and size of these growths. In many cases a fibrous tumour which ordinarily is productive of no discomfort to the patient, becomes at each menstrual period the seat of pain. This is a fact I have several times noticed. That actual increase in bulk should also occur at the epoch is easily understood. The following case illustrating the fact is recorded by Dr. Ernest Lambert, of Paris:—"Age of patient, 38. For ten years past a tumour appeared before each menstrual epoch, disappearing in turn to reappear again; for a year it has ceased to disappear, and has become the seat of severe pain." After death a large fibrous tumour was found growing from the anterior surface of the uterus From the same author I here quote the following instructive case on the authority of M. Depaul, who relates that having been summoned to a patient, at a distanc from Paris, he found three physicians in attendance on a primipara, supposed t be three months pregnant. She had suffered for some time past great difficult both in passing water and in defecation, and for four days previous to M. Depau seeing her had been unable to empty either the bladder or rectum, even th catheter could not be passed except with great difficulty. She suffered from th most powerful expulsive pains, and her agony was very great. M. Depaul recognised the existence of a large fibrous tumour which filled the pelvis; the patient's state was one of great danger. With difficulty he reached the os uteri introduced a sound, and brought on premature labor. The next day a fœtus flattened like a sheet of cardboard, was expelled; in a short time this tumou had decreased to a third of its former size, and at the end of four months wa not larger than a small apple; it was situated in the anterior wall of the uterus near the neck.—*Medical Press and Circular, Aug.* 16, 1871, *p.* 181.

83.—CASES OF CANCER OF THE WOMB SUCCESSFULLY TREATED BY BROMINE.

By Dr. A. Wynn Williams, Physician to the Samaritan Hospital, and Physician-Accoucheur to the Western General Dispensary.

[The details of the following cases contain full particulars as to the mode of treatment.]

Case 1.—*Medullary Cancer.*—Mrs. P., æt. 30, mother of one child, consulted me May 16th, 1868; well fed, but of leuco-phlegmatic temperament. She complained of pain across the loins, extending down the thighs, causing a constant feeling of sickness; bowels much constipated, always acting with pain and difficulty; menstrual discharge regular but very profuse, lasting seven or eight days; constant leucorrheal discharge. Tongue furred, heart's action feeble.

On making an examination per vaginam the finger came upon an uterus lower than natural, with a protrusion of the posterior lip to the size of a walnut, hard and smooth, not very painful on pressure; anterior lip and body, although large, appeared healthy and readily moveable. On examining with the speculum, it was with considerable difficulty, although using a full-sized one, that I could get the posterior lip within the end of the speculum; when within there was seen a mass about the size of a walnut partially smeared over with a creamy-looking discharge; when this was wiped off with cotton wool, the tumour was seen to be glassy and white, with a few dark spots scattered over the surface; anterior lip swollen, with open os. The appearance indicating the stage shortly preceding the softening and ulceration of a malignant tumour (medullary carcinoma). She was ordered an aperient and to take a mixture containing tinct. ferri perchloridi, tinct. nucis vomicæ, and spt. chloroformi. I determined to inject the growth with a solution of bromine, which I did at her own house on the 19th, using the syringe handed round, made of glass and platinum, charging the syringe with a solution of bromine, bromi. min. xij ad spt. vini rect. ʒ j. I passed the syringe through the speculum down to the growth, which, however, I found so hard I could not get the point to penetrate, until I procured a pair of hooked forceps, with which I took hold of the neck, drew the womb forward, whilst I forced the syringe inwards with a kind of half screw motion to nearly half an inch. I, before forcing out the fluid into the tumour, covered the surface of the exposed parts with cotton wool saturated with a solution of carbonate of soda, and then withdrawing the syringe a few lines so as to free the point, forced the contents about twenty minims into the tumour, leaving the syringe in *sitû* about two minutes. On the withdrawal of the point of the syringe a few drops of the fluid followed, but was immediately neutralised by the soda. As the patient complained of a good deal of pain, a quarter of a grain of acetate of morphia was injected under the skin of the arm, which soon sent her off into a doze.

May 19th. On examination the surface of the tumour, to about the size of sixpence, was seen to be of an ashy colour, but the slough had scarcely begun to separate. She had not remained in bed, and was tolerably comfortable. Ordered to continue the mixture and to use an injection, by means of an india-rubber syringe, of bromine min. xij, spt. vini. rect. ʒ ij, aquæ dest. ℥ xvj, three times a day.

23rd. The slough had not quite separated, but it could be seen to what extent the tumour had been destroyed, nearly one-half consisting of the anterior portion. I determined not to wait for the entire separation, but proceeded to inject the other portion of the tumour, which I did by passing the point of the syringe through the opening already made and forcing it backwards and upwards, taking care not to penetrate the mucous membrane covering the back of the tumour, using the same precautions as on the former occasion. Some of the bromine solution on pressing down the piston was forced through into the cavity already made. On removing the cotton wool the raw surface was seen as it were scorched. The patient complained of a good deal of burning pain, which was again relieved by the subcutaneous injection of morphia. She was also ordered ext. *cannabis indicæ* gr. i, each night, if required.

29th The slough caused by the first injection had now separated, the othe separating; any hard or suspicious looking portion was touched with the bro mine solution held in *sitû* for a few minutes. She had not had occasion to tak the pills. To continue remedies.

June 5th. All the sloughs had now separated, leaving a large cavity. A there was still some suspicious-looking growth at the back part, it was agai touched with the bromine solution.

12th. The cavity was filled with healthy granulations, there being n appearance of disease. To continue the bromine injection and mixture.

19th. Parts skinning over. From this time the wound rapidly healed an attained apparently a perfectly healthy condition. The patient visited me agai in the May following, when I examined and found the parts quite healthy. have not seen her professionally since, but have met her walking about appa rently in perfect health.

Case 2.—*Medullary Cancer.*—Mrs. L., æt. 28, widow, mother of one child consulted me May 7th, 1868. Had never enjoyed good health since her confinement nine years previous. Five weeks after confinement suffered from sever pain in the left side; six weeks after there was a great discharge of matter from the vagina. Has since then suffered more or less from pain and a discharge. Consulted me more particularly as to the propriety of again getting married. Catamenia irregular, not profuse. Pain in back and loins running up to left side under the ribs. On examination the womb was felt low down in the vagina, moveable, with anterior and posterior lips large, hard, and protruding. Examined with the speculum the os was seen stretched out laterally with everted edges. Sides higher up, being pressed closely together by a tumor on either lip, the one on the anterior lip being the larger and of a more pearly appearance. Before proceeding to destroy the tumours by bromine I had recourse to leeching and other remedies, usually employed for the removal of chronic enlargement of the os and cervix, without avail; and as matters were certainly getting worse instead of better, I determined on June 17th to attempt the removal of the growth in the anterior lip by bromine. This I proceeded to do by applying over it cotton wool saturated with bromine solution (♏ xij a ʒ j), by means of a vulcanite cup, taking the usual precautions to preven injury to the adjacent parts. The bromine was kept applied for two hours a my own house, the patient residing some miles off. On the removal of the cup &c., the tumour was seen to be whitened as if scorched. The patient com plained of a good deal of pain during the application, but was able to procee home in a cab. She was ordered to inject a weak bromine lotion, and to tak a mixture with tinct. ferri perchloridi, &c., and also, if required, every nigh ext. cannabis indicæ, gr. j.

June 23rd. Slough not yet separated, which appeared to include nearly th whole of the tumour. To continue remedies.

26th. Slough not entirely separated.

30th. More than two-thirds of the tumour removed, but the base of th tumour was unduly hard. A piece of cotton wool was now dipped in th strong solution, and kept applied to the wound for about ten minutes, th evaporation of the bromine being prevented by covering the wool, &c., wi gutta-percha sheeting. On removal the wound was observed to be we scorched. The bromine and spirit was again applied on July 3rd, 7th, an 10th; on the 15th all the granulations appeared healthy, and the wound wa allowed to heal, the injection of the bromine lotion being continued.

July 18th. Wound healing.

22nd. Nearly healed. Informed my patient I should proceed to destroy th other growth on the following visit, but she never appeared, and I saw nothi of her for twelve months, when, on the 31st of July, 1869, she again paid me visit. She informed me that family matters had necessitated her instant d parture from London, and that she had not had time to communicate with m before leaving. She also informed me that she had got married, but had su fered from wretched health ever since I had last seen her, indeed, had been to ill to return to London. She had been suffering from an attack of ague an rheumatic fever, for which she had been attended by a medical man in th country, but never mentioned her uterine ailments. She had suffered much from pain in the back, loins, &c., and irregular menstruation, sometimes very profuse, at other times very slight, and coming on at irregular intervals. After recruiting her health by rest, nourishing diet, tonics, &c., I proceeded on September 1st, to destroy the tumour in the posterior lip, which had considerably enlarged during the last twelve months, being also of a more pearly colour, with here and there a few dark spots. There was also considerable creamy discharge. The same means were adopted to destroy this tumour as had been used on the former occasion. The patient was allowed to leave town apparently quite well on the 21st of October. I have not heard of or from her since. I must just remark that there was no appearance of the return of the disease in the anterior lip.

Case 3.—*Epithelial Cancer.*—Mrs. B., æt. 49, presented herself before me at the Samaritan Hospital, February 13th, 1867. Mother of several children; had ceased to menstruate. Complained of pain in the back, &c., and great irritation in the genitals caused by the acrid discharge. On examination there was found sprouting out from the side of the os uteri a bright red growth, broader at the extremity than at the base, the extremity spreading out exactly like a branch broken from a cauliflower, bleeding readily when touched, and having a velvety feel. I diagnosed it to be an epithelial cancer in its commencement. To it the strong bromine solution was applied by means of cotton wool, &c., and was left in contact for about half an hour. She was also ordered to inject the weak bromine lotion. The same process was gone through on the 19th and 26th. On March the 5th there was none of the outward growth remaining, but the depression or stump, so to speak, was very red and hard. A bit of cotton wool rolled round the point of a stick wetted with the bromine solution was inserted into the depression, and held there for twenty minutes. This procedure was repeated on the 12th and 19th. The strength of the lotion was now reduced by one-half, and she was ordered ol. morrhuæ ʒ ij twice a day.

March 26th. The hole was now about an inch deep, with no appearance of disease. She was ordered to inject a simple lead lotion, and by May the 9th she was to all appearance quite well. Dismissed, with strict injunction to show herself from time to time, which she did up to the end of June. On the 25th the spot from whence the growth had been removed felt hard and looked very red. She was ordered liq. arsenicalis, min. v, twice a day in water, and to inject the bromine lotion.

July 9th. Tumour evidently growing again. I now determined to inject the strong solution with the syringe, and forcing the point of the syringe through the centre of the growth to the depth of two-thirds of an inch, forced in as much of the fluid as I could. To continue the arsenical solution and the injection, and to take pulv. ipecac. comp. gr. x each night if in pain.

19th. The growth was again injected, the syringe being passed through the opening already formed to the depth of fully an inch.

30th. The hole formed by the separation of the slough was nearly an inch and a half deep, and as the bottom felt to the probe somewhat hard, the bromine solution was applied by means of cotton wool, and again on August 6th. The arsenical solution was now discontinued, as it caused griping. After this date it was not considered necessary to apply any more of the strong solution, and the wound was allowed to granulate, using only the bromine lotion. I must here remark that the solution had never penetrated the canal, and that the growth appeared to have had its origin in the vicinity of the inner os. The patient continued under observation for some weeks after the wound had healed. I also saw her several times the year following, and up to the present time I believe there has been no return of the disease.

Case 4.—*Epithelial Cancer.*—Mrs. M., æt. 33, presented herself at the out-patient department of the Samaritan Hospital, February 11th, 1868. Had been married twice and had seven children; age of youngest four years. Was delivered of a dead child last November; present husband a sweep. Has been an inmate and out-patient at several hospitals, where, as she states, she was treated for consumption, the state of the womb never having been examined. She complained of *pain in the left side* which went through to the back, also pain in the lower part of the abdomen, thighs, back, &c., and a great discharge, acrid and somewhat offensive. Catamenia natural. A tumour could be fe the anterior surface of the neck of the womb, commencing near the os an tending upwards as far as the reflexion of the mucous membrane to the va which felt rough and velvety, not painful to the touch, but bleeding m readily when handled. The womb was somewhat enlarged but moveable. looking through the speculum a growth very much resembling sweep's ca as seen on the scrotum, was observed, only of a more vivid red colour. surface was not much elevated, and was of greater extent than the base, probe could be readily passed under its several lines. The vagina having protected with soda, &c., the strong bromine solution was placed over the face of the growth, and covered over with gutta-percha tissue, and fixed filling the speculum with cotton wool, the whole being left there about an h On removal the growth was seen to be whitened over its whole extent. was ordered to take a chalybeate mixture, and inject the bromine lotion t times a day. By the 21st the slough had separated, when the strong bro solution was again applied in the same manner.

April 25th. The surface of the tumour having been nearly all destroyed, base was now injected with the strong solution. As the lotion caused a g deal of smarting, it was ordered to be discontinued and warm water to be s stituted.

Feb. 28th. Had suffered a good deal of pain, bowels constipated, was orde a pill containing ext. cannabis indicæ, gr. i, ex. colocynth co. gr. ii, every ni

March 6th. Slough caused by injection not yet separated.

15th. Slough separated, suspicious-looking parts touched with the str solution.

19th. Touched again, and ordered to resume the injection of the lotion. is unnecessary to follow the particulars of the case further. I need only rem that under the use of the lotion the wound filled up, and the patient got q well, and in the following year, having removed to the country, was delive of a living child, which died some months after birth. She is now again n her confinement, if she has not already been confined. She has had no ret of the disease.

It must, I think, be admitted that cancer of the womb, when it comes su ciently under the cognizance of the physician, is amenable to treatment, o any rate certain forms of it. My own opinion is most decided that cancer growths or formations situated in the lips or neck of the womb can be succ fully eradicated when confined to this part only, and previous to the commen ment of ulceration. The fact of ulceration having set in must not be consi ed by any means as precluding any attempt being made for the removal of disease; on the contrary, as long as the womb is moveable and the glands other parts apparently healthy, the fact of ulceration having commenced sho be an incitement to immediate action. In point of fact, the earlier the dise is discovered, and active measures taken the better are the chances of the tient's recovery. No evil can possibly accrue, even supposing the tum operated on should not be malignant, as it is certainly as ready a mode of ting rid of it as any other with which I am acquainted. The manner in wh the disease should be dealt with will necessarily vary in different cases. more superficial, as some of the epithelial varieties, will be best destroyed by urating cotton wool with a strong spirituous solution of bromine, placing over vulcanite cup or gutta percha; any stump left must be treated as the more s tumours, and injected. The solid fibroid growths are best treated by injec of their substance, and for this purpose a small trochar and canula, to whic glass syringe can be attached, answers best. It is not easy to force the p of a syringe into the more solid growths, and, further, it is by no means e to force the fluid into the tumour. Without freeing the point of the syri all attempts to force out the contents will be futile. To do this the syri must be withdrawn a line or two. On the other hand, with a fine trochar th difficulties do not occur, the steel point is readily forced in, and, when wi drawn, leaves a space beyond the end of the canula.

The strength of the solution I now invariably use as an escharotic is twel drops of bromine to a drachm of rectified spirit of wine. This proportion I find to act far more powerfully than does pure bromine. It is not necessary to mix it immediately before use. I find that when it has been mixed even weeks and months its escharotic properties seem to be just as powerful as they were when the solution was first made, even though it may have almost lost its colour. This is no small boon to the operator, as the smell and irritation caused by the escape of the fumes of bromine from the recent solution is most distressing and, at the same time, very injurious. The fumes of bromine have so weakened the delicate structure of my olfactory nerves, that I have been for many months without the sense of smell; it is now returning, though very slowly. Before manipulating with bromine I now take the precaution of moistening some cotton wool with a solution of soda, and placing some of it in each nostril. I am not at all satisfied in my own mind as to what is the nature of the change that takes place when bromine is mixed with rectified spirit of wine. There is effervescence with evolution of heat. After a time the liquid, although stoppered ever so carefully and kept in the dark, gets paler and paler, but the loss of strength by no means keeps pace with the loss of colour. Probably we have bromide of ethyl and hydrobromic acid formed. On making trial with pure hydrobromic acid, I was surprised to find it very feebly escharotic. It is advisable that the solution should always contain some free bromine. I must not forget to mention that the injection of the bromine lotion into the vagina is a very essential part of the treatment; I find the surface of the wound under its use always clean and having no offensive smell. It was by observing the beneficial effects in open cancers in other parts that I was induced to employ it in diseases of the womb. I am, as many of you are aware, a great advocate for the application of weak solutions of iodine to scrofulous ulcers, and in this way I was led to try the effects of bromine on cancerous sores. This happened some twelve or fourteen years ago, when practising in the country. After leaving the country on account of ill health, several years elapsed before I had an opportunity of carrying on further experiments. It was not until I became attached to the Western General Dispensary that such an opportunity occurred, and it was at this institution that I first injected a spirituous solution of bromine into cancerous tumours.

Some of the results of this plan of treatment you have heard; whether the plan can be advantageously adopted when cancer affects other parts is still to be proved. I have never been able to give it what I consider a fair trial. the cases have been all too far advanced; nevertheless, the results have not been void of promise. The internal exhibition of bromine has not been sufficiently tried. I have ordered it on one or two occasions, but only for a short time; its use, to be of benefit, would, I should suppose, require a long continuance. The most convenient form of administering it is to mix it with glycerine and water; thus mixed it is not unpleasant to take—one patient considered the taste not unlike raspberry vinegar.

[The above paper, which was read before the Obstetrical Society, was freely criticised, especially by Dr. Playfair, who remarked that Dr. Williams's cases afforded no conclusive proof that they were malignant at all, inasmuch as fixature of the womb, which is the first positive sign of malignancy, did not exist, indeed, in each instance the womb was said to be freely moveable. Dr. Williams stated in reply, that there must be a time in the early stages of the disease when it was confined solely to the neck of the uterus, and in which the wound was not fixed; such was the case in the mammæ and other organs.] *Transactions of the Obstetrical Society of London, Vol.* xii., 1871, *p.* 294.

84.—ON TUMOURS OF THE BREAST.

By Christopher Heath, Esq., Surgeon to the Hospital for Women, and Assistant-Surgeon to University College Hospital.

[Even the highest authorities are not agreed as to whether undoubted cancer is a local *manifestation of a constitutional* taint, or whether the disease is at first purely local and the constitutional affection a consequence of the local m chief. The word "cancer" should be reserved for those tumours which, addition to the naked-eye and microscopic appearances generally recognised characteristic of that disease, have a clinical history showing progressive infi tion of the lymphatic system, leading to secondary deposits and a constitutio cachexia.]

The *recurrent fibroid* or *fibro-nucleated tumour*—the chief characteristics which are a close external resemblance to the ordinary fibrous tumour, but tendency to recurrence after removal, with a progressive softening of successi growths—is occasionally found in the breast, and an excellent example of it h recently been put on record by Mr. Nunn. (*Path. Soc. Trans.*, vols. xviii. a xix.) The tumour was developed in the right mammæ and measured 2 inches in circumference, weighing 4 lb. 12 oz. after removal. The patient w twenty-eight years old and the mother of two children; and the tumour h grown in two years, ulcerating through the skin at the centre of the grow fourteen months after it was first perceived. There was no enlargement of t lymphatic glands. The tumour was removed in March, 1867; and the patie made a good recovery. The tumour was lobulated and its structure fibro with spindle-shaped cells in abundance. Towards the end of May the patie discovered a fresh outgrowth; and on her readmission to hospital in July mass the size of two fists was to be seen at the site of the original tumo On July 31st, Mr. Nunn repeated the operation, but it was evident at the tim that the disease had extended through the intercostal spaces and could not b entirely removed. Fresh sprouting of the growth occurred, and the patie died exhausted on October 23rd of the same year. At the post-mortem exami tion, no secondary deposit in internal organs was found, but the tumour w seen to have invaded the pleural cavity, forming rounded protuberances unde neath the costal pleura. Microscopic examination showed the tumour to b composed of filamentous tissue abounding with nuclei, and such as would b generally classed as a fibro-nucleated tumour.

The experience of these recurrent tumours in other situations besides t breast is conclusive as to the necessity for complete and early removal, a therefore nothing less than the removal of the entire breast should be thought when any recurrence of an apparently simple tumour takes place. Even th the prospects of the patient are as gloomy as in true cancerous disease; for t growth is almost certain to reappear, and to destroy the patient, either by t exhaustion of repeated operations or by involving the pleural cavity.

The *myeloid tumour*, whose characteristic is the large multinucleated c resembling those of the fœtal marrow, is found much more frequently in conn tion with bones (and especially the jaw-bones) than in the mamma. Doul have been thrown upon the return of myeloid tumours; but, from what I ha seen in the case of the jaws, I have no doubt that myeloid tumours are freque ly, though not constantly, reproduced. I have never met with a case in breast, and must therefore content myself with referring to one recorded by Paget. The patient was fifty years of age, and had an irregular tumour, betw two and three inches in diameter, in her left breast, which had existed for n months. On removal, it proved to be myeloid; and eighteen months later patient returned with a large ulcerated tumour in the corresponding axilla, f which she died.

The forms of cancer occurring in the breast are scirrhus, medullary, and coll and their frequency agrees with the order in which I have placed them, scirr being more common than medullary disease, and colloid cancer being of r occurrence.

Scirrhus or *hard cancer* is rarely seen before thirty years of age, and may pear at any period after that, but about fifty is the most common age for disease to show itself. Unmarried women appear less liable to the disease t married women. Thus, of 260 cases of scirrhus given by Mr. M. Baker, 23 cent. were in single women, 72 per cent. in married women, and 4 per cent widows; and consequently, as Mr. Paget remarks, the percentage among sin women is smaller than in the female population generally. Commencing a *small nodule*, so painless at first that it is often overlooked for some time, disease slowly increases in the majority of cases, though occasionally, in what has been termed acute scirrhus, the increase is rapid. Soon, as a rule, pain is experienced in the tumour, of that peculiar lancinating or stabbing character which is often looked upon as pathognomonic; but it must be remembered that absence of pain is by no means conclusive as to the non-malignancy of the tumour, since some cases of scirrhus run their course with little, if any, suffering. As the tumour increases, the tendency to involve and drag upon the surrounding tissues is developed; and thus, if the disease is central, the nipple becomes retracted and fixed; or if peripheral, the skin overlying it is bound down and dimpled. Lymphatic enlargement now takes place, the glands along the axillary border of the pectoral muscle being those ordinarily affected, though the glands above the cavicle are also liable to infiltration, particularly when, as Sir Astley Cooper has remarked, the original disease is on the sternal side of the nipple, so that the internal mammary lymphatics become the means of conveyance of the morbid material.

Ulceration of the retracted skin is the next pathological feature, and there is, I think, more variation in this than in any other part of the disease. In some cases the ulceration is so slow, and the consequent discharge so slight, that the patient is able, for months and even years, to conceal her malady from her nearest relatives and friends; whilst in others the ulceration proceeds with rapidity, and extends either widely or deeply—for it rarely does both. The depth to which scirrhus may penetrate is of course limited only by the pleural cavity, perforation of which has been often witnessed; but to the extent to which scirrhous ulceration of the skin may reach there is really no limit save the vital powers of the patient. Thus, a fortnight since, I saw a woman, in very fair general health, with an enormous ulcerated surface on the chest and axilla of the size of a plate, which showed no tendency to become arrested but by the death of the patient.

By the time the skin over a scirrhous breast has become at all extensively ulcerated there will in most cases probably be some amount of constitutional cachexia produced, particularly if enlargement of the lymphatic glands has been an early symptom in the case. Added to this, we have the œdematous and painful condition of the arm due to obstruction of the axillary vein by the cancerous deposit; and the patient is at last worn out with the pain and suffering acting upon a lowered vitality. The duration of a case is affected very much by the age and constitution of the patient, its course being slower in the older and less plethoric individual; but the dictum of Sir Astley Cooper may be considered a fair one, taking the average of cases—namely, two years for the full development of a scirrhous breast, and from six months to two years longer for a fatal termination to the case. Instead, however, of running its ordinary course, the disease may become arrested in its progress by a form of atrophy, and the patient may then live for years, and die from disease unconnected with the breast. It is, so far as I have seen, in thin elderly women that this atrophic scirrhus is found; and it may happen, as in a case recently under my care, that the disease will suddenly light up afresh, as it were, and its renewed activity will require immediate operative interference.

In these cases, and also in other cases of scirrhus, particularly (in my experience) those in which the primary disease has been removed with success and without return in the breast, we meet with growths of cancer in internal organs—such as the liver, stomach, or uterus,—which may be taken as evidences of a constitutional taint consequent upon or antecedent to the primary disease, according to the pathological creed of the surgeon. Development of cancer in an internal organ is, however, ordinarily so much less painful a cause of death than open cancer of the breast, that this well-known liability to the development of internal cancer ought to form no bar to operative interference in suitable cases. In addition, moreover, to these well-recognised positions of cancer, I have once seen a patient, whose breast had been removed some months before, covered all over with small cancerous tubercles of the skin, varying in size from a pin's head to a medium-sized button; and as there was ptosis with strabismus on one side, together with symptoms of brain disturbance, I have no doub that a cancerous tumour was developed in the brain which pressed upon t third nerve, though I was unable to procure a post-mortem examination.

Medullary Cancer is much more rapid in its course than scirrhus, often de troying life in a few months. As might be expected from the differences i structure of the two growths, the medullary tumour is larger, softer, and mo succulent than the scirrhous cancer. Its tendency to infiltrate the neighbou ing tissues is much greater; and hence, when the skin gives way, it is by process of sloughing, or very rapid ulceration, leaving a chasm through whi protrude masses of soft bleeding cancer, giving the appearance characterised true *fungus hœmatodes*. Any rapidly-increasing solid tumour of the brea must be viewed with suspicion, and more particularly if the veins running ov it are much engorged, and if there is enlargement of the neighbouring lympha glands. In this form of disease there is much greater probability of a develo ment of secondary tumours in other parts of the body than in scirrhus; th not unfrequently patients suffering from medullary breast are carried off some acute chest attack, when secondary deposits are commonly found in t pleura and lungs.

Colloid cancer is exceedingly difficult of recognition prior to removal; in fac almost all the examples of it have been mistaken for some other disease. Th disease wants the hardness which is so characteristic of scirrhus, and is les rapid in its growth than medullary cancer, the lymphatics being also more slowly affected. A good example of colloid tumour of the breast, with a secondar tumour in the axilla, of five years' growth, is recorded by the late Mr. Price i the 8th volume of the Pathological Society's Transactions, and has appended t it an elaborate report upon the microscopic structure of the growth, by Dr. Andrew Clark.

A section of a recent scirrhous tumour shows a dense white structure, th fibrous nature of which is at once obvious to the naked eye. On scraping it whitish juice exudes, and this under the microscope shows an abundance o cells, with nucleus and nucleolus, which may be considered to be the so-calle cancer-cell. I have the advantage of showing you Mr. Arnott's beautiful drawing of the microscopic appearance of scirrhus, and I cannot do better than quot his own description. The drawing shows "the typical form of hard cancer—*i.e.*, cells of an epithelial type, of varying size and shape, but with tolerabl uniform (and usually single) large nuclei closely packed in the meshes of a stou fibrillated stroma, without any visible cellular elements." The proportion cells to the fibrous stroma it is that determines the character of the growth, f if the cell element preponderates over the fibrous, we have the more vascul form of medullary cancer or encephaloma; whilst if the cells have degenerate and the fibrous interspaces are filled with a little fatty fluid only, we have t atrophic scirrhus.

There is, then, no special microscopic appearance by which medullary canc can be distinguished from scirrhus, except by considering it as a whole, a here the naked-eye appearances and clinical history assist us much. The brai like texture, the great vascularity, and the rapid growth and invasion of su rounding structures, enable us to recognise encephaloma in almost all case It is remarkable, as showing how one form of cancer runs into the other, th the secondary deposits in the axillary glands and internal organs after scirrh of the breast partake more of the medullary or soft form of cancer than t hard; and, in fact, Mr. Arnott's illustration, which I show you, is taken fro an axilliary gland. This fact is not so surprising, however, when we find th as the recent researches of MM. Cornil and Ranvier have shown (by means nitrate of silver as a reagent), the lymphatics communicate directly with t interspaces in the fibrous stroma of scirrhus in which the cells are lodged. is not surprising, therefore, that the cell element should predominate in t secondary deposits thus produced.

The structure of colloid cancer is unmistakable, a section showing the loc filled with gelatinous material of various shades of colour which are so chara teristic of the disease. Microscopic examination shows a delicate fibrous strom enclosing in its meshes cells, of which some are round or oval, mononucleate and having within the outer cell-wall several very delicate concentric lines, gi ing to the cell somewhat of an oyster-shell appearance (H. Arnott.) Other larger polynucleated cells are found, and also an arrangement of the stroma in concentric circles, enclosing a number of nucleated cells, and having nuclei interspersed among the concentric layers of tissue.

Few persons in the present day would deny the hereditary nature of cancer, though the late Mr. Charles Moore held that the disease "should rather be styled heritable than hereditary," since he believed that the failures in transmission were more numerous than the occurrences.

The results of operations for cancer of the breast vary of course considerably according to the nature of the case and the previous health of the patient. The operation itself is a remarkably successful one as regards immediate danger to life, although every surgeon must occasionally lose a patient after this, as after every other surgical proceeding, even the simplest. But, in stating the risk to a patient, one has the satisfaction of pointing out that in all probability great relief from immediate suffering will be obtained, and a possible immunity from return in favourable cases. With regard to the prolongation of life as influenced by an operation, professional opinion has undergone a change within the last few years. It was thought by many surgeons that, though comfort was gained by an operation, yet that life was shortened; but the elaborate tables of Mr. Sibley and Mr. Baker, published in the *Medico-Chirurgical Transactions*, show a different result. Mr. Sibley, whose statistics are drawn from the records of the Middlesex Hospital, states that "in the cases of cancer of the breast, those who had been operated on lived 53 months, whilst those in whom the disease was allowed to run its natural course lived only 32 months." Mr. Baker, whose data are drawn from Mr. Paget's experience, says "the average length of life in scirrhous cancer is 43 months when the primary disease is not removed, and 55 months when the operation is performed; whilst in the case of medullary cancer the results are even more striking, being 20 months without, and 44 months, or more than twice the time, with an operation." These results are sufficiently encouraging, and probably the statistics of operation cases may prove more and more satisfactory as both the profession and the public become convinced of the necessity for an early and complete removal of cancerous growths.

With regard to the average date of recurrence of cancer, the greater malignancy of the medullary cancer is again seen, that disease ordinarily recurring within one year, whilst scirrhus does not reappear till the second year, the average times given by Mr. Baker being nearly 14 months for scirrhus, and 7 months for medullary disease.—*Lancet, June* 24, 1871, *p.* 847.

85.—A NEW CAUSTIC FOR MOTHERS' MARKS.—CARBOLIC ACID.

By Dr. George Berwick, Sunderland.

It may not be uninteresting to some of your readers to call their attention to the following case which occurred lately in my own practice.

Mrs. W., a young lady, during an early stage of her last pregnancy, received a strong mental impression by accidentally seeing a young girl with a large hairy mark on her face. The consequence was, that her own child when born, was marked in a similar way. One half of the nose, the upper part of the left cheek, and over the left eyebrow half way on the forehead, there was a large hairy, corrugated, dark-coloured, ugly-looking mark. As an experiment, I tried the strong glacial carbolic acid with the most satisfactory result. I took four ounces of carbolic acid, B. P. (as manufactured by Davy, Yates, and Routledge), put the bottle in warm water till the acid liquefied, and then added a teaspoonful of distilled water, in order to keep it from again crystallising. With this liquid acid I painted the mark twice a day, taking only a limited portion of the mark at one time. By repeated applications, a scab or crust formed, which ultimately scaled off, and left the skin beneath perfectly free from blemish, much to the mother's gratification. I assure you the whole of the large mass is now removed, *with the exception* of a small portion about the size of a half-crown, part of which is over the upper eye-lid, which we are now touchin more cautiously and slowly in order to save the eye from harm.—*Lancet, Sep* 2, 1871, *p.* 347.

86.—ON THE TREATMENT OF INCESSANT HICCUP.

By Dr. John Ross, Chesterfield.

[The following admirable epitome of the treatment of hiccup was elicited by letter of enquiry on the subject to the Editor of the *Lancet.*]

Allow me to recommend the application of the ether spray to the neck a epigastrium (so as to affect the pneumogastric and phrenic nerves), with f doses of bromide of potassium, at the same time giving ice and soda-wate Should this fail, I should be inclined to blister the neck, and give large doses quinine during the day, and chloral hydrate at bed-time. Dr. John Constabl about two years ago, related in your columns a case of persistent and alarmin hiccup in pneumonia, which was cured by the subcutaneous injection of mo phia under the intercostal muscles. This treatment is well worthy of furthe trial. Widal records a case of obstinate hiccup, accompanied by serious symptoms, which was cured by twelve-grain doses of quinine, repeated every day i the same dose for three days. The hiccup, which had lasted without intermission for nineteen days, never returned. The same author relates another cas which was cured by valerianate of zinc with extract of belladonna, and a thir by Vichy water and gentian. In the last case the stomach appeared to hav been chronically inflamed. In continued fever, hiccup is occasionally troublesome, and will sometimes yield to small doses of rhubarb. Dr. Habershon related, about eight years ago, to the Medical Society of London two cases of hiccup. One could be restrained by counting continuously, and it stopped on th patient walking about. It was cured by iodide of potassium given freely, the steel and blistering to the neck. In the second case the hiccup preceded th formation of a perineal abscess, and did not finally cease until it had bee opened and was quite healed. Dr. H. thought both cases due to some indirec irritation of the pneumogastric nerve acting on the spine, and thence reflectin on the phrenic nerve. In the case of a young woman in whom hiccup was present night and day, depending on some intestinal irritation, the late Dr. Tod gave a scruple of calomel, and recovery ensued.—*Lancet, July* 29, 1871, *p.* 181

ADDENDA.

87.—ON THE PHYSIOLOGICAL ACTION OF THE ORGANIC HYDRIDES.

By Dr. BENJAMIN W. RICHARDSON, F.R.S.

[When hydrogen combines with the organic radicals it forms hydrides of them. The hydrides of methyl, ethyl, propyl, and butyl, are highly inflammable gases, but the *hydride of amyl* is a liquid, so light that it boils vehemently in the hand. A glass globe of it boils actively by merely placing the palms of the hands on the sides of the globe. It is thin and bright, and runs like water. It is this liquid, which, diluted with ether, is known as anæsthetic ether, and as it is itself perfectly innocuous, and procurable in any quantity by careful distillation of American petroleum oils, we can hardly foresee the extent of its future utility in medicine. The following are some of the practical applications of Amyl Hydride.]

Anæsthetic Ether for Local Anæthesia.—The first application studied had relation to the production of a new fluid compound for local anæthesia by the spray process. The hydride having been found soluble, in all proportions, in rectified ether, various combinations of the two fluids were made, and the effects registered. In freezing with the spray, it is not the best practice to freeze too rapidly, for if the superficial parts be instantly frozen, the layer of frozen surface acts as a non-conductor, and deeper freezing is rendered impossible. At the same time, the practice is bad that delays the freezing process too long, since delay gives pain in the act of freezing, and pain during reaction. The point to find, therefore, was the correct medium or proportion between ether and the hydride for the end held in view. After many experiments and many applications for operation on the human subject, I find that a mixture of one part of the hydride to four parts of ether is the most effective and ready fluid for spray. I call the mixture "Compound anæsthetic ether for local anæsthesia." As a rule, this compound induces, in the form of spray, perfect insensibility of the skin in from ten to twenty seconds of time. It gives less pain than ether, when applied to a cut or open surface; and in operations upon the teeth it is much better in action than the best ether used alone. It may be diffused into the mouth, as spray, with perfect safety, having no quality that needs to be dreaded. Once or twice during long operations on the mouth, and when the compound vapour given off from the spray was unavoidably inhaled freely, there was produced general insensibility, but this was rather favourable than otherwise to the operative procedure.

Solutions.—I have said that the hydride of amyl is a solvent of many medicinal substances; and this fact has led me to use it in medicine, as a solvent, in various ways. I show you some of the compounds thus formed.

Iodized Hydride.—Iodine dissolves readily in amyl hydride, and produces, in the proportion of twenty grains to the ounce, a solution of great service in practice. When this solution is applied to the skin, the volatile hydride passes off at once as vapour, and leaves the iodine, in considerable quantity, behind, stranded on the part in most equal form of distribution. This application is of singular utility in cases of hard open sores, where it is desired to apply iodine evenly and deeply. Thus, in cases of open strumous glandular disease, the solution plays an important part as a means of cure, and the same in chronic indolent bubo. In bad sloughing fetid ulcerative and suppurative wounds, and in cancer, no solution is so simple, painless, and effective. In these last-named cases the iodine exerts more than a curative influence—it deodorises; it destroys decomposing organic products; it prevents the absorption of decomposed products, and *protects against* the secondary fever depending on such absorption. In applying the solution in the cases named, it may be gently poured over the part. There is necessity neither for cotton-wool nor for the brush.

From the iodized solution of the hydride, iodine itself may be inhaled with advantage in cases of ulcerated throat, and in cases of cavity of the lung. Indeed, whatever value in the treatment of phthisis and of bronchial phthisis there is in iodine, it is best obtained by the mode of administration now being described. Of course we have amongst us much difference of opinion as to th actual value of iodine inhalation, and I do not suggest this method in support o any one particular opinion. I hold my own view, and in favour of the practice in fitting cases, but I wish, for the moment, merely to describe a ready metho of applying the practice, so that all who wish may adopt it. In using th twenty-grain solution for inhalation it is best to dilute it with more of th hydride until the vapour of the iodine given is scarcely at all irritating to th throat. The patient's own sensations on the matter are here the best guid and with a very little instruction it is easy to secure that five grains of iodin shall be inhaled at one time. There is not the least occasion for hurry, or fo causing the slightest constriction of the fauces, or pain. I usually administe from a little funnel of parchment paper, holding it in some finely teased cotton wool, on which I drop the solution. From this funnel the patient breathes holding it a short distance from the nostrils and mouth, so as to allow the admission of plenty of fresh air.

The solution of iodine in amyl hydride has another application, adapted this time not to the sick person, but to the chamber of the sick. There is no agent at one and the same time so potent for purifying the air of the sick-room as iodine. I introduced it several years back for purifying the air of the room or ward in which sufferers from small-pox are lying; and from all parts of the world, but from India especially, I have received recognitions of the value of the practice. But there has always been some difficulty in carrying out the process. Diffusion by volatilisation of the metalloid itself from a chip-box covered with muslin—a method invented by that able surgeon, Mr. Hoffman, formerly of Margate—although it is in many cases most effective, is in most cases too slow, and if I may use such a term, too local; while the plan of driving off the iodine by heat from a porcelain or metal plate is not a plan to be safely entrusted to a nurse. But with the volatile iodide solution all difficulty subsides. We take a packet of ordinary filter paper, the paper being cut into pieces three inches in diameter—in fact, cut as it is sold from the chemical storehouses—and on to a packet of one or two dozen, or more, of such papers we pour the solution until all the pieces are fully saturated with it. Then the papers are allowed to dry; they dry very quickly, and are put into a box ready for use. We give a dozen or so of these papers to the nurse, and tell her to keep some of them exposed to the air in two or three places about the sick-room, so that the odour of iodine may be faintly recognisable through every part of the room; and this done, all is done for ordinary circumstances. To meet any unusual unpleasantness of the air, the nurse may take one or two of the papers and burn them like a taper or spill, when the deodorisation will be the more rapidly and determinately carried out.

In instances where a room or ward has been occupied by infectious cases, and it is required to purify quickly and effectively, the iodized hydride may be used in the form of spray. The spray-producer to be employed must be constructed of glass, as a metal spray-tube is injured by the solution. Siegle's simple tub answers for the purpose well. It is advisable before using the solution to hav the room to be disinfected completely stripped of all furniture, the walls rubbe down, and the floors well swept, scrubbed, and dried. Then, from differen positions in the room, the iodized hydride should be distributed in spray. Th solution containing twenty grains to the ounce is strong enough. The room should have all its windows and doors closed before the iodine is distributed and the quantity of solution sprayed should be measured. Practically, I find that one ounce of the solution to four square feet of space is a good adjustment of quantity to space. After the iodine has been distributed from the spray producer, the room should be kept closed for twenty-four hours at least; during this time the iodine deposited, at first, in the finest layer on the floor, ceiling and walls, slowly volatilises, and, coming into contact with the organic matter, destroys it rapidly. It is prudent not to take a light into the room after the distribution of the solution, until the windows and doors can be reopened, as the amyl hydride vapour easily takes fire.

The most persistent and offensive odours in rooms that have been occupied by the sick may, by this simple method, be more speedily purified than perhaps by any other known method. In asylum practice we get the most difficult of tasks of purification; for, from the bodies of the insane, organic compounds—probably of the sulphur class—diffuse and permeate everything, yielding the most offensive smells. As products of disease, these, perchance, have not a little to do with the unhealthy condition and physical derangement of the bodies from which they are emitted; and when they once fix in an apartment or room, they stay with a perseverance that is wonderful. I was consulted quite recently, at a house I visited, respecting a room of the house in which an epileptic man died, even months ago. This patient, during his fatal illness, suffered from profuse perspirations giving off the most offensive odours, and still in the room where he had lain, despite all efforts at cleansing it, there was distinct evidence of the odour. To remove this unpleasantness there is nothing approaches iodine, as asylum experience has proved, and the best way of applying the iodine here, again, is by the spray process described. But when the process is being carried out, it must be carried out thoroughly. If the room be opened too quickly, and air be admitted so as to create too speedy a diffusion of the iodine, the cure will only be temporary, and after a lapse of three or four weeks the odour will be once more distinguishable; for these organic odorous products, if they be not absolutely destroyed, release themselves in time from the destroyer, and, being less evanescent, proclaim that the victory over them is incomplete.

[The nature and some of the properties and uses of hydride of amyl are explained in the preceding part of this article. There are, besides the solution of iodine in hydride of amyl, two other solutions of simple substances in which the amyl hydride is the solvent.]

Solution of Oils and Fats in the Hydride.—Common oil mixes in the hydride of amyl freely, and stearine, spermaceti, and other similar fatty substances, readily go into solution in it. When a solution thus made is exposed to the air, the volatile hydride evaporates, and leaves the oily or fatty matter behind. By this simple means we are able to leave upon the skin an even layer of substances which effectually exclude the air, and which, if required, may act also as a means for diluting more active remedies. There is here in the hydride a solution of spermaceti and olive oil; the spermaceti, cut into fine shavings, is added, until the fluid is saturated, and then a sixth part of oil is added. This solution is most useful in the treatment of burns. It is gently poured over the burnt surface, and as the evaporation of the hydride proceeds, the cooling that occurs is an immediate source of relief from pain. In time, the part is left covered with a pellicle or false skin of fatty substance, and the air is entirely excluded. If, when the surface of the body is freshly covered with the solution, a little cotton-wool is lightly placed upon it, the rapidity of evaporation is subdued, and the dressing is more effective. I met myself with the accident of a severe burn this winter, and treated the injured part precisely as I have stated, and with the best success. The relief from pain was all but instantaneous, and, by re-applying the solution over the cotton-wool, I kept the pain entirely in abeyance until the balance of the circulation was restored. I left the dressing on the abraded surface, retaining it with a light bandage, and when, a few days afterwards, I removed the cotton-wool and the layer of fatty matter, I found the healing perfected. It is important in using this solution to avoid bringing the flame of a candle or other light near to the part, the vapour that goes off being very inflammable.

The advantage of the hydride over ether and other volatile solvents of fats is, that by its presence it excites no pain. The oxides and the chlorides themselves create local pain on sensitive surfaces—this fluid does not. In some forms of skin affection, where there are symptoms of extreme vascularity, heat, and irritation, the application of a mixture of the hydride and olive oil is very soothing and useful, and I should think it might be of service in some ophthalmic cases; but on this point I have as yet had no experience.

A solution of wax, or of spermaceti, in the hydride may be turned to much good account in bandaging. The bandage is well saturated with the solution, and is applied while in the moist condition. In a short time the bandage is left quite dry, and the part encircled by it is encased in a firm but flexible structure which effectually excludes the air.

Ammoniated Hydride.—When a solution of ammonia is placed in contact with amyl hydride, bubbles of gas rise freely from the ammonia, and diffuse into the light hydride. After a time the hydride may be decanted off, when it will be found strongly impregnated with ammonia. The mixture, I think, is merely mechanical; at the same time it is very useful for several purposes. The solution may be diluted with more hydride, until the pungency of the ammonia is reduced, and then the vapour may be inhaled—a method of administering ammonia which in many cases would prove of advantage. In scarlet fever, for which disease ammonia is so excellent a remedy (I had almost said an antidote), we are often embarrassed, in cases where the patients are very young, because we cannot get the sufferers to swallow ammonia in solution. In such cases, then, we could with little difficulty introduce the remedy by the lungs, and so sustain the fluidity of the blood and keep up the action of the heart.

A solution of camphor in this ammoniated solution of the hydride is useful as a preservative of animal substances, and may be applied for the preservation of specimens of natural history. The specimen to be preserved is dipped in the solution, and is allowed to remain in it until it is quite saturated. Then it is removed, and the hydride is permitted to evaporate, when the structure is left charged with the camphor, and is retained in good condition for a long period of time. Half an ounce of camphor in sixteen ounces of the hydride forms a good compound for the purpose named.

Lastly, on this point, the simple ammoniated hydride serves well for keeping pathological preparations during considerable periods of time. A good many years ago I discovered that if a little common ammonia be put into a closed jar with a pathological specimen, the specimen retains its freshness for many weeks. I now use the solution before us in preference to the simple ammonia, and with still better results. Into a pint jar I place, with the specimen to be preserved, half an ounce of the ammoniated solution of the hydride, and, after the vapour is well diffused, I close the jar closely with a tightly-fitting stopper. The jar being placed in a cool place, there is not the least occasion to hurry over an examination, for the specimen will keep (even for microscopical research) three or four days in excellent condition.

AMYL HYDRIDE AS A GENERAL ANÆSTHETIC.—To complete the research instituted as to the physiological action of amyl hydride, I have experimented with it as a general anæsthetic. There are many points in common between this substance and amylene (the anæsthetic introduced by the late Dr. Snow).

To test the properties of the hydride, its effects were first tested on pigeons, guinea-pigs, rabbits, and frogs. In all these researches the same plan was adopted. The animal in each case was placed in this glass chamber, which has a capacity of 1000 cubic inches. The arrangements here are such that every condition of a practical kind is at hand. A measured quantity of the vapour is gently diffused through the air of the chamber with hand-bellows: an aneroid barometer within the chamber gives the atmospheric pressure, a thermometer registers the temperature, while, by means of a warming apparatus beneath, the air can be sustained at a fixed temperature at all seasons of the year. The amount of vapour yielded at the temperature that exists during the time at which the experiment is carried out is calculated, and, by a simple contrivance, the animal is so introduced that no air is let in to interfere with the charge of vapour within the chamber at the moment of the introduction. Thus we have means for obtaining the facts we require to learn, in a steady and systematic manner.

In order to produce a decisive effect with the vapour of amyl hydride, the air in the chamber was charged with different percentages of the vapour at 65°

Fahr., and some preliminary experiments were made, in which the special symptoms induced were not noticed in detail, the production of decided anæsthesia being only sought. After this the particular phenomena presented were carefully noted.

The research with hydride of amyl was commenced by narcotising pigeons. Observations on these animals are specially important, for, as they are extremely susceptible to the dangers of anæsthesia, it may be safely inferred that an anæsthetic which proves safe to them will prove safe to the human subject. Moreover, in the pigeon, even more than in man, vomiting and muscular excitability during the action of the narcotic are easily induced; and, again, in them, modification of the animal temperature is always strikingly marked. In a word, so close is the analogy of symptoms caused by an anæsthetic in a pigeon and in the human subject, the value of any anæsthetic may be safely determined by the effect produced on the pigeon; and no new anæsthetic agent ought ever to be administered to the human subject until the inferior animal named has been found in several instances to pass through the ordeal with perfect safety.

When, then, in the case of amyl hydride, a pigeon is exposed to an atmosphere containing from thirty-five to forty per cent. of the vapour, the period required for the production of decided symptoms is under a minute, and within two minutes the insensibility is so profound that the animal must be removed into pure air. The insensibility is accompanied with slight muscular movement, and sometimes with a drawing back of the head; but the muscles speedily relax, and the sleep is calm, and attended with the deepest insensibility. On removal of the animal from the vapour, and on placing it in pure air, the recovery is very rapid—from one and a half to two minutes being sufficient to insure entire restoration to consciousness. The specific lightness of the vapour, and its insolubility in the blood, account for the briefness of the insensible condition it produces. The temperature of the body of the animal is but slightly modified; there is a reduction of temperature, but it amounts barely to the fourth of a degree on Fahrenheit's scale. In this particular the amyl hydride differs materially from chloroform and ether. Chloroform administered to the third degree of narcotism causes a reduction of from 6·5 to 6 degrees of temperature in pigeons, and ether causes a reduction of 4 degrees. Perhaps the shorter period of administration required to produce the deep insensibility when the hydride is the agent employed, may account for the difference observed. On guinea-pigs and rabbits the vapour of amyl hydride acts with the same rapidity as on pigeons. Recovery from the effects is also equally rapid, while the animal temperature in these animals undergoes no appreciable decline. In these animals the muscular excitement is not developed, but in the rabbit there is noticed a rapidity of breathing during recovery which instantly ceases if a little of the vapour be re-administered.

In order to see the extreme effects of the amyl hydride, the vapour of it was made to kill;—the animal after being narcotised was, I mean, retained in the vapour until the evidences of life had ceased. Animals of the three classes named above were thus allowed to sleep to death. The process of death is gentle—a continuance, as it seems, of the deep sleep. The circulation and the respiration appear to cease simultaneously; but in one case I could hear the heart in motion a few seconds after the cessation of respiration. Towards the close of the life the temperature falls suddenly from 1° to 1½° Fahr., and the pupils dilate.

After death the heart is found fully charged with blood on both sides and in all the cavities. The arterial colour of the blood on the left side is decidedly darker than is natural, but the coagulation is not interfered with, and the corpuscles show no evidence of change.

The lungs after death are not found congested with blood as in death from agents which kill simply by causing asphyxia, neither are they found bloodless and blanched, as is so common after death by chloroform and some other volatile chlorides. They are left retaining a natural colour and character. The brain shows no visible sign of injury.

For a long time after death from amyl hydride the muscles of the body retain their irritability, and may be called into vigorous action by electrical excitation. I recollect no agent that destroys life and interferes at the same time so little with the muscular irritability. Let it be understood, however, that this retention of condition for muscular motion under stimulus extends only to the voluntary and the semi-voluntary muscles. In the midst of the life of the other muscles the involuntary heart is dead, and makes effective response to no stimulus, not even to electricity.

The influence of the vapour is slower on frogs than it is on warm-blooded animals; but after a little time frogs become so profoundly narcotised, it is hard to determine they are alive, and if the temperature be lowered they will lie in the vapour for two and three hours, and still, on removal, will recover. Thus, if a strong, healthy frog, narcotised deeply, be placed in a shallow glass basin, with a little of the liquid hydride around the body, the evaporation of the liquid will keep up such a degree of cold that change into fatal death is suspended: and when the liquid has evaporated altogether, if the temperature of the air be kept at from 65° to 70° Fahr., and a little water of the temperature of 70° be occasionally supplied to the animal, it will recover when every indication of life is, to the naked eye, absent. In plain fact, it is difficult to say when death has actually occurred. The muscular irritability may positively be so suspended that it cannot be called forth by stimulus, and yet recovery may be the ultimate result.

After I had conducted these experiments, I ventured to inhale the vapour of amyl hydride myself to the production of unconsciousness. I placed four drachms, by measure, of the fluid in this vulcanite inhaler, pouring the liquid upon a double layer of domette, with which the inhaler is loosely lined. The vapour was to me agreeable to breathe; it caused none of the suffocating sensation experienced from the inhalation of vapour of ether, and none of the irritation produced by the vapour of chloroform; but it induced a sensation of gentle glow or warmth in the chest. After six deep inspirations of the vapour, I felt evidence of change in the cerebral circulation—viz., giddiness and inability to stand firmly, with the swaying movement, or sense of movement, common to the first stage of narcotism. Then, for a few seconds, I lost consciousness, but the inhaler being removed, I recovered very quickly, and in three minutes was perfectly well. Neither nausea, chilliness, nor headache followed the administration.

On the whole, of the facts thus gleaned by experiment, I infer that the amyl hydride, in addition to its other useful applications, might be employed as a general anæsthetic for the prevention of pain in certain surgical operations, such as extraction of teeth, where the operative proceedings are short—not exceeding, that is to say, two or three minutes in duration.

There is one other application of amyl hydride, interesting in a physiological as well as practical point of view, to which reference should be made. It becomes of service in studying the question of restoration of life after death by extreme cold. In some animals of cold blood—fishes and frogs—if their bodies be suddenly frozen, it matters not how extreme, how absolutely through the mass of the animal body the freezing may be, there will still often be recovery on thawing their bodies, if the process of thawing be not too rapidly carried out. We may therefore, taking one of these frozen animals with the intention of restoring it, so utilise the fluid as to moderate the thawing at will. We place the frozen animal in a tall glass jar, and over it we pour some hydride of amyl. Then we warm gently the lower part of the jar; and as the liquid can never be made to exceed the temperature of 86° Fahr., if the evaporation be perfectly free, the animal thaws in it without any danger of what is called reaction, and recovery is the general fact.

I cannot do better in closing this part of the lecture than demonstrate the fact just named by an experiment. Here are some frozen fish that have by the frost been frozen in an aquarium; they have been retained in the frozen state in this room by a surrounding of ice and salt; and here also, are some frogs frozen so completely that they are, you will say, of stony hardness. I will pass some of these frozen animals round, but one of them—a frog—I will retain. I take this glass jar, an anatomical-preparation jar, fourteen inches long and two and a half inches in diameter; I place the animal in the jar, pour over it five or six fluid ounces of the light hydride, and apply a gentle warmth so as to raise the fluid to its maximum of 86° Fahr. There is not the least occasion now to do more than wait and watch. In a short time, as the muscles of the animal relax, there will be a sign of renewed respiratory action, in the escape of a bubble of gas from the mouth of the animal. As I am speaking of it the phenomenon occurs, although there is no apparent breathing movement. That escape of gas is of itself quite sufficient. I turn the fluid, together with the animal, into a large glass funnel, having a narrow escape-tube, so that the fluid can run away into a receiver below it, and the animal be left in the air. Soon the animal recommences to breathe, and, as you see, in the freshness of its renewed life, it leaps out of the vessel. It has recovered without sustaining a trace of injury.—*Medical Times and Gazette, September* 23 *and* 30, 1871, *pp.* 374, 401.

88.—ON HYDRAMYL AS A GENERAL ANÆSTHETIC.

By Dr. Benjamin W. Richardson, F.R.S.

In the course of the present year, 1871, I have made a new investigation of the amyl hydride as an anæsthetic. I was led to this new, or rather renewed, research by some observations on the effect of the compound anæsthetic ether, when it was being used in the form of spray. I have stated that this compound is a mixture of amyl hydride and rectified ether; and the facts I observed in using it for producing local anæsthesia were, that when it was applied for operations on the nose and mouth the patients often passed into a temporary general insensibility whenever the operation was prolonged. In an operation for cleft palate in a youth, Mr. Christopher Heath being the operator, this general anæsthesia was twice fully developed during the operation. In a case where a small but tedious operation was carried out for removing a piece of dead alveolus, the same event occurred, and even in two cases of simple extraction of teeth it was repeated. The sleep in all these cases was so insensibly induced, was so gentle, so deep, and yet so temporary, it could not fail to attract my attention, and as it was clear the insensibility was due to the inhalation either of the vapour of the hydride or of the ether, I began a series of inquiries as to the part played by the hydride. The labour has been useful in that it has led to the application of hydramyl for producing general anæsthesia with great rapidity for short surgical operations. To amyl hydride I have given, for the sake of brevity, the name of *hydramyl.* I am indebted for the suggestion of this abbreviation to Mr. Ernest Chapman.

In recommending the research, I obtained from Mr. Robbins a good specimen of the fluid hydramyl. It had a specific gravity of ·625, and it commenced to boil at 86° Fahr. (30° Cent.) It was nearly inodorous, but to some persons it gave the sense of a faint odour. When breathed it created no irritation, and I found, by breathing the vapour of it out of a bag charged with it to the extent of 60 per cent., that I was almost immediately rendered unconscious, the recovery from the inhalation being singularly rapid. I found, also, that by inhaling it again, as I had done originally, from the simple vulcanite inhaler, the same effects could be produced.

On June 6 I administered hydramyl for the first time for a short operation —viz., the extraction of a firm molar tooth from the lower jaw, Mr. Peter Matthews being the operator. The patient was a woman 30 years of age. I poured into a small vulcanite inhaler two fluid drachms of the hydramyl, and, letting the patient hold the inhaler herself, asked her to take a few deep inspirations. She carried out the instructions readily; and at the end of twenty seconds, as there was distinct evidence of an effect, I removed the inhaler, and asked her to open her mouth. She complied, and Mr. Matthews immediately extracted the tooth. The whole proceeding was from twenty-five to thirty seconds. Within the minute the patient had recovered, and was talking to us consciously. She *said, immediately after* she commenced to inhale she felt as if she were passing into a natural sleep. She remembered being told to open her mouth, and said she obeyed "as well as she could," but she could recall nothing more relating to the operation; she felt nothing whatever of the extraction. In recovering she had neither vomiting nor nausea, although she had breakfasted a few minutes before; in brief, her recovery was as perfect as it was rapid.

The character of the anæsthetic sleep itself was most satisfactory. It was induced without a movement of any kind; the face retained its natural colour and expression; the pulse underwent no change whatever.

There was in the case this phenomonen, on which I must dwell:—The insensibility to pain occurred before the actual abolition of consciousness. I have observed, with Snow, precisely the same condition from amylene, and have made the fact matter of comment in my reports to the British Association for the Advancement of Science. I have observed the same phenomenon in experimenting with methylic ether, and I do not think there can be a more interesting subject of study.

The metaphysicians, in treating of conscious and unconscious states of mind, have long taught that there may be periods of consciousness with an absence of common sensibility, and the truth of this inference is now sustained by physical facts. In several cases where I administered methylic ether for removing pain in surgical operations, the patients, when quite insensible to pain, were so conscious they were able to obey every request asked of them, and in some instances were even anxious to reason, stating that they knew what was going on, and arguing that they were not ready for the operation because they were sure they should feel pain. Nevertheless, in this state of mental activity they were operated on, and afterwards, while remembering every incident, were firm in their assertion that they felt no pain whatever during the operation. One patient, who sat for the extraction of two teeth, selected the tooth to be first extracted, putting her finger to it, and afterwards re-arranging her position for the second removal. To the looker-on it seemed, in fact, as though no change in her life had occurred, yet she affirmed that she was sensible of no pain whatever; and several other less striking, but hardly less singular, examples came before me. We may then, I think, fairly assume that in course of time we shall discover manageable and certain anæsthetic substances which will paralyse sensation only, leaving the muscular power unaltered, and the mental little disturbed; and we gather from this either that in the cerebral hemisphere there is some distinct and simple centre of common sensation which may be acted upon by certain agents without involving all the cerebral mass, or that the peripheral nervous matter may be influenced without involving the other portions of the nervous system. On the whole, I incline to the view that the action of those agents which destroy pain before they remove consciousness is primarily on the peripheral system; for we know, from the process of local anæsthesia, that it is easy to destroy sensation at the extremities without destroying or even interfering with consciousness; while those who have inhaled the vapours which destroy common sensation before interfering with the mental condition, invariably describe the experience of a numbness and insensibility in the extreme parts of the body.

I offer this theory, wishing, as on all occasions, to enforce the fact only on which it is based. I have said the fact is of importance physiologically; it is of equal importance practically. To have an instrument in our hands by which we can, at will, induce insensibility to pain, with or without destruction of consciousness, as the case before us may demand, means, when it is perfected, an advance second only to the discovery of general anæsthesia itself—a refinement of the art rivalling the art. There are many minor surgical operations for which consciousness need not be destroyed, although pain ought to be; there are other operations in which the consciousness of the person operated upon is of great service to the operator; and there is a third class of cases in which it is essential to suspend both sensation and consciousness.

What will not, then, be the perfection of our science and of the art that follows it, when we can so master pain at pleasure as to isolate the consciousness from the common sensation, benumb generally, and from within, the periphery of the nervous matter, leaving the grey external matter of the cerebrum undisturbed; or plunge, if need be, consciousness and sensation alike into forgetfulness? Yet this in process of time is certain to be accomplished. It is an advance practically founded at this moment; it waits only for more labour to be made manifest to the world by its daily application.

Returning to the details of practice, I next administered the vapour of hydramyl on June 19 and 24 to three patients for the extraction of teeth, and in both cases with success; but I found a little difficulty, owing to the higher temperature of the air that then prevailed, in retaining the fluid in the inhaler, its lightness and low boiling-point causing it to evaporate with too great rapidity; in fact, a few breathings of the patient emptied the inhaler. I therefore proceeded to an endeavour to meet this difficulty by slightly weighting the fluid with a heavier body, but one having still a vapour density nearly the same and a low boiling-point.

Hydramyl-Chlor.—In making bichloride of methylene, we place a mixture of alcohol and chloroform in contact with pure zinc. On the application of heat there is set up a brisk action, during which an equivalent of chlorine from the chloroform ($CHCl_3$) passes to the zinc, and, after a free escape of gases, bichloride of methylene (CH_2Cl_2) distils over. On my request, Mr. Robbins, while manufacturing some bichloride of methylene, added to the materials in the retort prepared for making the bichloride about eight times the volume of amyl hydride. The result was an immediate brisk action without the aid of heat. A copious stream of gases first passed over, and then, the fluid in the receiver having risen in temperature, there began to distil a compound fluid, very light specifically, and of a most agreeable odour. Collecting some of this fluid, I found it had a specific gravity of ·699, and that it commenced to boil at 92° Fahr. It was much more manageable for inhalation than the simple hydride; was stable, and acted excellently as an anæsthetic. After carefully testing the vapour of this compound, I administered it fourteen times in cases of extraction of teeth, and with results almost identical with those that followed the administration of simple hydramyl.

Finally, in repeating the process of distillation, the first portion that distilled over was set aside, and none was collected until the temperature had reached 90°, the temperature being sustained between that degree and the degree of temperature of the human body (98°). By this means there was obtained a fluid still very agreeable to breathe and extremely rapid in its action. This fluid has the specific gravity of rectified ether—viz., ·725. It boils in the warm hand, and may be considered as containing one part of bichloride of methylene in nine of amyl hydride. I propose to call this fluid "hydramyl-chlor." I administered the vapour of this fluid for the first time for a surgical operation on July 3. The patient was a young woman, who wished to have a large firm molar tooth extracted. I placed two fluid drachms of the fluid in an inhaler specially constructed for it, and let the patient take the inhaler in her own hand and administer to herself. There was good anæsthesia in forty seconds, and ten seconds later Mr. Matthews took out the tooth without causing the least pain. The recovery was complete within the minute, and was neither attended with vomiting nor nausea. Since this case I have continued to administer the same form of vapour in short operations, and, so far, have every reason to be satisfied with the results. I have administered it to children as young as 4 years, and to adults of different ages up to 70, and of both sexes. Mr. Matthews has also administered it a great many times for tooth-extraction at the Marylebone Dispensary with success equally good.

Method of Administration.—The method of administration I adopt for the hydramyl-chlor is very simple. Messrs. Krohne and Seseman have made for me an inhaler of leather, on the plan of Randall's inhaler for bichloride of methylene. Instead, however, of having the inhaler perforated at the lower part with a number of holes, it is closed there, and is furnished with a light valve for the admission of air. There is also an escape-valve at the upper part. Inside the inhaler is a lining of domette, covered with an inner fold of muslin.

In application I pour one to one and a half fluid drachms of the anæsthetic solution upon the folds of muslin, and apply the inhaler thus charged gently to the face, covering the nose and mouth. The patient is allowed to inhale until there is a movement of the eyelids—a quick restless movement or twitch of the lids, without any turning-up or other movement of the ball of the eye. This symptom developed—and it is the first symptom that attracts attention in most cases—the inhalation may cease, presuming the operation to be performed is one of short duration. At this period the patient, however, is not unconscious, and is not insensible to pain. Owing to the insolubility of the vapour in the blood, a short space of time is required for the fluid to diffuse; and so it happens that, in eight or ten seconds after the inhalation has ceased, the patient is in a better state for the operation than he is *instantly* afterwards. To the dentist this occurrence is of importance, because he is not placed in condition of hurry—he can often tell his patient to open the mouth, or give other short directions, and then proceed to extract; and he has no occasion at all to use a gag to keep the mouth open. In every case the phenomenon of prolonged action of the anæsthetic after the inhalation has ceased should be remembered by the administrator, otherwise he may narcotise far more deeply than he intended. It is merely necessary to watch for the first signs of involuntary movement; if the hand of the patient holding the inhaler become unsteady, or if the eyelids begin rapidly to move, it is time to suspend the inhalation—to wait a few seconds—to operate. I have stated the quantity of fluid required to be two drachms; but not half of this is, in reality, used by the patient. It is necessary to have an excess, because there is considerable and unavoidable loss. For dentists' practice I think the loss may be overcome by having constructed a fixed receiver for holding several ounces of the fluid—a receiver so arranged that, on removing a stopper for the admission of air and exit of vapour, the vapour above the surface of the fluid may be inhaled from a simple mouthpiece. Messrs. Krohne and Seseman are at this time carrying out a design for me of this kind.

So far, in speaking of administration, I have referred chiefly to adults. In the cases of children and old people I make no difference as to the quantity of fluid used in an administration. I am rather guided by symptoms and duration of administration. If a child be allowed to hold the inhaler, I notice that in a few seconds the indications of sleep are usually unmistakable; we have only to remove the inhaler and proceed to the operation.

Hydramyl-Ether.—The composition of what is called compound anæsthetic ether for local anæsthesia has already been given. I should add that a compound ether, which I name *hydramyl-ether*, for both local and general anæsthesia, may be made by adding together in equal portions pure hydramyl and absolute ether. The specific gravity of absolute ether (·720), the vapour density (37), and the boiling-point (92° Fahr.) are all conditions so nearly allied to those of hydramyl, that the two fluids, hydramyl and ether, act in combination practically as if they were one. They differ only in the matter of solubility in water and in blood, the ether being soluble in the blood to the extent of eleven parts in the hundred, the hydramyl being insoluble.

In the production of general anæsthesia, the vapour of hydramyl-ether acts differently to the vapours we have previously discussed. It is slower in its action, it produces some sense of suffocation, it causes a little darkening of the face, pulsation of the vessels of the neck, and a series of disagreeable but not dangerous symptoms, from which the hydramyl-chlor is free. Nevertheless, it is a good general anæsthetic, and, though less agreeable, would be found, in a large number of cases, *safer* to inhale. I have no doubt it will receive favour, especially as it supplies the requirement for both local and general anæsthesia.

Conclusion.—The study of the physiological action of the organic hydrides has afforded me much satisfaction, and many months of, I will hope, useful labour. Whether any of the new applications that have sprung out of the study, and that I have ventured to suggest will find favour with the Profession of Physic, remains to be seen: I do myself no more than submit them for their trial. Knowing how sublime is the folly of assuming that whatever a man may offer is good because it is his to offer, I put forward such results as, by reading from nature, I have gathered, without thought of forcing them on my fellows, or anticipating for them a fraction of unproven general experience. If every other practitioner cannot use the results with profit, as well as I can, they are useless. If every other practitioner can use one of them, and no more, with profit, I have done a work—little as it in itself may be—which, by multiplication of hands devoted to it, is sure to be a good work; and this is all the earnest man ever can do, though his ambition devour him, body and soul.—*Medical Times and Gazette, Oct.* 21, 1871, *p.* 490.

89.—THE ORGANIC BROMIDES.

By Dr. Benjamin W. Richardson, F.R.S.

[The physiological action of bromine itself—the element—is definite and well pronounced. Inhalation of its diluted vapour produces a peculiar constricting action in the vessels which supply the secreting surfaces, so that these become dry and painful. After a time there is what may be called a reaction, due probably to a temporary paralysis of the vessels, and then there follows a free excretion of fluid, what the older writers would designate a flux or salivation, attended with some degree of local insensibility.]

Applied directly, in the liquid form, to the body, and especially to a mucous surface, it acts as a direct destructive of tissue, not precisely as a caustic, but as a substance which leads to shrinking and slow death, with still more determinate local insensibility.

In combination with other elements, as with potassium, its direct action is modified, but not removed. Passing through the tissues in a condition of fine distribution, and probably separating from its ally, it exerts on the nervous matter its special sedative influence, causing, if it be carried far enough, its direct paralysing influence over the vessels which govern secretion, and leading to a certain extent to decreased sensibility of the nerves which govern common sensibility.

On the whole, bromine may be considered as a medicine which acts primarily on the sympathetic or organic system of the nervous system, and as a modifier of vascular tension; and this, whether it be applied locally and directly, or generally and indirectly—*i.e.* in combination.

Thus we may rationally administer bromine with any other substance with which it will enter into chemical form of combination; we may trust to the development of its due independent action, without regard to the action of the substance with which it may be combined, and we may be satisfied that it will not materially interfere with the action of the agent with which it has been made to combine.

Bromide of Quinine.—Bromide of quinine is formed by subjecting the alkaloid quinia to hydrobromic acid, or by acting on a salt of the alkaloid with bromide of potassium. The bromide of quinine is soluble, and, mixed with simple syrup, is ready for administration as a medicine. I prefer to employ it as a syrup containing one grain of it in every fluid drachm. The dose of this syrup is from one to four fluid drachms.

Bromide of Morphine.—Bromide of morphine is made by a similar process to that used for making bromide of quinine; morphine or a salt of morphine being substituted for quinine or a quinine salt. This compound also makes up best in the form of a syrup, and the preparation I prescribe contains an eighth of a grain of bromide of morphine in a fluid drachm of simple syrup. The dose of this syrup is from one to four fluid drachms.

Bromide of Strychnine.—Bromide of strychnine is made in the same way as the two last-named preparations; strychnine, or a salt of it, taking the place of quinine or morphine. This, again, I always prescribe as a syrup, one thirty-second of a grain of the bromide being contained in one fluid drachm of the simple syrup. The dose of this syrup is from one to four fluid drachms.

Combinations.—I am in the habit of sometimes combining the preparations named above, in order to suit particular cases of disease. For example, I combine the bromide of quinine and morphine in syrup, so that each fluid drachm of the syrup contains a grain of the salt of quinine with an eighth of a grain of the salt of morphine; or I combine the three salts, so that the fluid drachm of syrup contains a grain of the quinine, an eighth of a grain of the morphine, and a thirty-second of a grain of the strychnine salt. Speaking generally of all these salts, I may state that, in action, the bromide throughout, in so far as its action is indicated, is eliminative and sedative. I am satisfied the bromide of quinine can be administered freely, when quinine itself, or other salts of it, cannot be readily tolerated. I am equally clear that the bromide favours the sedative action of morphia, while it at the same time allays the astringency which morphia induces; and lastly, I am satisfied from experiment that bromide reduces, or rather subdues and prolongs, the action of strychnine or muscular motion.

Notes on Practice.—I have prescribed bromide of quinine, and the other bromides named, in a large number of cases of disease, and with results I did not fully expect. I will proceed briefly to indicate the leading facts that have occurred to me in the course of observation.

Bromide of quinine simply appears to me to be of good service in cases where certain special and persistent symptoms follow upon syphilis. I hardly speak now of the symptoms which patients themselves connect with that malady, but rather of those insidious symptoms which we, as medical men, who have lived long enough to have seen years of practice, trace back to a syphilitic basis, hereditary or acquired. A case of recurring rheumatism of this nature; a case of recurring ulceration of the fauces; a case of general nervous exhaustion, with flying pains in the limbs, loss of appetite, general debility, loss of hair, and remaining thickening enlargement in the groin, a sequence of bubo; these have been instances in which the administration of the bromide of quinine, in doses of from two to three grains three times a day, has been more immediately and determinately beneficial than any other treatment I have either practised myself, or seen practised by my brethren of physic, in such forms of disease.

One great advantage of this preparation seems to me to be, that it allows one to give much larger doses of quinine than are common, and in frequent and continued doses without setting up the symptoms of headache, oppression, and singing in the ears, which mark what has been called cinchonism. Thus we may give three grains of bromide of quinine, three times a day, without inconvenience for several days if a smaller dose does not suffice.

I have an idea that the bromide of quinine might be administered with advantage in the earlier stages of the contagious diseases, such as small-pox. It would, I think, allay the severe nervous symptoms which usher in these diseases, and so moderate the secondary symptoms that follow in train. Since I began to introduce the bromide into practice, I have not had an opportunity of putting this suggestion to the test, but I have sent some of the preparation to Mr. Marson of the Small-pox Hospital, asking him to give it impartial trial. I have also asked my friend Dr. Broadbent to make trial of it at the Fever Hospital in cases of acute febrile disorders. The results they obtain I shall hope to communicate in a future number of this journal.

Bromide of morphine is a useful addition to the salts of the alkaloid. It seems to me that a smaller dose of the salt than is effective in the case of the other morphine salts produces as distinct a narcotic influence, and also that the dose may be repeated more frequently without producing those after-effects of an opiate which tell against repetition of administration. For instance, in a case of extreme depression of a nervous kind, attended with determinate insania, in which, owing to the headache and nausea it produced, the muriate of morphia had been replaced by chloral hydrate, as the latter remedy had been continued until it had become hurtful, I prescribed the fourth of a grain of bromide of morphia at bedtime with excellent results, producing sleep without production of nausea or other distressing symptom. Knowing too well how apt we are to ascribe an efficacy to new remedies which belongs to other causes, I pen these first impressions on the action of this bromide with all due reserve.

I write, in fact, mainly to secure the larger experience which will ensue when many acute observers are bringing the same remedy into daily use.

The Bromides of Quinine and Morphine in combination constitute a remedy of which, in cases suited for their administration, I cannot speak too favourably. Four classes of disease seem to me to be specially benefited by this compound; viz., neuralgic fever, cerebral irritation, diabetic phthisis, and extreme acute attacks of intermittent pulse, the result of organic nervous shock. In acute neuralgia I administer a drachm of the syrup of bromide of quinine and morphia to an adult every two hours until the pain is altogether removed, and am able to report not only that pains can be effectually removed by it, but that the medicine exerts no derangement of the body that lessens its value. It calms pain without inducing deep narcotism, it interferes little with the secretions, it rarely causes nausea, and it interferes little with the appetite. In the case of an esteemed member of our own profession, who has been for twelve months under my care, suffering from right hemiplegia, the most distressing symptom I have had to meet has been intense sciatic neuralgia. After a run of all narcotic tonic measures, I found happily in the bromide of quinine and iron a remedy which has now for three months held him free of suffering, and, as a consequence of freedom from pain and sleepless weariness, has led to a distinct improvement in his general health.

In diabetic phthisis I have administered the bromide of quinine and morphia with the same freedom. Under its influence, in these cases, the quantity of sugar and of fluid excreted by the urine notably decreases, cough is relieved, the appetite and digesting power is improved, and recurrent hectic is held in abeyance more certainly, I think, than by any other remedy or combination of remedies with which I am practically conversant.

In a case of intermittent pulse, where the lapse in the heart-stroke was painfully frequent, where there was continued feverish restlessness, and a fear of going to sleep that more than all sustained the irregular nervous action, the symptoms gave way at once under a few doses of bromide of quinine and morphia in a manner that was as gratifying to the prescriber as to the patient. The purpose of the medicine, in a word, was promptly fulfilled, and as demonstrably as if it had afforded mechanical instead of therapeutical relief. In a second case of intermittent pulse, where the intermittency is the prelude of great mental excitement, followed by depression and melancholia, the remedy has exerted a similar beneficent influence. It induces rest and sleep without the production of deep narcotism and without deranging digestion.

The Bromide of Strychnine has rendered unquestionable service in a few cases of dyspepsia with and from deficient nervous control over the vascular supply of the organs concerned in the process of digestion, in cases of partial organic nervous paralysis of the ventrical division of the organic nervous system. In such cases of disease, and they are by no means uncommon, where, when the body is without food, there is a knowledge of hunger without the true sense of it; when there is congestion of liver, and suppressed secretion to-day, accompanied by giddiness and irritability and præcordial oppression with diarrhœa to-morrow, and then constipation: in these cases the bromine of strychnine in the proportion of one thirty-second of a grain may be given three times daily with marked advantage, an alterative being at the same time occasionally added.

In some mixed cases of nervous pain, with want of organic nervous action in the digestive organs, I have combined the bromide of strychnine with bromide of quinine, and in many cases of this nature I have prescribed the three bromides with good result.

Syrup of the bromide of quinine and strychnine, and syrup of the bromide of quinine, morphine, and strychnine, will both, I believe, become favourite compounds with the profession, finding their place as Eastin's syrup of the superphosphate of iron, quinine, and strychnine has found its place in the list of tried and approved medicaments.

One other point of practice remains to me only to note. In cases where there is much dryness and irritability of the mucous membrane of the pharynx and larynx, the bromides *are not* commendable; the bromine increases the irritation. This was so marked in a case where there was a small ulcerated surface in the larynx, that I had to stop the administration altogether, the smallest dose producing violent and long-continued irritative cough and spasm.—*Practitioner, June*, 1871, *p.* 337.

90.—CHLORAL HYDRATE: ITS USE IN LUNACY PRACTICE ILLUSTRATED.

By Dr. N. G. Mercer, Senior Assistant Medical Officer County Lunatic Asylum, Lancaster.

The addition of an hypnotic agent of high value to the resources of our Materia Medica is obviously of the first importance in the medical practice of asylums. Apart from the mere curative power exerted by drugs which are administered with the view and effect of producing sleep, it soon becomes apparent in an intercourse from year to year with the society of a large asylum, that the comforts of the inhabitants are promoted in a wonderful degree by making their noisy neighbours of peaceful habits, especially during the night. It will scarcely be considered a slight upon some highly respectable soporific medicines, which have been erewhile recommended and employed from time to time, if we summarise the results of experience by saying that, when contrasted with opium and its preparations, their utility dwindles into comparative insignificance. It would be out of place here to dilate at length on the precious service rendered by opiates in the many varieties of mental infirmity. Let me, however, make one observation, which must be familiar to all asylum practitioners,—viz., how great is the change for the better produced in the general comfort of a ward where a patient of particularly, or continuously noisy habits, lives, by the administration to that patient of morphia at intervals. Under its influence clamorous excitement subsides; a disposition to quarrel is for a time completely subdued; obscenity, execrations, and abuse, no longer constantly offend the ears of convalescent and of sensitive neighbours; and it is well understood that a temporary tranquillity of mind, in a person with such troublesome tendencies, is attended (for the quieter inmates of the ward) with a transition into a state of comparative Elysium. These remarks especially apply to the female wards, and it is improbable that any remedy will be found to supersede opium in achieving such results. Unfortunately, however, the sedative effects of the drug become less and less marked by repetition of the dose, and it requires to be increased before they are developed; ultimately the medicine is for a time abandoned, so that its action may possess increased impulse when resumed. The inconvenience felt from first having to enlarge to a great extent the dose of opium, and, again, from having to intermit its administration on account of the negative results, has been especially experienced when a full hypnotic action has been sought, and the medicine is given in the form of a draught at bed-time. This leads me to observe that, used with this object, the chloral hydrate appears to possess certain advantages. Thus, suppose thirty grains of the latter medicine be given in a draught to a restless and sleepless patient, and a good night's sleep follow its exhibition, we shall find that the drug does not lose its potency by repetition; as, after the lapse of a few weeks, during which time it has been regularly taken, the same quantity of dose will produce effects as marked and as happy as when first administered. Although prone to view with suspicion any medicine claiming to rival opium, I have been compelled, by experience of the virtues of choral hydrate in practice, to attribute to it this advantage and superiority over any opiate as regards the effects of an uniform dose.

[Dr. Mercer then relates eleven cases, and concludes as follows:]

From a study of these cases the following conclusions on the virtues of chloral hydrate given as an hypnotic draught seem fairly deducible:—1. In very many bad cases of noisy habits during night it is a powerful and efficient hypnotic, and more certain than an opiate. 2. The effects of the drug do not fail of development through long continued use of it in an uniform dose. 3. In the most intractable cases of noisy habits it is probably of less service than an opiate—such cases, it would seem, as those of intensely maniacal and excited patients who in a previous attack of mental derangement presented the symptoms of melancholia.

Given thus, then, as an hypnotic draught, the chloral hydrate appears entitled to a high position in the future of lunacy practice. As the progress of recovery proceeds apace with the establishment of sound sleep, it is obvious how, as a therapeutic agent, its effects must be in a very large degree curative and remedial. This remark applies to attacks of acute disease. But surely, in the chronic insane, that mental agony produced by the constantly-returning presence of delusive voices and visions in the long nights of maniacal clamour and excitement is an object scarcely less worthy of attack and of relief than is the sensation of pain in those who are of sound mind. The collateral happy results of this artificial rest are not to be measured except by those who are themselves the nervous, timid, and excitable neighbours of patients whose constant habit it has been to "make night hideous" by their cries.

The eleven cases recorded here are selected as being some of the earliest which were used to test the efficacy of chloral hydrate in this Asylum. But it has been largely employed in a great number of other cases, including some of acute mania, of hysteria attended with suicidal impulses, and of general paralysis, accompanied as it so often is, with dangerous and distressing restlessness. It is an anxious matter to remove a suicidal patient from a dormitory to a single room, because in the latter, with all the means at our disposal, a patient bent upon self-destruction might probably concoct a plan to compass the ghastly purpose. A medicine which will enable such a patient to pass tranquil nights among watchful neighbours in an associated room is one whose precious service cannot be over-estimated. I have not been able to verify the observation, put forth upon respectable authority, that the hypnotic effects of chloral hydrate are sometimes postponed for twenty-four hours; that a draught given to-night, for example, will not affect the patient's sleeplessness to-night, but will cause it to vanish to-morrow night, without repetition of the draught. Should such apparent connexion between supposed cause and effect arise, it seems to me it would be very difficult to establish its reality unassisted by imagination.

It only remains for me to add that any attempts which I have made to substitute chloral hydrate for opium as a partial sedative by day have been attended with negative results. I give up using it for this purpose, as morphia was found far more useful. Thus, to a restless, meddling, and noisy general paralytic, aged 30—puerperal case—who is in the third stage of her disease, twenty-five grains of chloral hydrate were given at intervals, but her excitement was not subdued until she had full doses of morphia. The chloral has made some patients sleep in the day-time, but has produced no tranquillising influence during their waking hours.—*Medical Times and Gazette, April* 22, 1871, *p.* 450.

91.—CHLORAL IN TOOTHACHE.

By Dr. David Page, Kirby Lonsdale.

For some time past I have been in the habit of using chloral hydrate, not only as an internal sedative in cases of severe dental neuralgia and of caries where it was at the time inadvisable to permit extraction, but more frequently as a direct local application to the carious tooth. A few grains of the solid hydrate, introduced into the cavity of the tooth upon the point of a quill, speedily dissolve there; and, in the course of a few minutes, during which a not unpleasant warm sensation is experienced, the pain is either deadened, or more often effectually allayed. A second or third application may be resorted to if necessary. The hours of agony that various circumstances may otherwise entail are in this way avoided; and, in the last instance under my notice, a young lady in a delicate state of health, suffering intensely from a carious tooth, received immediate relief from this application. She lived at a great distance from a dentist, and was unwilling to have the tooth extracted without some anæsthetic; and the chloral hydrate was the means of obtaining a sound night's rest. I find in my notes her own report that "the toothache was relieved in a few minutes; aching did not return all night; and when it did in the morning, was again stopped by the same means.—*British Medical Journal, Sept.* 2, 1871, *p.* 262.

92.—DIAGNOSIS OF STRICTURE OF THE RECTUM BY THE SEX.

[The following is taken from a report of Mr. Hutchinson's clinique.]

A case of stricture of the rectum called forth the remark that, speaking roughly, it was sufficient to ask the sex of the patient to ascertain the nature of the stricture, whether malignant or innocent; malignant stricture being almost limited to men, whilst in women the disease was as frequently innocent, syphilitic, gonorrheal, or following parturition.—*Medical Times and Gazette, July* 1, 1871, *p.* 8.

93.—VULCANITE URINOMETERS.

At the suggestion of Mr. Pollock, Messrs. Blaise, of St. James's Street, have turned their attention to the adaptation of vulcanite to the manufacture of urinometers, hydrometers, alcoholimeters, saccharometers, and other instruments for indicating the density of liquids; and they have found, it seems, after repeated trials and tests, that it is better suited to the construction of the above-named instruments than either glass or metal.

Vulcanite is perfectly proof against all kinds of acid, except bisulphide of carbon. Instruments made with it, therefore, do not get out of order or break, if used with ordinary care; whereas slight taps will often break those made of glass, or a fall will injure those made of metal, which are likewise apt to corrode. The vulcanite instrument takes its gravity much quicker than one made of glass or metal, and is cheaper and more durable than either.

We think the attention of the profession may well be directed to the differen instruments prepared in vulcanite.— *Lancet, Oct.* 28, 1871, *p.* 612.

SYNOPSIS

(ARRANGED ALPHABETICALLY) CONTAINING

SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE HALF-YEAR.

AFFECTIONS OF THE SYSTEM GENERALLY.

AGUE.—It often happens that we meet with cases of ague which have been unbenefited by several weeks of quinine taking. In most of these cases what s wanted is relief to the engorgement of the internal organs. Two or three grains of colocynth and rhubarb in combination with quinine will often at once cure the disease. (Dr. H. W. Fuller, p. 44.)

CANCER.—That cancer is to be regarded as a blood disease requires stronger evidence than has yet been adduced. It is really a parasitic disease in its nature. To a certain extent a constitutional tendency must be admitted, just the same as there is to sebaceous tumours and warts, but no one ever thinks of calling these the results of a blood disease. (Mr. C. De Morgan, p. 33.)

Cancer flourishes best when there is plenty of good blood, and grows very slowly when the blood is poor. If a patient with cancer of the uterus is kept on the sparest vegetable diet, just enough to keep body and soul together, the disease will be arrested and may disappear, as it has been known in one well-authenticated case to do. The antagonism between tuberculosis and cancer may be explained by the fact that tuberculosis is associated with waste of tissue. (Mr. C. De Morgan, p. 40.)

GENERAL DEBILITY DEPENDING ON DEFICIENT ELIMINATION.—There are many cases of so-called debility, the symptoms being chiefly extreme languor, weakness, and total loss of appetite. They are treated with tonics, chalybeates, and good food with stimulants—but without success. The fault is our not examining the secretions with sufficient care. The tongue may be clean and the bowels said to be regular, but the urine will be scanty and loaded with lithates, and on examination the motions will prove to be white, lumpy, and offensive. The fact is that the patient is oppressed with the presence in the blood of matters which ought long since to have been eliminated, but which the inactivity of the secreting organs has caused to be retained in the system. If a few doses of calomel and colocynth are given the urine will become abundant and pale, and the motions normal in character, and the tonics which previously did harm will now rapidly effect a cure. (Dr. H. W. Fuller, p. 45.)

GERM-THEORY OF DISEASE.—If the germ-theory of disease is believed, it is not on bad air or foul drains that the attention of the physician will primarily be fixed, but upon disease-germs which no bad air or foul drains can create, yet which may be pushed by foul air into virulent energy of reproduction. (Prof. Tyndall, p. 29.)

INTERMITTENT FEVER.—*Sulphate of Quinine.*—In treating simple intermittent fever by quinine we must take into account the intensity of the cause, also the duration of the complaint; for the more intense the first dose of the poison, and the older the case, the stronger must be the dose of quinine. We must also consider the type of the fever. Thus quartan fever requires a much larger dose of quinine than the quotidian or tertian type. Again, if we find the spleen enlarging we must at once increase the dose of the remedy. By attending to these rules our treatment of intermittents will be much more successful. In a general way the suitable dose of quinine varies between five and twenty-three grains. The *minimum dose* should be administered in recent cases of simple intermittent fever unattended by tumefaction of the spleen; the maximum in old cases of intermittent accompanied by considerable enlargement of spleen. The maximum dose must be commenced with immediately we see the case and continued for four or five days, when it may be decreased. Quinine is best tolerated by patients who are properly fed. (M. A. Monat, p. 27.)

Osteo-Arthritis, or Rheumatic Gout.—This disease results from a form of mal-assimilation in some way connected with a depressed state of vitality. The causes which produce it are all of a depressing nature. There is a remarkable antagonism between this disease and albuminuria—though the two never occur together, whilst in chronic gout the presence of albumen is not uncommon, and when chalk stones are present is almost constant. This is not unfrequently a valuable aid in the diagnosis, and even supposing albumen is not present in chronic gout the specific gravity of the urine is always below the average, whilst in rheumatic gout it is normal. The disease, on the other hand, is more liable to occur in members of consumptive families although the individual himself is almost exempt from liability to consumptive diseases. Rheumatic gout must not be treated with colchicum and alkalies, which would depress and aggravate the disorder. Even in the acute form when the patient declares it is agony to move, and is bedewed with perspiration, he must be ordered to leave the bed, and if that does not check the perspiration the cold douche should be used for that purpose. Mineral acids with vegetable tonics, cod-liver oil, and a generous diet must be prescribed, and if the urine remains clear after standing, iron should be given, and a fair quantity of wine, beer, or porter. (Dr. H. W. Fuller, p. 49.)

Rheumatism and Gout.—It occasionally happens that during the exhibition of alkalies in cases of rheumatism or gout, and whilst there is a certain amount of alkalescence of urine, we have a copious deposit of phosphates. This is owing to depression, and is sure to be accompanied or followed by other symptoms of a corresponding kind. It is not necessary as might be supposed to give up or diminish the amount of alkali, for a few grains of quinine given daily will at once diminish the deposit, and enable the patient to persevere with the alkali. (Dr. A. W. Barclay, p. 95.)

Typhoid Fever.—*Mode of Propagation of.*—There is nothing to prove that typhoid fever is infectious by breathing air contaminated by the presence of a typhoid patient. Nor has it yet been proved that the poison is contained in, or disseminated by, the exhalations from drains, privies, &c., however the breathing of air contaminated by such exhalations may predispose to the disease. *Almost* invariably the disease proceeds from a special poison contained in the alvine excreta, which is *directly* introduced into the alimentary canal either in the food, or, most generally, in the water drunk. Thus the physician, after examining the rose spots may in the course of a few minutes unconsciously pass his hand across his lips. The nurses do not observe always that scrupulous cleanliness so necessary in such cases, and thus the fever may be disseminated through a family by partaking of a meal. If there is any typhoid in a district, no surface-well water or pump water should be used at all. (Dr. P. W. Latham, p. 25.)

Relapses in Typhoid Fever.—Relapses almost always occur from giving solid food too early, that is before the ulcers in the ilium are healed. We have, however, in the thermometer an infallible guide. If the temperature of the patient at 8 a.m. and 6 p.m. has remained for two days at least about the normal point, or between 98° and 99° Fahr., solid food may be given, but not without. The tongue may be clean and moist, the appetite ravenous, yet if the temperature is not permanently at 98° the ulcers are still unhealed. (Dr. P. W. Latham, p. 27.)

AFFECTIONS OF THE NERVOUS SYSTEM.

Hyperæsthesia.—Patients of the quasi-hysterical class, both male and female, are liable to suffer from hyperæsthesia of portions of the surface of the body, or even of the whole. The sensitive portion of skin will not bear the least touch; such a pressure as that of a stethoscope in auscultation would be intolerable. The reality of such suffering is materially confirmed by the fact that, in some cases of structural disease of the cord, certain parts of the cutaneous surface manifest intense reflex excitability, though ordinary sensation is much impaired. The throat, stomach, and even the tracheal and bronchial mucous surfaces are liable to hyperæsthesia. The heart enjoys no exemption. Cases are not at all rare where there is more or less præcordial uneasiness or pain, with rapid action of the heart: such cases are generally seen in young persons. What is known as "irritable uterus" is essentially a neurosis. The female breast and the joints are particularly liable to this affection, which is undoubtedly a condition of imperfect nutrition, not an exaltation but a deterioration of vital power. (Dr. C. H. Jones, p. 50.)

Neuralgic Headache.—*Turpentine.*—Turpentine in doses of one or two drachms, given in a little cold water, is an excellent remedy in cases of severe neuralgic headache, occurring in young persons of a delicate excitable temperament, without any menstrual or leucorrhœal complication. It is also of great use in the frontal headache, liable to occur after prolonged mental effort in either sex. A cup of very strong tea often relieves this form of headache, but it may produce general restlessness, and, worst of all, banish sleep. (Dr. J. W. Begbie, p. 65.)

Toothache.—*Chloral.*—A few grains of the solid hydrate introduced into the hollow cavity of a carious tooth will in most cases relieve toothache rapidly. A second or third application may be resorted to if necessary. (Dr. D. Page, p. 265.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

Acupressure.—In ordinary circumstances, when acupressure has been skilfully performed, vessels of comparatively small size, such as the facial, temporal, radial, and ulnar, may be safely freed from acupressure in eight hours; and the larger arteries, as the humeral, axillary, and femoral, in twenty-four. The late Dr. Keith and Mr. Pirrie have altogether compressed upwards of two thousand arteries, and in only two cases has hemorrhage occurred on removal of the pin, and in each case the artery was again acupressed in a few seconds. It is proved by dissection that the occlusion is effected by adhesion of the internal surfaces of the artery, and an internal coagulum more or less adherent to it. Union by the first intention is the aim of every surgeon, where accurate coaptation of surfaces and edges can be maintained. The superiority of acupressure over all other means of arresting hemorrhage yet devised consists in the removal of all local hindrance to early union within twenty-four hours of the operation. (Prof. Pirrie, p. 138.)

The Three Methods of Acupressure.—It was proposed by Mr. Pirrie, and agreed to by Sir James Simpson a short time before his death, that as only three methods of acupressure are necessary, these three methods only should be recommended by the friends of the operation, with the object of accelerating its more general adoption. These methods were named by Sir James Simpson, Circumclusion, Torsoclusion, and Retroclusion. *Circumclusion* is the well-known plan of a loop of fine wire passed over the point of the needle, and then, after compressing the artery, secured by twisting the ends round the needle. In *Torsoclusion*, a piece of tissue is taken up by inserting the pin close to the mouth of the bleeding artery, then pressing it onwards in a direction parallel to that of the vessel, and then causing its point to emerge. The second step consists in giving a quarter rotation to the pin, so as to place its emerged extremity above and at right angles to the artery; in pressing it well down against the small portion of tissue between the instrument and the vessel; and in sending the point for some distance into the tissues beyond the artery, for the purpose of securing the pin in position and of maintaining the twist. The artery is securely compressed by this plan, which may be described as the simplest, the easiest, and the quickest. *Retroclusion* is so named because the pin passes ultimately behind the artery. The pin being held almost flat upon the surface of the wound, is made to take up a piece of tissue of small size, and the point then to emerge before reaching the artery. The head of the pin is then made to describe the greater part of a semicircle, so as to be placed nearly flat on the opposite side of the wound, its point is then sent behind the artery in the contrary direction to that in which it passed in the first movement, and pressed on until it is fixed in the tissues behind the vessel. (Prof. W. Pirrie, p. 137.)

A New Method of performing Acupressure.—The acupressure needle must be modified thus: About an inch from the point it must be twisted on itself, so as to form a small loop, and there must be another loop at the blunt end. The needle as a whole, is straight, except for the two loops. A piece of fine wire is then doubled in the middle and passed, first through the ring at the end of the pin, then through the loop on the staff. After sufficient has passed to reach easily over the point of the needle, the loop of wire should be bent up at right angles, so as to be out of the way. The needle is then ready for use. In using it, the point is first passed under the vessel, and then the loop of wire is bent down over the vessel and slipped over the point of the needle. By pulling on the free ends of the wire, and then twisting them round the needle close to the ring, the vessel is secured. To remove the wire ligature first untwist it, then pull a little on the pin point, and the loop of wire will glide along towards the point, and release the vessel. Should bleeding now take place, the vessel may again be compressed by pulling on the wire. After removal of the pin the wire follows. (Mr. R. C. Lucas, p. 141.)

ARTERY CONSTRICTOR.—*A new mode of Arresting Hemorrhage.*—In a case of amputation of the arm for injury, at Guy's Hospital, Mr. Bryant, instead of securing the artery by ligature or by torsion, which is the method chiefly employed at this hospital, used an instrument which had been brought to his notice by Dr. Fleet Speirs, of New York. This instrument is called the artery constrictor, and consists of a flattened metal tube, six inches, more or less, in length, open at both ends, with a sliding steel tongue running its whole length and having a vice arrangement at one end, and a hook-shaped depression at the other, by which the artery can be grasped. It is then made to contract upon the artery by means of the vice at the upper end, which forces it within the sheath. In principle its action is the same as that of torsion, but the integrity of the external coat is more thoroughly preserved, while the continuity of the vessel is maintained. (Mr. T. Bryant, p. 126.)

DIGITALIS.—*Its Action on the Heart.*—Digitalis used to be considered a cardiac sedative, because it was found to relieve palpitation, which was thought to be over-action of the heart. It is now known, however, that there is no such thing as over-action, that hypertrophy is rather evidence of deficiency than of excess of power. It consequently follows, that if digitalis relieves palpitation it must be by a tonic action on the heart. It is in cases of deficient expulsive power of the heart walls that digitalis is chiefly of use; where the heart is distended, and in contraction only gets rid of a little blood off the top, remaining more or less full in systole. Under digitalis the contraction of the ventricles becomes complete, and the pulse consequently steadier, firmer, and less compressible, the system is relieved, dyspnœa is abated, the deficient secretion of urine is improved, and free secretion takes its place. Dropsy is thus often relieved. (Dr. J. M. Fothergill, British Medical Journal, July 8, p. 27.)

The Use of Digitalis.—For persistent use digitalis should be given in powder. A favourite pill is the following: Half a grain to a grain of powdered digitalis, with an equal quantity of the dried sulphate of iron in powder, and a morsel of cayenne in extract of gentian or aloes and myrrh pill. Thus we secure at one cast an action on the circulation, the addition of iron, in a form which will act locally on the stomach, and thus act as an astringent on the gastric catarrh so common among the sufferers from heart disease. (Dr. J. M. Fothergill, British Medical Journal, Aug. 5, p. 148.)

DISTRAIN OF THE HEART.—By "distrain of the heart," is meant a sudden *dilatation of* the heart without previous disease. That the heart may dilate slowly from long continued and excessive exertion is popularly recognised, but that it may yield suddenly, and in all directions, has not until recently been thought possible. Such, however, is the case, and with rapidly fatal results in most cases. The history is generally that, after severe exertion, intense pain occurs in the region of the heart, or in some part of the abdomen (very liable to mislead in diagnosis.) The skin is at the same time cold and clammy, the face blue and congested, and the patient in a state of collapse. The pulse is small and rapid, and the heart's sounds extremely indistinct. After death the heart is found dilated, both cavities full of black blood, and both auriculo-ventricular apertures much enlarged. Should the patient recover, perfect rest is the most important remedial agent, and those medicines which quiet the action of the heart, such as conium, henbane, and belladonna, rather than digitalis, which is of little service. (Dr. R. Thompson, p. 78.)

HEART DISEASE.—*Heart disease from Strain in young people.*—A great number of cases of heart disease occur in large towns from strain and overwork amongst young people. These cases are quite unconnected with rheumatism, which has hitherto been considered almost exclusively the cause in early life. The actual pathological change produced is dilatation of one or both chambers of the heart, accompanied or not with hypertrophy, according to the general health and nourishment of the patient. If the heart overtaxed be a full one, we find dilatation of both chambers. Prolonged rest will do a great deal for these cases, if the disease has not gone too far—but such hearts recover very slowly. After a time tricuspid regurgitation becomes established; this is as truly a disease as mitral regurgitation; for Dr. King's safety-valve hypothesis must not be believed in. When the right auriculo-ventricular orifice is once weakened, it is generally permanently so, for whenever the venous reflux in the external jugulars has been distinctly visible, it is always easily recalled in the same persons after long periods of rest. (Dr. T. C. Allbutt, p. 72.)

Disease of the Aorta from Over-exertion and Strain.—When a man is habitually engaged in a very laborious exertion, involving great strain on the heart and great vessels, the heart is able to accommodate itself to it by muscular growth, but with the aorta the case is very different; its resistance is great but its activity is nothing, or rather it is nothing more than the recoil of elastic fibre. Strain of such coats as these, so far from bringing gain of strength, brings loss of elasticity and weakness. Slight dilatation, with the occurrence of points of endo-arteritis, is the first change which takes place. From this point, pouching of the aorta, with consequent or concomitant incompetence of the valves, is quickly established. Such cases form the staple of heart diseases in the younger male patients of a hospital of a manufacturing town,and are totally unconnected with atheroma as a cause. (Dr. T. C. Allbutt, p. 77.)

State of Secretions in Heart Diseases.—A case of heart disease is related in which salines, with digitalis, squills and other diuretics, had failed to give relief to the increasing anasarca, with scanty urine. On admission to hospital, it was found that the stools were extremely pale in colour, indicating a deficient action of the liver, and of the secreting apparatus of the bowels. Five grains of the compound digitalis pill was prescribed, containing three grains of blue pill, a grain of squills, and half a grain of digitalis, three times a day; and within three days the patient had begun to improve. By the end of the week the motions were a healthy colour, and then, the medicines which were useless before produced their proper effect—viz., a free secretion of urine, with great relief to the dyspnœa and anasarca. The case shows the great importance to successful treatment of examination of the secretions. (Dr. H. W. Fuller, p. 44.)

HEMOPTYSIS.—*Subcutaneous Injection of Ergotine.*—The author relates a case of hemoptysis which he treated by subcutaneous injection of five grains of ergotine dissolved in ten minims of distilled water. The hemorrhage returned twice subsequently. On all three occasions it was at once arrested by the ergotine. (Dr. W. A. Jamieson, p. 84.)

HEMORRHOIDS.—A gentleman of good health suffered severely from hemorrhoids. Being told that if he starved himself he would obtain a cure without any operation, he did so. For weeks he kept himself at starvation point, o nearly so, and the tumours all became absorbed—nor has he been trouble with them since. (Mr. C. de Morgan, p. 40.)

LIGATURE.—*New Method of Ligaturing Bleeding Vessels.*—There is no doub that ligature is by far the safest method of arresting hemorrhage from a d vided artery, inasmuch as a ligatured vessel will resist the pressure of a colum of mercury 114 inches high, whilst those closed by acupressure or torsion on resist a column 23 and 13 inches high respectively. Still acupressure and to sion have the great advantage of not necessitating the retention in the wou of a body directly provocative of suppuration. The safety of ligature may, ho ever, be combined with the advantages of the other two methods by the use a peculiar kind of knot. This knot, whilst it is perfectly secure, only requi traction on the end of the ligature to allow of its complete and easy withdraw from the wound. The knot will be at once understood by reference to t woodcut at p. 143. (Dr. A. Ogston, p. 142.)

LIGATURE OF THE SUBCLAVIAN ARTERY.—*A New Incision.*—In the ordinar operation for ligature of the subclavian artery the incision is made close to and parallel with the clavicle, and the guides to the artery are the edge of the scalenus anticus and the tubercle on the first rib. But unfortunately it is often impossible to feel the edge of the muscle in a bleeding wound, and the tubercle on the first rib is not always so well developed as to permit of recognition by the sense of touch. The only real guide to the artery is the lower edge of the omo-hyoid muscle. Make the incision, beginning at the posterior edge of the sterno-mastoid muscle, one inch and a quarter above the clavicle, and ending within a quarter of an inch of the attachment of the trapezius to that bone, dividing skin and platysma. This incision should be a little short of three inches. The external jugular should then be ligatured in two places and divided, the deep cervical fascia then divided, and the posterior belly of the omo-hyoid will appear on retraction of the edges of the wound upwards and downwards. The white cords of the plexus, with the artery inferior and internal to them, will be seen to occupy the bottom of the wound. The knife is then to be laid aside, and the vessel ligatured in the usual way. (Mr. F. P. Staples, p. 145.)

POPLITEAL ANEURISM.—*Compression.*—The compressing power, whatever it is, provided that it just arrests the circulation in the vessel, and is elastic, should always be commenced at 9 a.m., and left off during the night, so as to allow of uninterrupted sleep, whereby the health of the patient is kept up to the condition most favourable for fibrinous deposit in the sac. Any force employed exceeding that which would be represented by a *dead* pressure of nine pounds is too great, as the pain to which it will give rise will soon render it intolerable. (Dr. R. Macnamara, p. 146.)

VALVULAR MURMURS.—In giving an opinion as to the prognosis of a case of heart-disease accompanied with valvular murmur great care is necessary. We are very liable to be misled if we take the murmur as our sole guide, both as to the presence of disease and as to its real importance. In the first place functional murmurs may occur at the apex as well as the base of the heart, and be so intense as in the closest manner to make the case resemble serious organic disease. Apart from the general history of the case the position in which the apex of the heart strikes the chest wall is our best guide, for if there is valvular lesion hypertrophy results, and the point of impulse is considerably lowered. It is wonderful also to what an extent the lesions upon which some murmurs depend admit of repair. Reparative action takes place, very slowly however, provided that the heart has rest. (Dr. H. W. Fuller, p. 81.)

VARICOSE VEINS.—*Application of strong Nitric Acid.*—Varicose tumours may be successfully treated as follows:—Suppose for example, that the tumour is on the leg. Pressure having been made above and below the tumour, the integuments must be raised from it, and an incision by transfixion made in the skin so as to expose the tumour. The fuming nitric acid is then to be applied to the external coat of the vein. No pain will attend this application. On the following day the contents of the tumour will appear solidified, and the acid may be applied again. The process of solidification will then go on rapidly, the tumour at the same time decreasing in size. In the course of a week a small slough will come away, leaving a wound which may be dressed with tinct. benzoin co. and glycerine. This practice was suggested by Sir Dominic Corrigan, to whom the idea occurred from the success which generally attends the treatment of hemorrhoidal tumours by nitric acid. (Mr. Stokes, p. 148.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

Asthma.—*Nitrite of Amyl.*—Nitrite of amyl is a remedy of the greatest value. Its importance has not yet been recognised. It should be given by inhalation in doses of five minims, on a piece of lint laid over the nostrils. It is a paralyser of the organic nerves which control the size of the blood-vessels, which, consequently, under its action dilate from the pressure of blood within. It is also highly probable that it acts upon the unstriped muscular fibre of the body, generally producing relaxation. The first symptom of its action is acceleration of the action of the heart, or flushing of the face. There is also warmth of head, face, and neck, and perspiration: the warmth and perspiration often being general. The pulse is the first tell-tale of its effects: in from eight to twelve seconds its frequency rapidly increases, even to double its ordinary rate. The beating of the heart and carotids is in some persons very marked. Its action in a case of spasmodic asthma is magical, and it may also be used with advantage in cardiac dyspnœa, angina pectoris. It seems likely to be of great service in attacks of laryngismus stridulus. It may be inhaled directly from the bottle neck. (Dr. T. Jones, Practitioner, Aug. 1, p. 213.)

Chronic Bronchitis.—If a little cotton wool is placed in front of the mouth, and slightly moistened to make it adhere, it will be found to prolong sleep, abate the irritation of the throat, and greatly mitigate a hacking cough. (Prof. Tyndall, p. 32.)

Paracentesis Thoracis.—As soon as ever it is ascertained that the absorption of pleuritic fluid is improbable, it is better to draw it off at once, otherwise the lung becomes bound down permanently by adhesions, or the fluid becomes purulent, and the patient worn down by fever and hectic. The admission of a little air during the operation, whatever it may be in theory, is found in practice to be of no consequence whatever. The suction trochars and other contrivances of a like kind are quite unnecessary. The chest should be tapped as low down as possible, and a free opening should be made, so as to allow of the chest emptying itself thoroughly. If the fluid is found to be purulent, some means should be adopted to prevent the wound from closing, in order that the matter may drain off as soon as it is formed. The patient should be supported after the operation by bark and good nourishment, and for a day or two opium should be given if necessary. (Dr. H. W. Fuller, p. 151.)

Ulcerated Throat and Cavity in the Lung.—*Inhalation of Iodine.*—The readiest and most effectual means of applying this practice is to use a solution of twenty grains of iodine to the ounce of hydride of amyl (a solution of great service in other cases), and to dilute a portion of this with more of the hydride until the vapour of the iodine given is scarcely at all irritating to the throat. This solution may be inhaled from a little funnel of parchment paper, holding in it some finely-teazed cotton-wool, on which the solution may be dropped. It should be held a little way from the nostrils and mouth, so as to allow the admission of plenty of fresh air. The quantity of solution used should be so regulated that about five grains of iodine are inhaled at a time. (Dr. B. W. Richardson, p. 252.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

Cholera.—If cholera invades this country the main objects for endeavour must be to secure such local circumstances that cholera-contagium, though not disinfected, shall be unable to act extensively on the population. There are two dangers which have to be guarded against as favouring the spread of contagium. First, and above all, there is the danger of water supplie are in any (even the slightest) degree tainted by house refuse or oth kinds of filth; and secondly, there is the danger of breathing air whicl with effluvia from the same sorts of impurity. Happily for mankind, is so little contagious that, if reasonable care be taken, there is but li that the disease will spread to those who nurse and attend the sick. matters, however, which the patient discharges from the stomach and are infective. They should be immediately disinfected, or their power of ing the disease increases more and more for some days. The infectiv of choleraic discharges likewise attaches to whatever bedding, clothes, have been imbued with them. (Mr. Simon, Lancet, Aug. 19, p. 279.)

Calabar Bean.—Calabar bean has the peculiar effect of causing dilat the minute arteries and the capillaries. This appears to be the ver wanted in cholera, in which these vessels are empty and contracted, th being driven into the veins. It not only opposes the cholera poison particular, but it raises the temperature of the external parts of th which are depressed in that disease. It should be given by subcutan jection, in doses of $_8$th grain of the extract every two hours. This treatment is simply a suggestion, as it has not been put to the test of but as it would not interfere with other treatment, there seems no reas it should not be. (Dr. Munro, Edinburgh Med. Journal, Oct., p. 327.)

COLLAPSE OF CHOLERA.—*Nitrite of Amyl.*—It is now pretty clearly that the collapse of cholera is owing to an impediment to the circul blood through the lungs by the contraction of the muscular fibres of nute pulmonary arteries. This is just the condition which nitrite of most calculated to relieve. It gives instant relief in asthmatic colla its administration invariably causes increased frequency of cardiac p dilatation of the arterioles, flushing of the face, warmth of body, and pers It should be administered by inhalation. Its effects have yet to be pu test of experience in cholera, but, so far as we can judge from our knowledge of its action in other diseases, it is likely to prove of grea (Dr. Talfourd Jones, Brit. Med. Journal, Sept. 30, p. 378.)

DYSPEPSIA.—We occasionally meet with cases of dyspepsia in whic dies ordinarily found efficacious are of no benefit whatever. There is waterbrash, flatulence, drowsiness after meals, and restlessness at nigh bowels act regularly, but the motions are pale and lumpy, or else dark ed and offensive, and the urine is scanty and loaded with lithates. Th the remedies for dyspepsia are inoperative is, that the secretory appa out of order. In such cases as this a few doses of calomel, combin opium if necessary, will unload the congested liver, and in a few days change for the better, which cannot otherwise be brought about in weeks. (Dr. H. W. Fuller, p. 45.)

STRICTURE OF THE RECTUM.—It is almost sufficient to know the se patient to determine whether the stricture of the rectum is malignant cent. Malignant stricture is almost limited to the male sex. (Mr. J. son, p. 266.)

AFFECTIONS OF THE URINARY ORGANS.

DIABETES.—*Ammonio-Saline Treatment.*—It has been found, by ana diabetic blood, that there is a great deficiency of certain alkaline salts. salts are absolutely necessary in order that the sugar which is formed ease, just as in health, should be burnt off at the lungs. M. Mialhe, covered the above fact, considers this deficiency the primary caus diabetes. Whether this is so or not, there is no doubt that such de must react upon the disease. Accordingly, treatment directed to sup *deficiency is likely* to prove of service, and in actual practice such is f be the case. The best saline mixture is composed of carbonate of ammonia, ten grains; phosphate of ammonia, ten grains; carbonate of soda, ten grains; tincture of ginger, a few drops; three times a day, in an ounce of water. This mixture is very grateful to the patient, it relieves thirst, and mitigates the morbid appetite. The tongue generally becomes moist, the urine diminishes in quantity, and contains less sugar. In one case, which may be taken as an average one, the amount of sugar was reduced from thirty grains to the ounce of urine to six grains, and the amount of urine from fourteen to four pints. (Dr. W. R. Basham, p. 84.)

Diet in Diabetes.—Dr. Wadham, of St. George's Hospital, has made a most exhaustive series of experiments, to determine the relative influence of bread, honey, and sugar, upon the amount of urea and sugar excreted in diabetes. He finds that bread in all cases, and in every stage of diabetes, largely increases the amount of urine, urea, and sugar excreted, and in every way aggravates the symptoms of the disease. Honey, on the other hand, may often be advantageously used as an article of diet, because in some cases a large amount of it may be eaten without materially increasing the weight of urea or sugar excreted, and the weight of urea may even diminish. Pure white sugar may be added to the diet in diabetes with every prospect of a beneficial result, for its use is accompanied by a diminution in the amount of urea excreted, and, when given in large quantities, less than one-sixth of the amount escapes as sugar in the urine, the remainder being either burnt off or otherwise appropriated to the uses of the system. (Dr. W. Wadham, p. 90.)

LITHOTOMY OR LITHOTRITY?—The most important element in deciding which operation is preferable in any given case, is the size of the stone. A stone which measures 1¼ inches to 1⅜ inches, in its longest diameter, is for the most part fairly amenable to lithotrity. A stone of which any measurement is 1¾ inches is mostly too large. The exact measurement is easily determinable, by catching the stone first in one diameter and then in the other, between the blades of a flat-bladed lithotrite, the size may be read off at the handle. The next thing is the nature of the stone, for an oxalate of lime calculus cannot be crushed, and must be removed by lithotomy. The feel of an oxalate of lime calculus is wholly different from that of a uric acid or phosphatic one, and when seized by the blades of the lithotrite experimentally, it does not "give" in the least, but the blades recoil from it with a spring, whereas, in the other two forms of calculus, a slight impression is made, and the blades feel to bite, or prick a little in the surface of the stone. The third element, in our judgment, of the most suitable operation is the age of the patient. The mortality from lithotomy below puberty is so small, that that operation should undoubtedly be preferred at that period of life, whereas in adults, lithotrity should always be performed, unless on account of the size or nature of the stone. (Sir H. Thompson, p. 151.)

MEDIAN AND LATERAL LITHOTOMY.—Some years ago, median lithotomy was much in vogue. It should now never, or hardly ever, be performed, for this reason: It is not suitable for the extraction of large stones, and it is not necessary for the removal of small stones, which should be crushed instead. (Sir H. Thompson, p. 154.)

SAYRE'S VERTEBRATED PROBE AND CATHETER.—Dr. Lewis A. Sayre, of New York, has invented a probe, consisting of a great number of short, hollow links, fitting one into the end of another, and connected together by a linked chain running through them. When this chain is slackened, which it can be by turning a screw in the handle, the whole becomes extremely flexible, and consequently capable of following the most tortuous sinuses as a probe, or distorted urethra as a catheter in cases of prostatic disease. (p. 155.)

AMPUTATIONS, FRACTURES, AND DISEASES OF BONES AND JOINTS, ETC.

Ankylosis of Joints.—If it is decided that a fibrous ankylosis is too firm to yield to gradual extension, even with the aid of tenotomy, it must be treated by sudden rupture. This operation is not attended with risk of inflammation or any bad consequences, provided the following precautions are observed. All contracted tendons must be divided and the punctures healed before the operation, or they may extend into rents of the skin of large size. Chloroform having been administered, the limb must be firmly fixed, so that all motion is prevented, except that which the operator is about to impart to the limb. The adhesions must then be ruptured instantaneously by force applied in the direction of flexion. The limb, and especially the joint, is then to be firmly bandaged and confined by a gutta percha or flexible splint. The bandage should on no account be removed until all tenderness has ceased. It seems impossible, with these precautions, to set up unhealthy action. (Mr. B. E. Brodhurst, p. 102.)

Antiseptic Surgery.—The discovery of Prof. Tyndall that such substances as cotton-wool possess the power of filtering the air passed through them from the invisible dust always suspended in the atmosphere, has given a new impulse to antiseptic surgery. It naturally follows that if a wound is covered with a good thickness of cotton-wool the further access of putrefactive particles or germs may be prevented. The cotton itself, however, requires thoroughly freeing from such particles, by being impregnated with the vapour of carbolic acid, to the extent of about the two hundredth part of its weight. The surface of the sore to be dressed should first be washed with a solution of carbolic acid, one part in forty, and then covered with a piece of oiled silk, the size of the sore, to prevent the dressings sticking through dryness. Over this must be placed a piece of folded linen rag, rather larger than the oiled silk, and impregnated with the carbolic acid vapour, in the same manner as the cotton-wool; the object of the rag being to absorb the discharge and prevent it from trickling down below the slightly-absorbent cotton, and so causing spread of putrefaction to the wound. Lastly, a well-overlapping mass of the carbolized cotton-wool is to be securely fixed by a bandage. By the cotton-wool all putrefaction will be excluded from the wound, although all chemical antiseptic power has left it. (Prof. Lister, p. 96.)

Antiseptic Cotton Gauze.—The material now used as an antiseptic dressing by Prof. Lister is a loose cotton fabric, the fibres of which are impregnated with carbolic acid securely lodged in insoluble resin, which holds the carbolic acid with remarkable tenacity, while at the same time a little paraffin is added to prevent the adhesiveness which the mixture of carbolic acid and resin would otherwise possess. The interstices between the fibres are kept free from these ingredients, so that the fabric may be fitted for absorbing discharges. This is laid on the wound in a thickness of about eight layers, and having a piece of impermeable tissue, such as what is known as "hat lining," interposed between the two outer layers. This compels the discharge to traverse all the extent of the tissue instead of passing directly outwards through it. Of course all septic particles must be destroyed in the wound itself before this dressing is applied. (Prof. Lister, p. 98.)

Antiseptic Spray during Operations.—In operating upon a case where the integument is previously unbroken, the entrance of all septic particles should be prevented by playing upon the part with a spray of carbolic acid and water. This need only be of the strength of one part of the acid to 100 parts of water, and not one part to 40 as previously recommended. The spray should be made as fine as possible, by having the lower end of the water tube almost entirely stopped up, leaving the air tube as before. (Prof. Lister, p. 98.)

Bandaging.—*An Aid to.*—A solution of wax in hydride of amyl is an excellent thing to pour over a bandage after being applied, or in which to saturate it after application. The hydride evaporates with great rapidity and leaves the firm but flexible casing of bandage and wax. (Dr. B. W. Richardson, p. 254.)

Drop-Wrist from Injury to the Musculo-Spiral Nerve in Cases of Fracture of the Humerus.—Occasionally in fracture of the shaft of the humerus the musculo-spiral nerve which winds closely round it is injured, and consequently we have paralysis of the extensors of the wrist and fingers, and of the supinators of the forearm. The appearance is a drop-wrist exactly like that resulting from lead-poisoning. Lower down it divides and comes into close relation with the outer condyle, injury of which also is liable to be accompanied by injury to the nerve or one of its divisions. The two divisions of the nerve are the radial and posterior interosseous. Sometimes the latter is the only nerve injured, and then the paralysis is not so complete, for the extensor carpi radialis longior and supinator longus are supplied by branches from the main trunk, and are consequently not paralysed. The best remedy in our hands is the persevering employment of faradisation to the arm, along with a splint specially constructed to produce extension of the fingers. (Prof. Erichsen, p. 133.)

Excision of Joints.—*When should a Joint be Excised?*—The state of the general health should primarily determine the necessity for excision in all cases, and not any arbitrary consideration of the period of the disease and the condition of the joint. Whenever therefore the general health is manifestly failing, whatever may be the stage of the disease, excision should be resorted to, without further delay. Osseous anchylosis with malposition of the limb will *not* justify the peril of attempted excision. (Mr. F. J. Gant, p. 117.)

Excision of the Elbow-Joint.—It not unfrequently happens that a spiculated enlargement, or growth of new bone occurs, just about the diseased portion of bone and limiting it; this must not be removed in excision of diseased joint. The bone should be removed up to this point, but not beyond it. (Mr. F. J. Gant, p. 125.)

Excision of the Wrist.—To perform this operation, make a curvilinear incision extending from just above the styloid process of the radius downwards across the back of the wrist and upwards to the same level above the styloid process of the ulna. The flap of integument is now to be reflected, carefully avoiding the extensor tendons of the fingers, and those of the thumb, on the ulnar half of the radius. Then, dividing the supinator tendons, and the extensor tendons of the carpus, and flexing the wrist, the radio-carpal articulation is to be opened. The other extensor tendons referred to must then be drawn aside with a curved spatula, by an assistant, and the articular ends of the radius and ulna, the carpel bones, and bases of the metacarpus, are successively removed by a small saw or cutting pliers introduced transversely. There is another method by means of two lateral longitudinal incisions, but there are several objectionable points to it. In the after treatment it is very important to maintain flexibility of the fingers and thumb by working them daily. (Mr. F. J. Gant, p. 127.)

Excision of the Ankle-Joint for Disease.—This operation may be said to have been introduced into British Surgery by Mr. Hancock, in 1851, although performed for injury in 1766 by Mr. Hey, of Leeds. It is an operation of value provided suitable cases only are chosen. It should be performed, 1, when disease commencing in the synovial membrane has extended to and destroyed the articular surfaces of the tibia and fibula, that of the astragalus, or of both opposed surfaces; 2, when disease, having the same articular consequences commenced in the cancellated tissue either of the long bones or of astragalus, provided it be limited to part of this bone—its upper articular portion. If resorted to at all excision should always be performed before the supervention of constitutional exhaustion. (Mr. F. J. Gant, p. 121.)

External Tumours.—*Michel's Process for Removing.*—The following is Michel's process for removing external tumours, for which Dr. Bell gave no less a sum than 25,000 francs. Asbestos in an impalpable powder is mixed with three times its weight of strong sulphuric acid. A mass is thus formed which may be easily worked with a silver or gold spatula into any size or shape corresponding to the tumour to be destroyed. Before its application the adjoining healthy parts of the skin must be carefully protected by a zone of collodion and pads of linen, and the patient must be so placed that the surface of the tumour is perfectly level. *The tumour is* usually destroyed by two applications of the caustic, the first of which must be of the thickness of half an inch for a tumour the size of a hen's egg. The second application should be smaller, and about twelve hours after the first. Hardly any pain is caused after the first half-hour. (Dr. W. A. Bell, p. 132.)

FRACTURE OF THE PATELLA.—The cause of separation of the two portions of a fractured patella is not, as is generally supposed, contraction of the rectus muscle, but effusion within the joint. There is, perhaps, no more successful way of treating these cases than to apply strips of plaster from below the lower fragment upwards, so as to fix this portion of the bone, and other strips from above the upper fragment downwards, so as to bring this portion of the bone downwards to the lower fragment. The limb should be allowed to be flat upon the bed. (Mr. J. Hutchinson, p. 119.)

HATTERS'-FELT SPLINTS.—Stout hatters'-felt, such as is used for making the hats of the Cornish miners, has for some time been largely employed as a material for splints at the London Hospital. It is particularly suitable for use after excision of the knee-joint. It is readily cut and moulded to the limb after soaking in hot water, and is capable of easy adjustment with iron brackets, &c., where such appliances are required. They are firm and yet light. (Mr. J. Hutchinson, p. 119.)

HIP-JOINT DISEASE.—*Dr. Sayre's Method of Treating.*—The author regards the majority of cases of hip-joint disease as the result, not of a strumous or tubercular dyscrasia, but of some local cause—a blow, or twist, or sprain. The resulting slight inflammation of synovial membrane, not being subdued by immediate rest, goes on developing slowly into a grave disease. He considers that purely local means of treatment are required, and that the treatment by long-continued rest and antiscrofulous medicinal agents is not only unnecessary but injurious. Dr. S. employs a splint by means of which slight extension of the joint is kept up, and the adaptation of which, according to the evidence of the editor of the *Medical Times and Gazette*, who saw it applied at the Middlesex Hospital, at once relieved all the pain in the case of a little boy in the second stage of the disease. For the description of this splint and mode of its application refer to the original paper. (Dr. L. A. Sayre, p. 107.)

Disease of the Hip-Joint in Children.—The great error we commit is in looking upon this disease as necessarily one of constitutional blood-disease, of which the joint affection is merely a local manifestation. We ought in th treatment to let the local affection be our sole source of care, for the disease i in no way connected with scrofula, nor is there any tuberculous disease of th bone. The reason these cases go on so unfavourably is that, owing to muscular contraction, the head of the femur is kept constantly pressed against th acetabulum, and absorption of both surfaces of bone takes place in consequence; there is destruction of bone, terminating in an abscess, which finds a outlet somewhere; or else it goes on to ankylosis. Now it will be found tha although ordinary movement of the limb causes exquisite torture, that if it i slightly extended it may be moved about without causing any pain whatever. I treating a case, put the child to bed, and apply extension of the limb in the direction of the angle of deformity as you find it. (If the capsule is distende with fluid, as in the early stages of the disease, the leg will be abducted; if th capsule is ruptured, it will be adducted.) In the course of a week or ten day there will be so much improvement that the limb may be brought straigh extension being still continued. For the details of this treatment refer to th article at page 112. (Dr. L. A. Sayre, p. 107.)

WOUNDS.—*Modified Antiseptic Treatment.*—Dr. J. S. Walker, of Hanle recommends a modification of the antiseptic treatment. His treatment aft amputation of the arm or leg, may be taken as illustrative. His directions ar —Sponge out the wound thoroughly with chloride of zinc ʒss. to ℥j. of wate then let all applications be quite dry for the first two or three days; soon as there is the least secretion of pus, take a Higginson's syringe and wa out the flaps with a strong solution of Condy's fluid, say ℥i. to ℥x. of wate apply strapping, and cover the wound with a small piece of lint, wetted with *lotion of* ʒij. carbolic acid, ʒij. liq. potassæ, to six ounces of water; cover wi *a piece of* gutta-percha tissue, and continue until healed, surrounding the edg or flaps with a little pad of carded lint. The carded wool, or marine lint, made by Mr. Westhropp, of the Falcon Works, London, is an excellent material for the purpose. (Dr. J. S. Walker, p. 129.)

M. Guerin's Method of Dressing Wounds.—During the late siege of Paris, the cases of important operations, such as amputations and excisions, as a rule did very badly. Having been struck with the experiments by Pasteur, Tindall, and others, illustrative of the dust and germ theory of disease, he bethought him of entirely encasing all surgical wounds with a thick coating of cotton-wool, with the view of filtering the air which had access to the part. Take the dressing of an amputation for example: he wrapped the stump round and round with successive layers of cotton-wool; a liberal use of the substance must be made, and several yards should successively disappear round the limb, and it should be carried up the limb to the body, and in the case of the thigh, as far as the waist. By this means all the approaches to the wound are carefully guarded in every direction. The whole should be gently supported by cotton bandages. This dressing is to be permanent for twenty-five or thirty days. On removing it a healthy granulating surface is discovered, and half a wineglassful of healthy pus is found within the folds of the cotton. This plan turned out very successful, the mortality after surgical operations from gunshot wounds being considerably lessened. (M. Guerin, p. 130.)

AFFECTIONS OF THE SKIN, ETC.

BURNS.—*Oakum as a Dressing for.*—Common oakum, soaked in water, laid on the surface of a burn, and retained in position by a bandage loosely applied, forms a most excellent dressing. It prevents all smell, and promotes healthy and rapid cicatrisation. (Dr. H. L. Snow, p. 177.)

Hydride of Amyl.—Hydride of amyl is a perfectly bland unirritating liquid, so volatile that it evaporates very rapidly, indeed it boils with the heat of the hand. It is an excellent solvent for oils and fats of every description, which by its means may be distributed very evenly over the surface of a burn or scald, so as to exclude the air. For this purpose spermaceti, cut into fine shavings, should be added to the hydride until it is saturated, when a sixth part of olive oil should be added. When this solution is poured freely over a burn or scald, the cooling which occurs immediately relieves the pain, and a pellicle or false skin of fatty matter is left upon the wound. If a little cotton-wool is loosely pressed upon the part, the rapidity of evaporation is subdued, and the dressing is more effective. The wool may be left upon the wound, more solution poured over it, and a bandage applied, under which dressing the burn will heal, if not very large. (Dr. B. W. Richardson, p. 251.)

HERPES ZOSTER.—The grand points in the treatment of herpes zoster are to exclude the external air by the application of flexile collodion constantly re-applied, and to prevent the neuralgia rising to any great height of severity by the subcutaneous injection of $\frac{1}{8}$ grain of morphia as often as required. (Dr. F. E. Anstie, p. 165.)

GANGLION AT THE WRIST.—Most of these cases can be treated successfully by subcutaneous incision, followed by squeezing out the contents into the surrounding tissues, and the application of a compress and bandage. Other plans of treatment are the following:—The joint may be flexed to the utmost and firm pressure made on the tumour with the thumbs; if the tumour is not got rid of by this means, a thread should be passed through it, and pressure made upon it as before, so as to empty it through the punctures made by the thread. The two ends of the thread should be tied together, and retained so until inflammation is set up, when the thread may be removed. A hare-lip pin may be passed into the cyst and made to puncture it in five or six places at the opposite side without puncturing the skin except at the point of entrance, pressure must then be applied as in the other plans. The cyst may be ruptured by pressure with the thumbs and then emptied of its contents by steady rotatory rubbing.

the part should be painted with a strong solution of iodine and a pad and ban age applied. (Report on Practice of London Hospitals, p. 168.)

Forcible rupture by digital pressure, followed by continued pressure, com bined with the use of a splint, have given the best results. In old and neglecte cases, the ganglion, instead of being confined to the tissue between the tendo and the skin, extends around the tendon, and thus every movement of the tendon after the rupture prevents the adhesion of the walls of the sac, hence the necessity for a splint. (Mr. Vincent, p. 176.)

Never forcibly rupture, but evacuate subcutaneously, by first incising the sac freely with a tenotome, then squeezing out the contents of the sac, and firmly pressing the walls together with a compress and strap of plaster. Should it recur, pass a fine silk thread through it with an ordinary suture needle, and leave it for three or four days. Injection with iodine is not very satisfactory. (Mr. B. Hill, p. 175.)

SKIN GRAFTING.—*An Instrument to Facilitate.*—Mr. Ferguson the instrument maker has at the suggestion of a Student of St. Bartholomew's Hospital brought out a most ingenious contrivance for facilitating skin grafting. I consists of a pair of curved scissors which are provided on their concave surfac with bent forceps. These are controlled by a lever, which descends with th separation of the blades and rises when they are brought together. When th blades are separated the forceps descend, seize a small portion of skin, whic they withdraw above the blades as they close. (p. 176.)

SUPPURATING SORES AND WOUNDS.—*Mode of Applying Iodine to.*—Iodi dissolves readily in hydride of amyl, and when this solution is applied to t skin, the volatile hydride passes off and leaves the iodine behind, stranded the part in most equal form of distribution. This application is of singul utility in cases of hard open sores, where it is desired to apply iodine even and deeply. Thus, in cases of open strumous glandular disease, the soluti plays an important part as a means of cure, and the same in chronic indole bubo. In bad sloughing fœtid ulcerative and suppurative wounds, and in ca cer, no solution is so simple, painless, and effective. In these last-named cas it exerts more than a curative influence, it destroys decomposing organic p ducts. The solution may be gently poured over the parts. There is no nec sity either for cotton-wool or brush. (Dr. B. W. Richardson, p. 251.)

VACCINATION.—The greatest care should be taken that pure lymph, witho the least speck of blood or any foreign matter, is used. If this old and w founded rule is carried out, and only children which are healthy both in the selves and in their family history are vaccinated from, there is not the le reason to suppose that any constitutional disease like syphilis can be tra mitted. (Ed. of Lancet, p. 180.)

VENEREAL AFFECTIONS.

GONORRHŒA.—There is no difficulty in running a stream of water through the urethra without inoculating its deeper portion. This is a better mode of applying injections than the syringe. The water used should be medicated with half a grain of permanganate of potash to the ounce of water. The amount may be increased up to two or three grains, but beyond this it causes pain and seems to take less effect on the discharge. Syphon power may be used, the vessel from which the injection flows being elevated considerably above the patient. This causes a steady and uninterrupted flow. A piece of small vulcanised india-rubber tube may be used for insertion into the urethra. An ordinary india-rubber enema ball and tube may be used for the same purpose, the ivory end being replaced by a glass cylinder. This renders the process very easy. (Mr. T. Windsor, p. 189.)

MODIFICATION OF DISEASE BY CONSTITUTION.—The most intensely specific diseases, such as syphilis, are always modified by the peculiarities of the patient's constitution. The course of syphilis in a gouty and in a scrofulous man, fo instance, is quite different. It is among the first necessities for success in prac tice that, in the total phenomena of a disease observed in any patient, we should be able to estimate what belongs to the disease and what to the man. (Sir J. Paget, Lancet, June 10, 1871, p. 776.)

SYPHILIS.—Experience in a regimental hospital, where the opportunities of watching cases for almost any length of time are very great, has convinced the author that in the infecting form of sore (whether soft or hard) there is but one safe practice, and that is, the continued administration of mercury in one form or another. It does not always prevent the occurrence of secondary symptoms, but it certainly postpones their occurrence and mitigates their severity. In the year 1864 the Lords Commissioners of the Admiralty appointed a committee to enquire into this subject thoroughly. The result of their investigations was in substance the same as the above opinion of Mr. Venning. As to the form of mercury most suitable for administration twenty-six witnesses were examined. Eleven gave the preference to blue pill, six to inunction, three to fumigation, two to mercury and chalk, two to Plummer's pill, one to bichloride of mercury, and one to protiodide. As a rule blue pill is well borne by the patient, and is a clean and convenient preparation. It is a good plan to combine it with a tonic, say three grains of blue pill to two of quinine. (Mr. E. Venning, p. 185.)

AFFECTIONS OF THE EYE.

CLOSURE OF WOUNDS OF THE CORNEA BY SUTURE.—Whenever a wound of the sclerotic or cornea gapes a suture should be applied, unless there is some special contraindication. Chloroform should be generally employed, and the thread should be very delicate. Dr. Williams, an American surgeon, who had up to 1867 applied a suture to the corneal flap in upwards of 100 cases of cataract, uses a single strand of the finest silk, and a fine needle a quarter of an inch long, with a flat cutting point. The needle is held by strong forceps and passed as near as possible to the edges of the wound, which are held by very delicate forceps. In a case of wound of the ball of the eye union at once followed the application of a suture, though the edges of the wound had been separated for many weeks. (Mr. T. Windsor, p. 162.)

EXTRACTION OF CATARACT.—The section must be purely corneal, not made where the cornea merges into sclerotic, for at this part union takes place with far more difficulty.. The usual cause of failure in this operation is the making the section too small for easy delivery of the lens. The easy extraction of a cataractous lens, like the easy extraction of a stone in the bladder, depends upon a clean cut and a fair sized opening; while a difficult and forcible removal is almost certain to end disastrously. (Mr. J. Hogg, p. 156.)

GRANULAR OPHTHALMIA.—*Local Use of Quinine.*—Some cases of granular conjunctiva with corneal opacity are greatly benefited by placing a small portion of quinine within the lower eyelid, on the point of a penknife. The operation is followed by severe smarting which continues for ten or fifteen minutes—but this is not invariably the case. In all cases considerable purulent discharge takes place, with shrinking of the granulations, and clearing up of the surface of the cornea. The intolerance of light ceases rapidly in all cases. Dilatation of the pupils is produced in from twelve to twenty-four hours after the first application, but they contract at once if exposed to a strong light. (Mr. C. Bader, p. 160.)

OPACITIES OF THE CORNEA.—If other means, including the pure turpentine oil, are found insufficient, touch the eye with a solution of one part of croton oil in seven of olive oil. This should be considered the standard solution for the purpose, but, on the first application, this solution should be diluted with an equal quantity of olive oil. Severe pain, great redness of the eye, with much lachrymation, usually come on in a few minutes. (Mr. T. Windsor, p. 161.)

MIDWIFERY AND THE DISEASES OF WOMEN.

Application of Strong Nitric Acid to the Interior of the Uterus.—Obstetric surgeons are becoming bold, for Dr. Atthill relates a case of sub-involution of the uterus following labour, and attended with menorrhagia, in which, after dilatation of the os and cervix, he cauterized the entire of the inner surface of the uterus freely with strong nitric acid. *No pain was caused.* The case was discharged cured in a short time. (Dr. L. Atthill, p. 224.)

Cancer of the Uterus.—*Destruction by Bromine.*—Dr. Wynn Williams says, "My own opinion is most decided that cancerous growths situated in the lips or neck of the womb can be successfully eradicated when confined to this part only, and previous to the commencement of ulceration. A solution of bromine in spirits of wine (12 drops in a drachm) is to be injected into the substance of the abnormal structure by means of a fine syringe. As, however, it is almost impossible to force the point of a fine needle into the more solid fibroid growths, a small trocar and canula, to which a glass syringe can be attached, should in this case be used. The point of the needle or canula should be withdrawn a little before injecting, or the fluid cannot be forced into the interior of the tumour. Before injecting the bromine the neighbouring parts should be protected by cotton-wool saturated with a solution of carbonate of soda. A slough will result, and separate in a week or so, and the process may be repeated in the same or another part of the tumour, although it is not necessary to wait until the first slough has separated. Instead of injecting the solution, it may be applied on cotton-wool directly to the os. The wool should be saturated with the solution and contained in a vulcanite cup. A slough results, as in the other case, but it is shallower and more extensive. No evil can possibly ensue, supposing the tumour thus treated should not be malignant, as it is certainly as ready a mode of getting rid of it as any other. A number of cases have been treated in this way without any return of the disease. (Dr. A. W. Williams, p. 241.)

Decapitation in Shoulder Presentations.—In cases of shoulder presentation, when either the pelvis of the mother is small or distorted, or the shoulder is wedged in the pelvis, the waters long discharged, and the uterus tightly moulded upon the body of the child, sometimes it is impossible to deliver by version, or only at such risk to the mother as to be unjustifiable. In these cases decapitation of the child is in most cases preferable to evisceration. It is a much older operation than evisceration, being described by Celsus and several later writers. To perform it it is only necessary to pass a piece of strong cord round the neck of the fœtus by means of an elastic catheter, and then to saw through the neck with the string. The string should be attached to the point of the catheter, which must be pushed forward, whilst the curved stilet is retained. The curve of the stilet will cause the catheter to curl round the neck. The ends of the cord should be crossed and passed through a speculum, so as to protect the soft parts of the mother; or if no speculum is at hand, two spoons may be used for the same purpose. (Dr. G. H. Kidd, p. 205.)

Dilatation of the Os and Cervix Uteri.—As is well known to those who have once tried the dilatation of the cervix uteri for the purpose of examining by touch the uterine cavity, after the withdrawal of the tent or tents it is found that the os internum has resisted free dilatation. Although the finger can be passed up the cervix, it cannot pass the os internum. Instead of using another tent, the best plan is now to introduce the smallest-sized Barnes' dilator, and dilate with air or water. The dilatation is effected more safely and rapidly. This diagnostic proceeding is always called for if in an otherwise healthy woman we cannot detect by careful examination any disease to account for persistent menorrhagia. After the dilatation the finger cannot be passed over the whole internal uterine surface without drawing the uterus down by means of a vulsellum forceps attached to the anterior lip, and at the same time pressing the organ down into the pelvis from above (this should be done by an assistant). Sea-tangle tents are preferable to sponge; they are more cleanly, and dilate the passage more uniformly. (Dr. L. Atthill, p. 219.)

Dilatation of the Os and Cervix Uteri in Supposed Polypus.—There is no danger in dilating the os externum, but if it is proposed to dilate the whole cervix, including the os internum, the greatest circumspection is required. Metritis, ovaritis, perimetritis, are frequent results of this exploration, and death itself may be a consequence. It is probable that forcible dilatation of the crevix uteri in cases where the upshot shows that it was required, is not nearly so liable to be followed by untoward accidents as when it is done in cases which turn out not to have required it. (Dr. J. M. Duncan, p. 234.)

Duck-Bill Speculum.—*How to Use.*—The thighs are flexed at right angles with the pelvis, the patient lying in a semi-prone position on her left side, her left hand being drawn backwards under her, and kept in that position; the chest rotated forward, bringing the sternum very nearly in contact with the table or couch, the head resting on the parietal bone; the head must not be flexed on the sternum, nor the right shoulder elevated; the patient is thus rolled over on the front, making it a left lateral semi-prone position. The nurse or assistant at her back pulls up the right side of the nates with her left hand, whilst the surgeon introduces the speculum, elevates the perineum, and gives the instrument into the hand of an assistant, who holds it firmly in the desired position. (Dr. M. Sims, p. 216.)

Fibrous Tumour of the Uterus.—*Incision of the Os and Cervix Uteri.*—This operation which was first brought into prominent notice by Mr. Baker Brown is founded on the theory that division of the os and cervix uteri permits the fibres of the body of the uterus to contract upon the contained tumour, and thereby to compress the vessels and prevent hemorrhage. Whether this be the true explanation or not, one thing is quite certain, that the operation is often followed by good results, and that in the case of very large tumours which are contained within the uterus, and when the cervix is thinned and spread over them, the operation is fully justified. (Dr. L. Atthill, p. 239.)

[This operation was recommended by Mr. Brown especially to arrest hemorrhage from these tumours, which it does most effectually and at once. Eds.]

Internal Medicines in Fibrous Tumour of the Uterus.—Medicines without number have been given for the purpose of producing absorption of fibrous tumours of the uterus. The most prominent among these are the bromides. After trying them fully and freely I have come to the conclusion that they are all perfectly useless. (Dr. L. Atthill, p. 239.)

Hemorrhage from a Fibrous Tumour in the Uterus.—Hemorrhage from a fibrous tumour within the uterus may be arrested by injecting the uterine cavity with a drachm of the liq. ferri perchloridi. The cervix should first be dilated. Of course this is only a palliative and temporary treatment. (Dr. L. Atthill, p. 240.)

Hysteria.—*A Test for.*—In every case of hysteria the commencement of the affection is marked by a special symptom, insensibility of the epiglottis. To determine the presence or absence of this symptom, introduce gently the forefinger into the mouth so as not to frighten the patient, and place it on the base of the tongue. It will be found that the epiglottis may be touched, displaced, and scratched with the nail, without producing the least regurgitation. When this symptom exists there will be found a congestion of one or both ovaries, usually the left. Out of 26,000 female patients this symptom existed in every case of hysteria—very numerous out of such a large number of cases. (Dr. Chairon, p. 66.)

Injury to the Sphincter Ani during Parturition.—In cases of laceration of the perineum with injury to the sphincter ani, the muscle merely being injured, not completely torn across, there is a tendency to looseness of the bowels, almost amounting to diarrhœa, and a source of great annoyance to a person of any refinement. It can only be cured by restoration of the perineum by operation. (Dr. J. M. Duncan, p. 195.)

Mechanical Dysmenorrhœa.—In cases of pin-hole os uteri with dysmenorrhœa, dilatation with sea-tangle tents should be performed, for nothing but this or dividing the os and cervix uteri with a hysterotome will suffice for the cure. Even supposing the latter operation is necessary, it is nearly always requisite to dilate the cervix with a small tent to allow of the introduction of the instrument. If, owing to anteversion of the uterus, there is difficulty in introducing the tent, a duck-bill speculum should be used to expose the os, which should be drawn down with a fine hook. If after trial of simple dilatation it is thought advisable to incise the constricted parts, we have the choice of several methods, the use of Dr. Savage's hysterotome is perhaps the best. It is passed through the os internum, and then by means of a screw in the handle its two fine concealed blades are made to protrude to a limited extent, which is increased when the os internum is incised. As the instrument is withdrawn the extent of incision is still further increased, or it may be completed by means of a curved scissors. It is upon the whole safer to divide the vaginal part of the os with the scissors. As there is often a tendency to unite, it is a good plan to introduce a thin roll of lead. (Dr. L. Atthill, p. 222.)

Menorrhagia from Sub-Involution of the Uterus after Abortion.—Supposing a patient subject to constant menorrhagia following abortion: the first thing must be to dilate the os and cervix uteri by one or more sea-tangle tents, and then having satisfied oneself that the hemorrhage does not depend upon the presence of polypus uteri, or disease of the mucous lining of the uterus, to measure the cavity of the organ by means of a Simpson's sound. If found enlarged, the hemorrhage depends upon sub-involution of the uterus. This must be treated on the principle of stimulating the womb to take on a healthy action and tone. No plan is better than passing into the uterine cavity ten grains of nitrate of silver, and leaving it to dissolve there. This must be done by means of Simpson's "uterine porte caustique," which consists of a hollow silver tube closely resembling a sound, and containing a stilette. Considerable pain is generally produced, which lasts five or six hours, but no further unpleasant results follow. This plan is most efficacious, and has not in any case been known to produce any serious symptoms. (Dr. L. Atthill, p. 224.)

Midwifery Forceps.—*The Mode of Introducing.*—Dr. Cappie, of Edinburgh, considers that in introducing the midwifery forceps, the task of introducing, guarding, and directing should be trusted entirely to the fingers of the right hand, and that, in the first stage of introduction, the left, crossed over the right wrist, should have little more to do than to support the shank or handle of the instrument. The point should be carried forward almost entirely by the index finger of the right hand, introduced within the fenestrum, and just catching on the inner edge of the rim. He says, "those who have never tried this plan will be surprised to find how easily it can be urged onwards, and how safely the proper direction can be given. When the finger has pushed it onwards as far as it can, the left hand becomes the propelling force. The forefinger, however, still acts at any convenient point as a fulcrum, to assist in giving the proper direction to the blade, while the hand gives the rotating and forward movements.' (Dr. J. Cappie, p. 199.)

Mothers Marks.—*To Remove.*—Dr. Berwick, of Sunderland, recommends the use of strong glacial carbolic acid for the removal of these disfigurations. He effected the removal of a large, hairy, corrugated, dark-coloured mark by its means. If of large size a part only should be operated on at a time, and to this part the acid should be applied twice a day. A scale or crust will form, which in time will drop off, leaving the skin beneath perfectly free from blemish. (Dr. G. Berwick, p. 249.)

Ovariotomy.—*Treatment of the Pedicle.*—The pedicle should be treated after the following fashion:—If the pedicle were long, narrow, and could be easily brought outside without traction upon the uterus or making any undue pressure upon the wound at the lower part of the belly, I should most certainly use the clamp; if, on the contrary, the pedicle were short, and I feared dragging or too much displacement of the uterus, and if it were not too voluminous, then I should transfix it with a firm ligature crossed so that it could be tied on one side only, cut the ligature close, and drop the stump into the pelvis. In cases where the pedicle could neither be safely brought out with the clamp nor ligatured for fear of vessels retracting or tissues shrivelling up; or where the pedicle was very short, vascular, and fleshy, and I feared the ligature might cut it; or where it was very close to the uterus, I should secure it with a clamp (moderately crushing it), divide it, secure each vessel with a ligature cut short, and apply the actual cautery to the rest of the stump, waiting a few minutes to see if any oozing of blood took place before dropping it into the pelvis. If the pedicle were very voluminous, the cyst having a very broad attachment to the uterus, so that I could not use the clamp to hold and crush it whilst I applied the cautery, I should divide it with a pair of blunt scissors slowly, taking up each vessel as I went on, and apply the cautery to the remaining portion of the stump. (Dr. D. L. Roberts, p. 226.)

Polypus Uteri.—*Hemorrhage as a Symptom.*—Although undoubtedly hemorrhage is the great sign of polypus uteri, its constancy as a symptom has been very much exaggerated. Among forty-one mucous polypi, nineteen, or about one-half, caused no extraordinary loss of blood whatever; while in several there was less than ordinary loss, and in some none of any kind. All fibrous polypi however, at one period or other of their growth cause loss of blood. Fibrous polypi are not so common as mucous. (Dr. J. M. Duncan, p. 234.)

Retention of the Secundines after Abortion.—At the early and the latter months of pregnancy there is comparatively little difficulty with the secundines, because, during the first two months, the aborted ovum is usually cast off entire; and during the last three months, if the placenta does not come away after the expulsion of the child, the uterus is sufficiently large to admit of manual extraction of it. But during the third, fourth, or fifth months, the womb is small and the vagina so undilated, that the removal of retained secundines can only be accomplished with the greatest difficulty. It is the best plan to place the patient under the influence of chloroform, and then carefully and gradually to introduce the whole hand into the vagina. If the os uteri is closed, it must be gradually dilated by passing first the forefinger into the orifice and then the second, with as little force as possible. The introduction of the fingers is generally effected easily. The placenta must then be removed with the fingers. When the os will only admit one finger, and the last phalanx cannot be passed much beyond the os internum, just reaching the retained placenta, a modified form of the ordinary ovum forceps (and invented for this pupose by Dr. Swayne) should be passed over the forefinger of the left hand, which fits within their hollowed inner surface, and when the os internum is reached, it may be enlarged by opening the blades, by means of a screw in the handle. The retained placenta can then be removed by the forceps, guided by the finger. (Dr. J. G. Swayne, p. 207.)

Sickness of Pregnancy.—The hydrated phosphate of lime, given in doses of from three to ten grains, three times a day, generally relieves the sickness of pregnancy. It should be simply suspended in water. The possible explanation is, that the requirements of the child in the production of bone, robs the nervous system of the mother of the phosphates necessary to its own health. (Mr. M. Johnson, p. 197.)

Tedious Labours.—*The Management of.*—By attending to certain rules, the result of his experience, Dr. Hamilton, of Falkirk, has been so successful as to have 731 children born alive successively, the 732nd being still-born. He never interferes with the first half of labour, however prolonged, but allows the os to be dilated naturally by the bag of membranes. The recommendation of the late Professor Hamilton, not to allow the first stage to last more than from twelve to fourteen hours, he considers bad advice. Supposing the membranes to be ruptured, and a rigid os uteri nipped between the child's head and the pubes, or the promontory of the sacrum, manual dilatation should always be resorted to, or the os will become swollen and inflamed, and so present still further obstruction to delivery. For this purpose, if necessary, the whole hand should be introduced, and the os resolutely pushed up over the head. The second stage of labour should never be allowed to go on more than two hours without the application of the forceps. In Dr. Hamilton's practice he applied them in one case in eight. In special cases we must not wait so long as two hours. The forceps should not be greased before introduction, as this tends to make them slip; on the contrary, it is a good plan to smear them on the inside with India-rubber paste, which soon dries, and enables them to lay firmer hold of the scalp than does the bare metal. Ergot should by all means be avoided. Dr. H. believes that the child's life is frequently lost in cases where everything is fair, the head well down in the pelvis, but the case lingering and protracted. (*Dr. G. Hamilton, p. 191.*)

MISCELLANEA.

ANÆSTHETIC ETHER (DR. RICHARDSON'S) FOR LOCAL ANÆSTHESIA.—This ether is composed of one part of hydrate of amyl to four of rectified ether. Pure hydrate of amyl is so volatile that it freezes too rapidly if used pure. The superficial parts become instantly frozen, and act as a non-conductor, preventing the deeper structures subsequently becoming frozen. (Dr. B. W. Richardson, p. 250.)

CHLORAL HYDRATE.—*Toxical Effects of.*—Dr. N. R. Smith, of Maryland, relates several cases, showing apparently that the long-continued use of chloral is liable to be followed by certain toxical effects. There may be a troublesome desquamation of cuticle around the nails, accompanied with superficial ulceration. It, in fact, produces somewhat similar effects to ergot when administered continuously, as the effects on the fingers justify us in believing. In large doses, say of thirty grains, its administration is not free from danger, especially if one dose is administered before the effects of a previous one are quite gone. (Dr. N. R. Smith, Lancet, Sept. 20, p. 466.)

Chloral Hydrate and Opium.—Chloral has this great advantage over opium as a hypnotic, that it does not lose its potency by repetition; as, after the lapse of a few weeks, during which time it has been regularly taken, the same dose will produce effects as marked and as happy as when first administered. (Dr. N. G. Mercer, p. 264.)

CHLOROFORM IN SURGERY.—The more I have used chloroform the less has my confidence become in its innocuousness. When I compare the results of my operations performed before anæsthetics were employed, with those performed during the last twenty-two years by the aid of chloroform, I am satisfied that unpleasant secondary results were less frequent during the first period than they have been under the use of that agent. I allude to secondary hemorrhages, pyæmia, erysipelas, and hospital gangrene. (Dr. N. R. Smith, Lancet, Sept. 20, p. 467.)

Chloroform Accidents.—Our aim should always be to give chloroform equally mixed, and in the smallest proportion needed for the purpose, so that if the effect becomes greater than is wanted, there may be as little as possible of the agent in the chest requiring to be got rid of. Clover's apparatus enables us to administer the vapour of three minims and a quarter of chloroform with every one hundred cubic inches of air. Should any laryngeal obstruction be threatened, the chin should at once be raised. An obstruction to the breathing sometimes occurs of a reflex kind—it is nothing but an act of swallowing imperfectly performed. Mr. Lister recommends the tongue to be drawn forward if laryngeal obstruction occurs, but Mr. Clover observes on this point: "I have met with several cases of partial obstruction of the larynx where the mere depressing or raising of the chin was sufficient to open or close the passage. In such cases, if I had tried to open the mouth by pushing down the chin, I should by that very means have caused total obstruction, and then, no doubt, dragging out the tongue would have been followed by a free inspiration, and probably louder and freer than would have been produced by the simple process of elevating the chin." (Mr. Clover, British Medical Journal, July 8, 1871, p. 33.)

Death from Chloroform.—Chloroform, ether, and other allied chemicals, cause death simply by poisoning the system by carbon, as do drowning and strangling. But in the former case the carbon is actively introduced; in the latter, the poisoning is passive, owing to deprivation of oxygen. In threatened death from chloroform, our endeavuor should be directed to supply oxygenated blood to the brain by lowering the head, so that what arterial blood is contained in the arteries may gravitate into the head. If the brain respond, all is safe; the heart resumes its labours, and the lungs answer to the call. If, however, we do not succeed in rousing the brain, the case is hopeless. Dragging out the tongue is no use—and artificial respiration is only loss of time. Let the head hang well down, and colour will return to the lips, and the functions of life will be resumed. (Mr. J. D. Brown, British Medical Journal, July 22, 1871, p. 93.)

COLOUR TESTS FOR BLOOD.—Prepare some fresh tincture of guaiacum by putting a few pieces of pure and unoxidized guaiacum resin into a small bottle and filling it up with alcohol—after shaking a minute or two it is fit for use. The stain must be moistened with this, and if this is followed by a negative result (many substances would produce a blue colour) we must add a few drops of antozonized ether, when, if the blood be present, oxidation and blueing of the guaiacum will follow. Robbins's ozonic ether is the best for this purpose. Blood produces no change in the colour of guaiacum except in the presence of antozone. A liquid has become antozonized when it is changed by absorption of oxygen from the air. All essential oils take on this change rapidly, and they then acquire the property of combining in all proportions with alcohol, and a little essential oil diluted with alcohol will do very well instead of the ozonized ether. By variations of this test mucus, saliva, and pus may be detected. It appears probable that this test will prove of service in medical cases—in testing for blood in matters vomited or expectorated. (Dr. John Day, of Victoria, Edin. Med. Jour., May, 1871, p. 1049.)

Cotton-Wool Respirators to Prevent Infection.—Prof. Tyndall recommends the use of cotton wool respirators for infectious places. He believes they would if properly constructed be perfectly efficacious in preventing the catching of a contagious disease. (Prof. Tyndall, p. 31.)

Disinfection.—*To Purify a Sick Room or Ward.*—No substance equals iodine for this purpose; but its successful use requires certain conditions, viz., length of time to act and complete diffusion through the atmosphere. The room should be completely stripped of all furniture, the walls rubbed down, and the floors well swept, scrubbed, and dried. A solution of iodine in hydride of amyl of the strength of twenty grains to the ounce should then be distributed in spray by means of a glass spray-producer, from different positions in the room. The room should have all its windows and doors closed before the iodine is distributed, and the quantity of solution used should be about one ounce of the solution to four square feet of space. After this the room should be kept closed for twenty-four hours at least, during which time the iodine, deposited at first in the finest layer on the floors, ceiling, and walls, slowly volatilizes and coming into contact with the organic matter destroys it rapidly. A light must not be taken into the room, or the amyl hydride vapour will take fire. (Dr. B. W. Richardson, p. 252.)

Hydramyl as a General Anæsthetic.—If hydride of amyl is carefully administered, insensibility to pain may be produced without loss of consciousness. The fluid however is so extremely volatile that it evaporates too rapidly. If to the materials for making bichloride of methylene (alcohol, chloroform, and pure zinc) about eight times the volume of hydramyl is added, a compound fluid distils over consisting of one part of bichloride of methylene to nine of hydramyl. This fluid is not quite so volatile as pure hydramyl, and yet possesses the same properties as an anæsthetic. In the administration of this fluid, a properly constructed inhaler should be used, and the first symptoms of involuntary movement carefully watched. This is generally twitching of the eyelids or slight involuntary movements of the hand. The inhalation should then be given up, as, on account of the insolubility of the fluid in the blood, its full effects are not felt for almost 10 seconds after the first appearance of involuntary movement. This slight interval prevents hurry—for the patient is quite conscious, and can be told to alter his position slightly, or open his mouth widely for the extraction of a tooth. There is complete insensibility to pain combined with perfect consciousness. (Dr. B. W. Richardson, p. 257.)

Organic Bromides.—Bromide may be considered as a medicine which acts primarily on the sympathetic system and as a modifier of vascular tension. It produces to a certain extent decreased sensibility of the nerves which govern common sensibility. We may administer bromine in combination with any other substance with which it will combine chemically, and thus procure valuable remedial agents having the action of their two constituents. Bromide of quinine, bromide of morphine, and bromide of strychnine are perhaps the most valuable of these. Two of these may be combined, thus bromide of quinine and bromide of morphine may be combined in syrup, of which a fluid drachm contains a grain of the quinine, and an eighth of a grain of morphine. Speaking generally the bromide in these salts is eliminative and sedative, and it is the result of experience that the bromide of quinine can be administered when quinine itself cannot be tolerated. It is certain also that the bromide favours the sedative action of morphia, while it at the same time allays the astringency which morphia induces. The bromides of quinine and morphine in combination constitute a most valuable medicine in the four following classes of cases, viz., neuralgic fever, cerebral irritation, diabetic phthisis, and extreme acute attacks of intermittent pulse, the result of organic nervous shock. In acute neuralgia it effectually removes the pain without producing any unpleasant symptoms. It calms pain without inducing deep narcotism, it interferes little with the secretions, it rarely causes nausea, and it interferes little with the appetite. (Dr. B. W. Richardson, p. 261)

Strychnia Poisoning.—*Calabar Bean and Chloroform.*—Dr. John White, of Glasgow, relates a case of strychnia poisoning in which 8½ grains of strychnia had been taken with intent to commit suicide. The treatment adopted was the inhalation of chloroform to check the extreme severity of the spasms, and the internal administration of calabar bean. The chloroform acted very well and was used freely, but the calabar bean was only given twice in doses of half a grain of the extract. No treatment can be conceived more suitable than this, as calabar bean directly and powerfully diminishes the reflex excitability of the spinal cord, so much so that it is difficult by galvanism to excite any reflex movements in a frog poisoned by it. In this mode of action it is far superior to curare, belladonna, and tobacco. (Dr. White, Glasgow Medical Journal, August, 1871, p. 488.)

Vulcanite Urinometers.—Urinometers made of vulcanite are cheaper and more durable than those made either of glass or metal. They, moreover, take the specific gravity more rapidly, and are unacted upon by all acids or alkalies, and by no liquid except bisulphide of carbon. They may be obtained from Messrs. Blaise, of St. James's Street, London. (p. 266.)

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THE

RETROSPECT

OF

PRACTICAL MEDICINE AND SURGERY,

BEING

A Half-Yearly Journal,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M.D.,

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.,

AND

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PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

1.—THE PATHOLOGY AND TREATMENT OF PYREXIA.

By Dr. Charles Murchison, LL.D., F.R.S., Physician to St. Thomas's Hospital.

The natural heat of the body is due to vital and chemical processes resulting n the oxidation or combustion of nitrogenous and carbonaceous substances furnished to the blood by the tissues, but mainly by the food. The products of this combustion are eliminated from the lungs in the form of carbonic acid, and from the kidneys as urea and uric acid. The oxidation of carbon resulting in the formation of carbonic acid is effected by the corpuscles of the blood; whereas recent researches make it probable that the albumen is transformed into urea and uric acid in its passage through the gland-cells of the liver, spleen, and other glands, and through the cells of the blood itself. The albumen which is thus being constantly transformed or split up into urea is not the fixed albumen of the muscles, nerves, and other formed tissues; but the so-called store-albumen, which exists in the blood, and is constantly passing to the cells through the body, and returning to the blood again. From this, also, the organs take what they require, and the waste is made up partly by the effete albumen cast off by the tissues, but mainly by the food.

The preternatural heat of fever is the result of vital and chemical action exalted above the standard of health, assisted perhaps by a disturbance of the processes by which heat is carried away. The proof of this is found in the augmentation of the products of metamorphosis eliminated by the lungs and kidneys, and in the loss of bodily weight far exceeding what can be accounted for by the mere abstraction of food. We have already found that there is an increased elimination of carbonic acid from the lungs in pyrexia, which may in part account for the consumption of fat. It is, however, the increased elimination of nitrogen by the kidneys which has been mainly studied. The amount of urea and uric acid voided in the urine is, as we have seen, largely increased in fever. There is evidence, also, that the increased excretion of urea precedes ny rise of temperature; and although the amount of urea cannot be measured y the degree of heat, there is a direct relation between the two. As a rule, he temperature is highest, and the quantity of urea greatest, in the early stages f a fever; and when there is an unusual elevation of temperature, there is an nusual amount of urea. There are, no doubt, exceptions. The temperature modified by the amount of evaporation going on from the surface of the kin; and the urea may be lessened by albuminoid matter, more or less hanged, being retained in the blood. In badly nourished persons, also, it has een found that comparatively little urea is eliminated notwithstanding the rise temperature; but that the latter is also less than in the robust and well fed, d may be due to an increased formation of carbonic acid. In one respect, the urce of temperature in fever differs from that in health. In health, the elimination of nitrogen is entirely regulated by the amount entering the body with e food, as has been well shown by Parkes and other observers; but the increase of nitrogen eliminated in fever does not come from the food, for it is out all proportion to it. The fixed albumen of the muscles, brain, and nerves eaks down into circulating albumen, to be in its turn transformed into urea d other nitrogenous *excreta.* Hence it is that the muscles waste and the brain becomes atrophied. The large amount of cerebral fluid so common in protracted fevers is merely thrown out from the blood to fill the space vacated by the brain, and is not of inflammatory origin, as was once believed. In all fevers the muscles become wasted, and, on microscopic examination, the muscular fibres can often be seen in the act of disintegration. The granular and waxy degenerations of the muscles found by Zenker in enteric fever occur in all severe fevers, and in typhus the cervical ganglia of the sympathetic have been shown to undergo a granular degeneration. The impaired nutrition of the heart and of the nitrogenous tissues accounts in part for the weakened cardiac action and for the stagnation of blood in the capillary circulation, which are so common in the advanced stages of fever. The only parts of the body that do not waste in fevers are the glandular organs, and especially the liver, spleen, kidneys, and lymphatic glands, which become enlarged and congested from the increased functions thrown upon them, the enlargement being greatest in the young and robust, who have most tissue to spare for conversion into urea. The gland-cells of these organs become swollen with minute granules; and a similar appearance is often presented by the white corpuscles of the blood, which are usually increased in number.

The large amount of nitrogenous detritus formed in fever may be all eliminated by the kidneys or bowels, or a portion may be retained in the blood, either as urea or as some half-transformed albuminoid matter, and then the temperature may be elevated without a corresponding augmentation of urea in the urine. Critical deposits of lithates in the urine are chiefly observed in those cases of fever where, from the symptoms, it might be inferred that there has been a retention of nitrogenous detritus in the system. The urea, or other less oxidised products of metamorphosis circulating in the blood and permeating the tissues, gives rise to symptoms of uræmic poisoning, or to the typhoid state already referred to; this condition being in some instances aggravated by the non-elimination of carbonic acid, consequent on the congested condition of the lung.

There are good grounds for the opinion that this is the true pathology of the typhoid state. 1. All modern pathologists are agreed that the cerebral symptoms of most fevers are independent of any appreciable structural lesion of the brain or its membranes. 2. The typhoid symptoms of febrile diseases often closely resemble those of uræmia resulting from disease of the kidneys. The two conditions are, in fact, often mistaken for one another; and excepting that the temperature is increased in the one case and not in the other, it may be sometimes difficult to distinguish them. The symptoms in both cases are probably due to the presence of similar morbid materials in the blood, the difference being that in pyrexia these materials are generated in excess, while in renal disease the kidneys are unable to eliminate the normal quantity. 3. In the course of these lectures I shall have occasion to tell you that in many fevers presenting typhoid symptoms—such as typhus, relapsing fever, yellow fever, and the secondary fever of cholera—urea has been actually found in the blood and in the cerebral fluid. 4. Lastly, the advent in fever of grave cerebral symptoms, and in particular of convulsions, which may be regarded as the acme of both uræmia in kidney-disease and of the typhoid state in fever, is often preceded by albuminuria, or by a marked diminution, or even suppression, of urine. When stupor, delirium, and coma show themselves in the course of a contagious fever, it is the custom to refer them to the action of the fever-poison on the brain; but the cerebral functions are more probably deranged, not by the fever-poison, which was the first and necessary link of the pathological chain, but by the accumulation in the blood of the products of metamorphosis, and the consequently perverted and defective nutrition of the brain and nerves. Hence it is that the symptoms in the advanced stages of many fevers and local inflammations (the typhoid state) are closely assimilated, although the primary maladies have been widely different. The precise grouping of the symptoms will vary in different cases of the same disease, while in diseases essentially distinct it may be identical.

It is difficult to say why the nitrogenous detritus is excreted in some cases and retained in others; but its elimination appears to be often prevented by some morbid condition of the large glands, and especially of the kidneys, either of old standing or consequent on the febrile attack. Disease of the kidneys is a serious complication of all fevers. As long as the kidneys are equal to the increased work thrown upon them, the blood is properly depurated, and the typhoid state is warded off. But if the kidneys be unequal to the task, either from the large amount of effete material to be eliminated, from previous disease of the secreting tissue, or from congestion and other changes resulting (as they often do) from their increased work, then the blood becomes polluted, and convulsions or the typhoid state supervene. This explains why albumen so commonly appears in the urine in the course of all acute diseases of a severe form, and why it is so justly regarded as an unfavourable symptom. It also explains why albuminuria is looked upon as a contraindication to any serious surgical operation. The surgical fever which follows an operation is attended by an increased metamorphosis of the blood and tissues; and if the kidneys be diseased, the products of this metamorphosis will be partly retained in the blood, and induce the typhoid state with all its dangers.

Another probable result of the non-elimination of nitrogenous products in fever is the development of local inflammations. The half-changed albuminoid matters circulating in the blood seem sometimes to be deposited in different organs, and there to excite secondary inflammation. Cases of idiopathic fever have been observed in which a sudden diminution in the amount of excreted urea has been followed by an attack of pleurisy or other local disease, the quantity of urea again increasing as the local complication has receded.

When convalescence from fever is fairly established, and the patient is regaining weight, the elimination of nitrogen and also the temperature are often below the normal standard.

The metamorphosis of nitrogenous matter which occurs in health is under the control of the nerves, and so the augmented metamorphosis of fever appears to be due to some abnormal state of the nervous system. According to the well-known experiment of Claude Bernard, an elevation of temperature to the extent of from 7 to 11° F. is produced on one side of the face of an animal, when the trunk connecting the sympathetic ganglia on the corresponding side of the neck is divided, the sensibility of the part becoming greatly excited and the vessels dilated and hyperæmic. This elevation of temperature must be ascribed to the hyperæmia and increased metamorphosis in the part, which had before been held in check by the sympathetic nerve. The converse of this experiment has been performed by Waller, who found that contraction of the dilated capillaries, diminution of vascular injection, and reduction of temperature followed the irritation of the divided sympathetic by the transmission of an electric current. Experiments on the vagus nerve have led to equally important results. Section of the vagus is followed by increased rapidity of the heart's action, and an increased lateral pressure of blood in the arteries; whilst irritation of the cut nerve by an electric current reduces the number of beats of the heart and diminishes the lateral pressure in the arteries. These and other observations make it probable that the increased metamorphosis, the elevated temperature, and the increased frequency of the heart's action in fever, are due to paralysis of the sympathetic nerves and the vagus. Many clinical facts show that the nervous system exercises a powerful influence on the early phenomena of fever, such as the rigors, the pain, the languor and prostration, which are among its earliest phenomena.

The facts and arguments bearing on the pathology of pyrexia now laid before you may be summed up as follows:

1. A morbid condition of the blood, due to the entrance of some poisonous matter from without or generated within the body, or to some local injury or inflammation, exercises a paralysing influence on certain portions of the nervous system, and particularly on the sympathetic and the vagus.
2. Increased rapidity of the heart's action is one of the earliest results.
3. A second result is a rapid disintegration of the nitrogenous tissues into substances of a simpler chemical construction, while little or no fresh material is assimilated to compensate for the loss. Increased temperature, great muscular prostration, and loss of weight are the natural consequences.
4. The non-elimination, from any cause, of the products of this disintegration gives rise to cerebral (typhoid) symptoms or local inflammations.

5. The impaired nutrition of the heart itself and of the rest of the body i conjunction with the polluted state of the blood and the nervous paralysi already referred to, induces in severe cases of fever great weakness of the cardiac contraction, and stagnation of blood in the capillaries in different parts of the body.

Why it is that the febrile process once lighted up should ever become arrested, and in many instances after a definite duration, is a problem in medical science of which no satisfactory solution has yet been offered.

Diagnosis.—From what has been stated, you will understand that mere quickness of the pulse does not constitute pyrexia, but that its diagnosis must depend upon the presence of an elevated temperature. It is this that constitutes the great value of the clinical thermometer in diagnosis. It enables you to say positively whether a patient is suffering from fever or not. Against one fallacy in diagnosis I wish to put you on your guard. A patient may be suffering from pyrexia; and at certain hours of the day, which may correspond to those of your observations, the temperature may be perfectly normal. In any case of doubt, then, it will be necessary, before excluding the existence of pyrexia, to note the temperature at two or three different hours in the course of the day. In phthisis, for example, and in certain cases of enteric fever, the pyrexia is often for a time distinctly intermittent.

The Prognosis in any case of pyrexia will of course be greatly influenced by its cause; but, irrespectively of the cause, there are certain conditions which must always be regarded as of serious import.

1. In grave fevers the prognosis is worse in robust full-blooded persons with abundance of muscle and fat than in those of spare habit, because in the former there is more material to spare for disintegration during the febrile process. In scarlatina it is a matter of daily observation that the plump and ruddy child succumbs, while the spare and more delicate-looking child pulls through. And in adults it may be said that all fevers are grave in proportion to the development of muscle and fat.

2. The prognosis is always bad when severe fevers attack individuals in whom the defæcation of the blood is naturally imperfect—for example, in persons of a gouty diathesis.

3. The danger in any case of fever is increased by the fact of the patient having a naturally weak heart, not so much from the presence of valvular disease as from weakness in the contracting power of the muscular walls. Its strength may be sufficient for the ordinary work in health; but when fever occurs, the work is increased while the organ is further weakened, and then it is apt to break down.

4. The danger is always great when there is evidence of antecedent renal disease. To the kidneys we must look for the elimination of the large amount of nitrogenous detritus resulting from the febrile process; and, when the eliminative power of these organs is impaired by disease, the peccant material is retained in the system, with the deleterious consequences already described. Here again, the kidneys may be sufficiently sound for the due performance of their functions in a state of health, although they are quite incompetent to discharge the additional work thrown upon them in pyrexia.

5. In most fevers the prognosis is bad in proportion to the intensity of the pyrexia, as measured by the thermometer. With few exceptions, a case with a temperature of 105° must be looked on as severe; and one with a temperature of 107° as highly dangerous.

6. In any case of fever the prognosis will be bad in proportion to the intensity of the typhoid state; and when convulsions occur in the typhoid state, recovery is rare.

7. In any case the prognosis will be bad in proportion to the weakness of the heart, and the tendency to stagnation of the blood in the capillaries of the lungs and other parts of the body.

Treatment.—If the views which I have laid before you respecting the pathology of fever be correct, our objects in treatment ought to be as follows:—

1. To remove when possible, the cause on which the fever depends.

2. To promote elimination, not merely of any morbid poison, but of the products of exaggerated metamorphosis in the blood and tissues.

3. To reduce the temperature and the frequency of the action of the heart.

4. To maintain the nutrition of the tissues and stimulate the action of the heart by appropriate food and stimulants, taking care, at the same time, not to excite congestion or increase the work of the already overtasked glandular organs.

5. To relieve dangerous and distressing symptoms.

6. To obviate and counteract secondary complications.

How these indications are to be fulfilled will be discussed under the head of individual febrile diseases, but a few general remarks on the treatment of fever in the abstract will not be out of place here, and may save repetition hereafter. You are not to imagine that each fever has a treatment of its own. According to the predominance of certain symptoms, the appropriate treatment will vary greatly in different cases of the same fever, while in fevers widely different in their causes it may be identical.

1. Unfortunately, it is not often that we have it in our power to remove the cause of pyrexia; but the object is one always to be kept in view, and sometimes the main efforts of our treatment must be directed to secure it; as, for example, when pyrexia depends upon pent-up pus, an obstructed bowel, or gouty, syphilitic, or periosteal inflammation.

2. The elimination of any morbid poison, as well as of the products of exaggerated metamorphosis, will often be promoted by the judicious employment of diaphoretics, diuretics, purgatives, and emetics. The old practice of commencing the treatment of pyrexia by giving a purgative to unload the portal circulation and promote the action of the liver, is undoubtedly a good one, and is particularly advisable in persons of robust habit, or who live too well. In mild cases of pyrexia, the only treatment necessary consists in the avoidance of any chill, and in the administration of a mild aperient, followed by frequent doses of diuretics, and diaphoretics, such as the citrate of potash, or the liquor ammoniæ acetatis with spirit of nitrous ether. Elimination will also be promoted by a plentiful supply of fresh air, which will favour the escape of carbonic acid from the lungs, and by the free use of diluents, which will help to wash away through the kidneys the products of tissue-waste. In all grave cases of fever you will remember the importance of maintaining the action of the kidneys, and of keeping a good watch on the state of the urine; noting carefully not so much its colour and the presence or absence of lithates (both of which characters will depend much on the quantity), but the quantity and the presence or absence of albumen. When the quantity becomes notably diminished, or albumen appears, advantage will often be derived from hot poultices to the loins, aperients, diaphoretics, diluents, and diuretics. But while you promote elimination, you must take care that the means for this end do not weaken too much the action of the heart; and you must remember that n some fevers, the natural processes of elimination are excessive, and conduce o dangerous exhaustion and death.

3. For reducing the intensity of the pyrexia, different measures have been roposed.

Blood-letting was at one time universally resorted to for this object, but in his country it is now entirely discarded, because it was found to increase one f the great dangers in pyrexia, viz., failure of the heart's action. There are ew accurate observations on the effects of blood-letting on the temperature of yrexia; but we know that, when a copious bleeding from the nose or the owel takes place in enteric fever, although the temperature may fall to below he normal standard, it speedily regains its former height, or rises above it.

The external use of cold water is one of the most certain means of reducing mperature in pyrexia, and in certain cases is attended with good results. The tention which this practice is now attracting will justify the following re-arks. In the seventeenth century, the brothers Hahn, of Leipzig, treated vers by the external use of cold water, but their observations were soon for-otten. Towards the end of last century (1787) cold affusion was proposed by r. Currie, of Liverpool, both for arresting and mitigating fever. The patient as seated naked in an *empty tub* or bath, and several buckets of water, of a *mperature of 40° or 50° F.*, were poured from a height of one to three feet, or more, over the head and chest. He was then hastily dried, and restored bed, and in most cases, the operation was repeated once or twice daily. It w stated that, in many cases, if resorted to during the first three days, this tre ment arrested the disease; while, in others, it reduced the pulse and temper ture, relieved many of the distressing symptoms, and particularly the headach restlessness and delirium, and conducted the disease to a safer and speedi issue. The affusions were employed at any stage of the fever; but the effec were always most salutary at an early stage. They were said to be contrai dicated when the temperature of the skin, ascertained by the thermomet was not much above the normal standard, or when, notwithstanding an ele tion of temperature, the patient complained of chilliness, or suffered fro severe diarrhœa or profuse sweating. The wonderful results obtained Currie, were confirmed by numerous observers in different parts of the worl whose testimony is recorded in the third edition of his work, published in 18 But in the British epidemic fever of 1817-19, the practice was followed by ma with great perseverance, and the general result, according to Sir Robert Chri son, was that in very few cases, if any, was the disease arrested by it; t although an abatement of febrile heat and restlessness occurred almost inv ably, it was of short duration, and not to be made permanent by any frequen of repetition; that as much good eventually was attained by frequent cold tepid sponging, together with cold applied to the head; and that often the c affusion occasioned for a time after each application an intense feeling of pr sure and weighty feeling in the brain, which could not be regarded with some uneasiness. These statements, backed by professional and popular p judice, account perhaps for the subsequent neglect of the cold-water treatm of fevers. But the observations made of late years by Brand, of Stett Jürgensen, of Leipzig, Liebermeister, of Basle, Ziemssen, of Erlangen, and Weber, and Wilson Fox, of London, show that, although the practice may shorten the fever, and is often inapplicable, yet under certain circumstance is useful not only for reducing the temperature, first of the surface and then the interior of the body, but for relieving headache and other distressing sym toms, removing congestion of the kidneys, warding off delirium and coma, an rousing the nervous system in cases of excessive stupor. The circumstan has perhaps been too much lost sight of that cooling the body may not infl ence the conditions on which the development of heat depends; but wi reduced heat it may be assumed that there will be diminished metamorphosi to the non-elimination of the products of which many of the dangers of fev are due. In point of fact, Schroeder, of Dorpat, has ascertained that co baths effect a marked diminuition in the excretion of carbonic acid and urea i fever; and as this was not attended by any aggravation of the general sym toms, it is fair to attribute it to a retarded metamorphosis of tissue.

Statistics have been appealed to to prove the great success of the cold-wat treatment of fevers (particularly of enteric fever) as contrasted with that of expectant method; and, although other conditions not stated may have help to influence the result, they suffice to show that the practice is not beset wi the dangers commonly imagined. But the most conclusive facts in favour the practice are those observed in certain cases of hyperpyrexia by Dr. Wils Fox and others, where its employment was followed by recovery from an e vation of a temperature (110° F.) which under every other method of treatm has been speedily followed by death. At the same time there are many ca of pyrexia in which the cold effusion or immersion would be unsuitable or jurious. It is likely to be of most service when the temperature is unusu high; and in all cases the practice is contraindicated when the temperatur under 102° F., or when the extremities are cold, although the temperature of central parts of the body be high; and it must always be employed with caut when there are the signs of weakened cardiac action or of stagnation of bl in the capillary circulation, although it may be noted that in one of Dr. Fo patients, who was apparently rescued from death, the face was cyanotic the radial pulse imperceptible.

There are different plans for employing cold water in the treatment of rexia, such as the cold affusion practised by Currie, packing in a cold wet sh resorted to by Brand, or immersion in cold baths. The last is the method r most in fashion. The patient is placed in a bath having a temperature of from 60° to 70° F., or better, as Ziemssen recommends, in one whose temperature is about 10° below that of the body, but which, after the patient's immersion, is gradually cooled down to 68° by adding cold water. He should remain in the bath for half an hour, or until shivering comes on, and all the time he is in the bath his limbs ought to be rubbed by assistants. He is then to be hastily dried and put into a warm bed. For some time after the bath, the temperature in the rectum continues to fall as the trunk parts with its heat to the extremities; but as soon as the temperature in the rectum rises again to 104°, the patient ought to have another bath. In the early stages of the fever, as many as seven or eight baths in the day may be necessary. When cold affusion or immersion is contraindicated or inexpedient, frequent sponging of the surface with cold or tepid water will also help to cool the body, and is often a source of much comfort to the patient.

Quinine in large doses has an undoubted influence in lowering the temperature of pyrexia. In most cases of severe pyrexia, ten, fifteen, or twenty grains will, within an hour or two, cause a fall of the temperature to the extent of three or four degrees, and to a less degree of the pulse. It is true that the effect passes off after a few hours, and that there is no good evidence (except in malarious fevers) of its cutting short the natural course of the attack; but the effect may be maintained by a repetition of the dose: and the remedy has often appeared to me to be of signal service when a pyrexia was at its crisis, and when the temperature was rising in place of falling.

Digitalis, Aconite, and Veratrum Viride have a marked power in reducing the pulse, and, to a less extent, the temperature in pyrexia, and are in my opinion, too much neglected for these objects in practice. Veratrum viride is largely used in America in the treatment of fevers, and its effect upon the pulse is speedy and most decided; the only objection to its use in private practice which my experience suggests is its liability to induce sudden nausea and faintness, but these symptoms are transient and cease on the administration of a stimulant. Ten or fifteen minims of the tincture may be given every four or six hours. Aconite is a remedy of great value for reducing the pulse and temperature in fever, and especially in the pyrexia resulting from local inflammations, and is much less used than it deserves to be. Digitalis is another remedy which I have often found very serviceable in various forms of pyrexia. While increasing the force of the cardiac contractions, it diminishes the frequency of the pulse, reduces the temperature, and increases the flow of urine. Lastly, *antimony* reduces, in a marked degree, the frequency of the pulse in pyrexia, and promotes diaphoresis and mucous secretion. It was at one time largely used in all fevers, but in many it is contraindicated by its tendency to weaken the contracting power of the heart.

4. The nutrition of the body must be maintained by appropriate food, in the form of milk, beef-tea, eggs, and farinaceous articles. Not long ago it was the custom to starve fevers; and you may probably have heard that the late Dr. Graves, of Dublin, who was mainly instrumental in doing away with this objectionable custom, expressed a wish that his epitaph might be "He fed fevers." The modern tendency, however, is perhaps to over-feed fevers, and especially to give too much nitrogenous food. Dr. Parkes has shown that there are theoretical objections to a purely nitrogenous diet in fevers. It is doubtful if the disintegrating nitrogenous tissues can be fed; and in that case the albuminous food must be got rid of by the already over-tasked glandular organs. Milk is in most cases preferable to beef-tea as an article of diet in fevers.

In many cases of fever it will be necessary to give stimulants. You must not give stimulants simply because a patient has fever. Many patients with fever do better without them. But you must not refrain from giving stimulants when the heart shows signs of weakness, as happens in the advanced stages of most protracted fevers. The heart may be artificially stimulated by sinapisms and other irritating applications to the skin, but better by the internal administration of ammonia, ethers, and alcohol, in quantities proportioned to the weakness of the heart and pulse.

5. *In every case of pyrexia*, you must combat dangerous symptoms as they

arise. Stagnation of blood in the pulmonary capillaries impeding the aeration of the blood is to be met by stimulants, such as alcohol, carbonate of ammonia, and ethers. Digitalis, by strengthening the heart's action, and turpentine, which seems to stimulate the capillary circulation, are also useful under these circumstances; while advantage will likewise be derived from mustard- and linseed-poultices to the chest, sometimes from dry cupping of the chest, and from warm applications to the feet. When uræmic symptoms predominate, the action of the skin and bowels is to be promoted, digitalis and saline diuretics may be given to increase the flow of urine, sinapisms and linseed-poultices are to be applied over the loins; while attempts may be made to rouse the patient by cold affusion to the head, by blistering the shaven scalp with liquor ammoniæ, and by sinapisms to the nape and feet. In many cases of fever you will also be called upon to relieve distressing symptoms—such as diarrhœa, pain, sleeplessness and delirium—which, if unchecked, hasten exhaustion and prevent recovery.

6. You must counteract, as far as possible, secondary complications, which will vary according to the primary cause of the pyrexia, and which always add to the patient's danger.

Lastly, I would caution you against two errors in the treatment of pyrexia.

1. You must take care that the remedial measure which you adopt in no way thwart the natural modes of recovery, or favour the natural modes of death.

2. At the same time, you must not be content with adopting a treatment of pure expectancy. You must not forget that the natural termination of pyrexia may be death, as well as recovery.—*British Medical Journal, Feb.* 27, 1872, *p.* 175.

2.—ON THE PERIOD OF INCUBATION OF TYPHUS, RELAPSING, AND ENTERIC FEVER.

By Dr. Charles Murchison, LL.D., F.R.S.

[The knowledge which we possess upon this subject is very meagre and unsatisfactory, being based upon insufficient data. Dr. Murchison first gives thirty-one observations on the period of incubation of *Typhus fever* and then continues:]

From the cases now recorded it would seem that the usual period of incubation is about twelve days. Of the nine cases in which it was exactly determined, in four it was twelve days; and in thirteen more of the thirty-one cases it might have been twelve days. In other words, out of the thirty-one cases, in seventeen the period of incubation was either twelve days or this duration was within the known limits. It may be added that Jacquot, who calculated the latent period from the date of embarcation of healthy French troops on board vessels infected with typhus, found in a considerable number of cases that it varied from nine to thirteen days, the average being somewhat less than twelve days.

But occasionally the period of incubation exceeds twelve days. It did so with certainty in four only of the thirty-one cases now recorded. In one only of the cases was there reason to think that it was as long as twenty-one days. Theurkauf records two cases, in one of which it was eighteen days and in the other between fourteen and nineteen days. Peacock also relates the case of a man who, in 1862, nineteen days after his admission into a surgical ward of St. Thomas's Hospital, was attacked with typhus to which it is believed that he could only have been exposed prior to his admission. I know no reliable facts, however, showing that the latent period of typhus can exceed three weeks, and statements to the effect that it can extend over several months require confirmation. Few, at all events, will admit, on the evidence adduced by Bancroft, that an interval of five or six months may elapse between exposure to the poison and the commencement of the disease, an opinion to which he was forced by his determined opposition to the possibility of an independent origin of the fever.

On the other hand, in not a few cases of typhus the period of incubation is less than twelve days. It was so in ten, at least, of the thirty-one cases now recorded. Davies records the cases of four Norwegian sailors, who on the night of their ship's arrival in Bristol from Onega, visited some typhus-fever nests, and all four sickened with typhus eight days after. In my own second attack the latent period was exactly five days (Case 20). There are also authentic instances of an extremely short latent period, or where there has been scarcely any latent period at all. The late Sir Henry Marsh collected nineteen cases in which the disease manifested itself almost instantaneously after exposure to the poison. In most of the cases the persons complained of an offensive odour proceeding from the beds or bodies of the sick, and immediately suffered from headache, great prostration, nausea, or rigors, followed by the usual symptoms of typhus. Similar cases were mentioned by Haygarth; others were observed by Gerhard at Philadelphia, in 1836; and in two of the thirty-one cases now recorded (Cases 24 and 25) there were reasons for believing that the symptoms commenced immediately after the first exposure. In some of these cases it might be difficult to exclude the possibility of previous exposure to the poison, but in others there were no grounds for such suspicion, and in all, the patients appeared to be conscious of the moment at which the poison entered the system. It would seem that the poison of typhus may be so concentrated, or that the system may be so susceptible of its action, that its effect may be almost instantaneous.

From the above facts the following conclusions may be drawn:

1. The period of incubation of typhus varies in duration in different cases.
2. In a large proportion of cases it is about twelve days.
3. In exceptional cases it is longer than twelve days, but it rarely, if ever, exceeds three weeks.
4. In many cases (one third or more) it is less than twelve days, and occasionally there is scarcely any latent period, the symptoms commencing almost at the instant of exposure to the poison.

[He is able to offer fewer observations on the period of incubation of *Relapsing Fever;* the facts which he has observed, however, point to the following conclusions:]

1. The period of incubation of relapsing fever is not a fixed period, and is even more variable than that of typhus.
2. It is, on the whole, shorter than that of typhus. In not one of the nine cases in which it was accurately determined did it exceed nine days; in none of the twenty-five was there reason to believe that it exceeded sixteen days; in only two did it certainly exceed twelve days, and in only three others was it possible for this period to have been exceeded; while in fourteen of the twenty-five cases, or in more than one half, it did not exceed five days.
3. Occasionally, as in typhus, there is scarcely any latent period at all, the symptoms commencing almost immediately after the first exposure to the poison.

Enteric Fever—(Typhoid or Pythogenic Fever.)—Reliable facts bearing on the period of incubation of enteric fever are even more difficult to obtain than illustrations of the latent period of typhus or relapsing fever. In my own practice I can call to mind only two cases throwing light upon the question, and in these all that could be said was that the period of incubation was not longer, in one case than twenty-one days, and in the other than fourteen days. Medical men of much experience in fever, and among others Dr. J. B. Russell, Superintendent of the City of Glasgow Fever Hospital, and Dr. T. J. Maclagan, formerly Superintendent of the Dundee Infirmary, have also informed me that they have met with no cases showing the latent period of enteric fever. Several circumstances contribute to make it very difficult to obtain satisfactory evidence on the point as regards enteric fever:—1. The difficulty in many cases in deciding when an attack of enteric fever really commences. 2. The circumstance that nurses and patients in fever hospitals rarely take enteric fever. 3. The fact that in private practice, when the disease has been imported into a healthy locality, according to my experience it rarely spreads. 4. The difficulty often in determining, when a person is seized with enteric fever soon after changing his *residence*, whether he has brought the disease with him, or whether he has been predisposed to the disease by recent arrival in an infect locality.

From the facts before us the following conclusions may be drawn :

1. The period of incubation of enteric fever is most commonly about t weeks.

2. Instances of a longer duration appear to be more common than in typh or relapsing fever.

3. The period of incubation is often less than two weeks, and, as in typh and relapsing fever, it may not exceed one or two days.

It would be an interesting inquiry how far the period of incubation vari according as the poison is introduced by the alimentary canal or by the lung —*St. Thomas's Hospital Reports*, 1871, *p.* 23.

3.—ON THE DIAGNOSIS OF TYPHOID FEVER IN ITS EARLY STAGES.

By Dr. P. W. LATHAM, Physician to Addenbrooke's Hospital, Cambridge.

[In typhoid fever cases the thermometer is indispensable. The physici who judges of fever cases without it may succeed pretty well if of large exp rience, but he is to some extent in the dark, and will not treat his patients s satisfactorily.]

During the first four or five days the general symptoms which may then, I have told you, accompany the disease—viz., the rigour, the languor and feebl ness, headache, epistaxis, giddiness, pain in the back and aching of the limb the appearance of the tongue, the state of the bowels, the condition of t urine, &c.—may not be very distinct, or any one of these morbid symptoms m be entirely absent. In a considerable number of cases, in fact, it would be i possible for you to say, without using the thermometer, whether the patie were suffering from typhoid fever or not. But the thermometric course of t disease at this time, unless it supervenes on some other malady, is very reg lar; and by taking the temperature at 8 a.m. and 6 p.m. for three days t presence of typhoid fever may be decided. On the other hand, one sing observation may, with very great probability, negative the existence of t disease.

The following is the formula (from Wunderlich) of this initial stage :—

	Morning.	Evening.
1st day	98·6° F.	100·4° F.
2nd „	99·4°	101·4°
3rd „	100·4°	102·6°
4th „	101·6°	104°

If, then, a person, previously quite well, feels uneasy, perhaps has a rig and in the evening we find his temperature about 100·4° or 101° F., falling next morning about a degree, rising again in the evening, and approximat following the above course, the disease may be diagnosed with tolerable tainty.

On the other hand, the disease is not typhoid fever if (1) on the seco third, or fourth evening the temperature approximates even to the nor (98·6° F.) ; (2) if during the first two days the temperature rises to 104° F.; if between the fourth and sixth days the evening temperature of a person und middle age does not reach 103° ; (4) if the temperature on two of the first th evenings is the same ; or (5) if it is the same on the second and third mo ings. From the fourth to the tenth day the evening temperatures are tolera uniform, the highest being most generally on the evenings of the fourth, fi or sixth days, and reaching from 104° to 105·5° F., or even higher. The mo ing temperatures are from 1° to 2·6° F. lower than the evening ones ; on fifth, sixth, and seventh days the variations between the morning and even temperatures being less than take place from the sixth or seventh to the ni or tenth days. During this period (from the fourth to the tenth or twe day), if the general symptoms are obscure, an absolute diagnosis may be dily made, and the disease may be confounded with several others, unless thermometric observations extend over several days. Here, again, the thermometer gives us then very definite information. Wunderlich tells us: "That if in a youth or individual of middle age, who was previously well, an illness has continued from five to ten days, and we find evening temperatures of 103·4° to 105° F., or a little over, alternating with morning temperatures of 1·4° to 2·6° lower, and without any other disorder showing itself to explain the elevation, and without the patient previously being subject to gross neglect, then we are justified in positively diagnosing the disease as typhoid fever." There is at the present time in the hospital a girl in whose case the application of this law materially helped us to arrive at a correct diagnosis; from the ninth to the twenty-first days of the disease, as you see from the chart, her evening temperatures mostly reached 103° to 103·8° F., with some morning temperatures 1°, and some 2° lower. This, excepting small successive crops of rose spots, was the only distinct evidence of the existence of the disease; the other symptoms —the condition of the tongue, the character of the evacuations, &c.—not being at all characteristic. The normal temperature in this case was not reached until the thirty-fifth day.

To Professor Wunderlich's work on temperature I am much indebted for the information it affords. I last year in my clinical lectures gave you copious extracts from it on febrile diseases in general, on pneumonia, ague, &c. It is soon to appear in an English form, translated for the Sydenham Society, and I recommend it to your careful study from the great importance and extreme interest of the subject.—*Lancet, March* 2, 1872, *p*. 287.

4.—ON THE ORIGIN OF FEVER.

By Dr. Pratt.

[The following paper was read before the Surgical Society of Ireland.]

I have read a great deal lately in the newspapers, both English and Irish, as well as in the weekly medical periodicals, relative to the cause of typhoid and other fevers. The writers abstaining in general from the production of any real facts, seem to be unanimous in attributing such diseases, in their inception, to the decomposition of animal and vegetable matter. They trace the first rise of the malady to malignant effluvia emitted from manure heaps, stagnant pools, drains, sewers, cesspools, and to all such heterogeneous accumulations as are found near the dwellings of the poor and farming classes, as well as to the gases arising from the closets and closed sewers of the rich, who dwell in palaces and fare sumptuously every day.

After an experience of a quarter of a century as an Irish Dispensary Medical Officer, I venture to record my dissent from these conclusions. As a practical man, in charge of a large and populous district, together with a practice over 200 square miles, I need not say that I have had abundant opportunities of becoming familiar with the dwellings and habits of all classes of the community; and it is my firm conviction that the agencies above indicated cannot be actually productive of fever of any type. Were it otherwise, Ireland would ere this be depopulated from sea to sea, or at most but sparingly and thinly inhabited.

It is a well-known fact that, with rare exceptions, the farm yards of the Irish agricultural classes are simply the open spaces either in the front of their dwellings or close behind. The office houses, the cow houses, stables, &c., either ranging with the main dwellings, or actually forming a component part of them. The farm yard manure, carelessly heaped, in many instances up to the very doors, and in such a way that it often becomes a problem to the perplexed doctor, whose practice is desired within, how to effect an approach, especially when called upon in the dead of night, without sinking ankle deep in mire and filth, or perhaps coming to worse grief in the shape of a souse in the slough of despair.

Such is the state of affairs during the winter months—in the hot weather of the summer the pits, from which the accumulated manure had been removed to the farm, serve as a receptacle for slops and refuse of all sorts thrown from the houses. These slops, fermenting in warm weather, produce a green stagnant pool, the gases generated showing themselves in the form of bubbles on the surface, which in due time burst, and, of course, discharge their *supposed* noxious contents in the immediate neighbourhood of the dwelling, with all its inmates, old and young.

In places such as I have very inadequately described, I may say I have rarely observed a case of fever of any type to occur. The average length of life is very high in many such places, and I may say also, that illness in many such places is almost unknown, save in the form of a common cold, or such diseases as are peculiar to infantile life and childhood.

In many instances, I am sorry to say, our peasantry live in a state of barbarism even worse than that already described—the farm stock being housed in the same dwelling as the human inmates, the manure thrown in heaps everywhere about the house up to the very threshold; the pigs and poultry fed upon the kitchen floor, and the refuse of the repast, together with the contributions from the guests—porcine and feathered—swept into a deep pit close by the fire-side (called the ash-hole or ash-pit), which is rarely cleaned out until it is filled to overflowing. Yet even in such habitations as these I have found the families hale and sound—strangers to fevers and other diseases which, if the contrary received theory be correct, ought to be too familiar acquaintances.

Let me add an illustration:—Some eight or ten years ago, during the construction of the Newry and Armagh Railway, the small town of Markethill was crowded to excess with navvies and their families, there was not an available spot in the town, but was filled to suffocation—many rooms tenanted with two or more families at night. Notwithstanding all the filth and stench consequent upon the over-crowding, the season was one exceptionally free from disease generally, and in particular marked by the absence or rare occurrence of those of a febrile character.

Nor is my experience in these matters at all singular in what I have stated in reference to the continuance of vigorous health in the close neighbourhood of influences commonly believed to be of a noxious character. Clergymen of all persuasions meet with the same state of things in their walk of life, as well as Dispensary Medical officers, and the intelligent members of the Constabulary Force who, being brought by their statistical duties into close connection with the farming classes, are perhaps qualified to give an opinion as well as clergymen and doctors.

It is a fact that vegetable and animal life will flourish and thrive wonderfully in the vicinity of such places as I have already pointed out: grasses of all sorts grow luxuriantly around the borders of such places, and ducks and many sorts of waterfowls feed and fatten upon such stagnant pools, with their bills as deep into them as they find necessary to procure feeding from the bottom, oftentimes as far down as length of neck will permit.

Rats and frogs also live and thrive well about such places. I have recently seen a closed sewer, which runs through a farmer's yard, opened for the purpose of having it cleared out; it had not been opened for many years previously; it was almost entirely filled with most offensive smelling matters. After the removal of the flags, which formed the covering, a number of frogs were found lodged together at one side of the drain, more than two yards from the opening into it, where they had collected to hybernate; they were all alive, and had every appearance of health, and no doubt, were they permitted to remain a little longer time, they would have come forth from their retreat of their own accord, in due season, to spawn.

I carefully examined the spot where the frogs collected themselves, and was unable to discover the remains of a dead one, thus showing that the gases generated in the sewer had no injurious influence upon them.

Familiar as I am with facts such as I have described, I own it has seemed to me passing strange to s[illegible]dently asserted that His Royal Highness the Prince of Wales m[illegible]t the dire disease, which has so lately been the cause of so much anxiety to every loyal heart in the realm, from defective sewerage in the house of his noble entertainer.

"The wind bloweth where it listeth;" and in my humble opinion it is the same with the seeds of disease.

Strange, indeed, are the laws of epidemics: at one time, small-pox rages, as at present; at another, fever; at another, scarlatina; at another, erysipelas; at another, *cholera;* whitlow, perhaps, and influenza, &c. Let it be *particularly noted* that the conditions productive of fatal disease are, in all cases, subject to little or no variation, and therefore, where the causes are perennial, as among the Irish peasantry, there ought to be no respite from the ravages of deadly maladies.

The facts, unfortunately for the theory, are perverse; instead of a confirmation, they supply a refutation, to my mind irresistible.

The conclusion to which all my experience tends may be expressed briefly, and will, no doubt, have been anticipated by my audience, viz., that the causes generally assigned as generating typhoid and other fevers are completely inadequate, and that it yet remains to be discovered from what mysterious sources those fatal maladies arise.

Dr. Darby said he could corroborate every word of Dr. Pratt's paper by his own observation. It was most valuable, because of the theories that were now put forward, not only as to contagion or infection spreading from the diseased to the healthy body, but also with regard to its production from manure heaps and decomposing organic matter. There was only one cause which he thought would account for the spread of disease from human being to human being, and that was overcrowding. All his life he had seen the healthiest human beings reared in farm-yards, the children playing from morning till night on the dunghill, and the pigs and fowls running about the house. The more filth about a farm the richer a farmer would be. His object was to make manure, and he did it, but without detriment to the health of the inhabitants of the farm. A few months ago he was attending a case of small-pox in the dirtiest house he knew. It was filled with children; it was filled also with cocks and hens, and he was not at all sure that the pig did not sleep under the bed. He had the curiosity to measure the room in which six children were sleeping. It was seven feet wide by nine feet long, and six and a half feet high, and had but one small window. Six children were sleeping in one large bed in that room, and one of the children had small-pox. He recommended the removal of the child to the hospital, but the parents would not consent. He then told them to remove the other children from the room, but this could not be done, as there was no other place to put them. This was three months ago, and not another case of small-pox occurred in the house. The overcrowding of people in small lodging houses, did, he believed, generate fever, but he denied that manure heaps or gas emanating from decomposing matter would do so. Practically, he thought the wisest course was to leave a vent for the gas to escape and to prevent its accumulation, and in some places that was done; but in truth, he did not think the disease called typhoid fever could be traced to any distinct source. In these farm-yards and houses the people went on from year to year with the same dirt, and there was no small-pox, typhoid, or scarlet fever among them. A change took place, an epidemic set in, and then disease spread among people who had been free from it for years. Some gentlemen said these diseases could be stamped out. He doubted the possibility of this being done. They did not stamp out the cattle disease, or the grape disease, or the potatoe disease. They were fighting with an unknown enemy, and they were not prepared with the proper weapons to fight him. He did not mean to disparage cleanliness—quite the contrary. He believed it possible to stamp out endemic disease by improving the habits and increasing the comforts of the people. They might stamp out scrofula and cachexia by sanitary and other measures, but he did not think they could stamp out epidemic disease.

Dr. Stokes was glad to be present at the reading of Dr. Pratt's paper. He was the first to bring forward the important fact connected with the prevalence of fever in Ireland, that, while the presumed causes of fever are permanent, the effects *are not permanent but* intermitting, or at least remitting. The dung heaps, the imperfect sewage, the filth of the country farmers' places formed permanent institution. The occurrence of fever was epidemic and inconsta That was one great fact not easy to get over in the theory of the septic orig of fevers. He remembered a good many years ago when there was a trem dous epidemic of fever in certain parts of Ireland, and especially the south portions; and when Limerick, Cork, and other towns were the centres of most terrible plague, the town of Killarney might be said to have escap The Grand Jury of Kerry, greatly struck with this, directed his brother, was County Surveyor, to make a survey of Killarney, which he did, and t result was something startling. It was proved that there was, by some eno mous proportions, a greater quantity of putrescent matter in Killarney th either in Cork or Limerick. The number of cubic feet of putrescent matter each individual was something incredible. A good deal had been said of th distinction between typhus and typhoid fevers. He should be sorry that th Society adopted those hard and fast lines of distinction between different form of essential disease. They were all very well on paper and in the books nosologists; but the practical man who met with these diseases in great num bers, would tell them that there were many cases which he could not put clear into this category or that category. Many of the authors who contended f these hard and fast lines of distinction, had not observed or practised in th country. Their experiences had been drawn from the London or Edinburg Hospitals, and they had not practised much, or at all, in Ireland. If they d they would have an opportunity which did not occur in English Hospitals, seeing an entire family in the wards of an hospital at the same time, both t parents and five or six of the children; and in that group of cases, where o cause most probably produced the disease, they would meet with every possib variety of fever, well marked petechial typhus, non-petechial fever, pythogen fever (to use the newest phrase), or enteric fever, simple fever, rheumatic feve Therefore it seemed a very difficult matter to say that these diseases are esse tially distinct. He had lately read a paper on the "Correlation of Zymot Diseases." He would go a step further and say, it might yet be a questi whether the various cases of zymotic disease were not only correlative but co vertible. With respect to the question of stink, the tendency of modern sa tarians was too much confined to the removal of nuisances. They thought th would do all that was required by preventive medicine. Preventive medic was a development of knowledge of our own generation, and promised to be of the most glorious products of the present time—far beyond curative me cine, for that depended on the slow advance of medical science, which all pended on the characters and the training of the professors of curative me cine. But preventive medicine implied the cure of the masses of mankind a of the population at large. It depended on wise legislation. He believed t sources of epidemic diseases were not yet known. Dr. Pratt gave utterance t the same idea when he said "the wind bloweth where it listeth." The grea Humboldt, in his Cosmos said, that among the various questions that involv human inquiry, there was none more obscure than the origin of epidemics. *It* was a fashionable doctrine that endemic and epidemic diseases could be removed or destroyed by what was called sanitary reform. The argument on which the doctrine was based was this: an epidemic disease appears: sanitary measures are adopted, and the disease disappears. But this argument was really a weak one, when they reflected that epidemic diseases were like diseases in individuals; they had their appearance, their growth and their spontaneous subsidence, and the question was whether this subsidence was the result o sanitary measures or merely the result of a general law. The greatest disgrac had come on the profession of medicine, by gentlemen putting forward remedies for this disease and that disease, that would disappear without any remedy at all, and attributing the recovery of their patient to the last remedy applied And so it was with epidemics. They could not then necessarily attribute thei cessation to the adoption of this or that curative remedy or preventive measure or sanitary reform. There was another matter which Professor Macnamara ha *not alluded* to, but which he knew was in his mind, for he had frequentl *spoken to him* about it—the great error of attributing great phenomena to *few limited* number of causes or to one cause. They only knew of one caus capable of producing great phenomena, and that was the cause of all the phenomena of the world and of man—he meant GOD. They did not know of any great phenomena or series of phenomena that was not the offspring of an infinite number of causes; and to say that typhus results from emanations proceeding from the overcrowding of human beings, or that pythogenic fever proceeds from the putrescence of matter in which the seeds of typhoid fever had been mixed, would seem altogether wrong. Dr. Graves, when there was an outbreak of Cholera in London, wrote his celebrated article against the view of the *Times*, in which he showed the folly of attributing it to the sewage. He thought it was settled that bad odours were not in themselves necessarily deleterious. It was quite possible that one odour might be deleterious and another not. Labourers working in knacker's yards and in sewers did not appear to be more liable to disease than other persons in the same class of life. Dr. Pratt, coming from the North of Ireland, was well aware of the frightful odour from the maceration of flax that pervaded the country at certain seasons, yet it had never appeared that it had been the cause of an outbreak of typhoid or any other fever. With regard to sanitary reform, civilization required that all these deleterious influences should be removed—not merely bad smells, but everything, whether of a moral or a physical nature that could retard the healthy development of the human frame. They should inquire whether sanitary reform acted simply by extinguishing the germs of disease, or whether it acted by the improvement of the general health of the population, thereby enabling the human body to resist contagion, or, if it yielded, to bear up well against the disease. He believed that this was the true way of looking at sanitary reform. *Med. Press and Cir., March* 6, 1872, *p.* 206.

5.—THE ORIGIN OF FEVER.

By Dr. EDMUND P. SHARKEY, Ballinasloe.

It occurred to me when reading the discussion on Dr. Pratt's paper before the Surgical Society of Ireland, [see preceding article] that he and those who adopted his view, did so without considering how difficult it is to prove a negative, inasmuch as that one well-authenticated fact will outweigh the most plausible opposing theory. The same thought must have occurred to other readers as well as to me, to many of whom, I doubt not, cases in their own experience must have suggested themselves confirmatory of the views sought to be impunged. It has so happened that I have at this time under observation what I consider a crucial instance. The case I allude to is the following:—A large family lived in a house in one of the parishes composing my dispensary district—a house long notorious for its filthy surroundings: the whole frontage was faced by a cesspool, the receptacle of all the filth which the proprietor, in the ardour of his manure manufacture, could rake together from every quarter. It was situated in a hollow, and the house and offices being higher, and there being no sewer, of course the rain and other moisture could not escape. His landlord, a very kind and generous man, frequently offered to assist him by shoring up the house in order to make a passage for the liquid portion of the collection; but in vain. The consequence was that a more than usually offensive atmosphere pervaded the dwelling. I doubt if my friend Dr. Pratt, in his Connaught experience, ever encountered so bad an one—I know I never did—and while writing an order for food for the patients I was obliged to beat a hasty retreat from the premises. Well, though the family constitution long held out, it at length gave way; the father was attacked by a low form of fever, which I have been in the habit of designating "typhoid," but which lacked two of the characteristics *now* included in the term—viz., enteric complication and rose rash. He with great difficulty struggled through it, though more than once thought to be moribund. When he became convalescent, the oldest daughter took it, and others in succession, so that at last there were six lying ill, four in one small room (the father's); but none of them had it so severely as the father. Now, I may observe that there had not been a case of fever in the parish for weeks or months, that the father had not been near any one sick during the same period any where else, and that a family of three persons relatives of these people, who had visited them during the course of the fever, took ill; the father of this second family, a fine young man, a ploughman, died of the worst form of the disease, marked by black tremulous tongue, delirium ferax, followed by coma vigil, and, at the close, convulsions. This last was the only one in whom the rash was observed, but there was no enteric complication. Now, here, was a fever of a bad type, for whose origin, in my mind, no reason can be given save one,—the odour of putrefying matter, animal and vegetable. I may observe that the well from which they drank was a very fine one, far away from the cesspool. With regard to the second family, it may be doubted whether their illness arose from infection or malaria. I think the former, and if I am right, it confirms Dr. Stokes's assertion "that we cannot adopt the hard and fast lines of distinction between typhus and typhoid disease, however well they may look on paper and in the books of nosologists." Here were several cases presenting different types, all originating in the same source, a source now generally believed to be the origin of the typhoid type of disease, yet without the characteristic marks of that type, and seemingly highly infectious, contrary to the received opinion regarding that type. I cannot help thinking, from the views advocated by speakers of eminence on that occasion, that Medical opinion is not, after all, very progressive. I remember, when young, hearing an anecdote of an old-fashioned and eccentric physician in Cork, who said, at a meeting of the Profession on the occasion of the fearful epidemic to which Professor Stokes alluded, "that he did not think there could be a more wholesome thing at a man's door than a fine dunghill." He was thought then to be not *au courant* of Medical science, and now we find his sentiments repeated more than half-a-century after, in a meeting embodying the highest Medical talent in the country. I agree with Drs. Stokes and Pratt that "stink" *per se* will not produce fever, but all effluvia are not deleterious in proportion to their offensiveness, for I am free to confess with one of the speakers that the smell from flax pools is even worse than that from the ordinary dung heap. As to Dr. Pratt's difficulty of understanding "how the greater part of the Irish population should not have perished, if these emanations were morbific," he must remember the Irish cabins are generally built with two "hall doors," through which the wind freely plays, and when it blows strongly in front the front one is closed, and when at the back the back door; so that the tainted air is blown over and round the house, or to speak poetically, "'Tis odour fled as soon as shed." Again, as to his argument about the luxuriant verdure of the herbage about its brink, that it must be equally favourable to animal life, he might as well say that the Sunderbunds of Bengal, noted for its luxuriant jungle, could not produce jungle fever or cholera, whereas it is well agreed that it furnishes the most lethal forms of both.—*Medical Press and Cir., March* 27, 1872, *p.* 283.

6.—ON THE TREATMENT OF THE FEBRILE STATE.

By Dr. LIONEL S. BEALE, F.R.S., Physician to King's College Hospital.

[In considering this question it is necessary to discuss the influence of remedies upon the feverish condition generally, irrespective of the causes which have given rise to it. It will appear that there are certain general facts of the highest importance, the proper estimate of which will lead to the adoption of certain measures of great practical value in all forms of fever.]

Acting upon the hypothesis that the danger to life from fever depends upon the increased temperature, many Physicians have adopted plans of treatment which have for their object the carrying off heat from the surface. This is accomplished by the external application of cold. Although there is evidence that cold to the surface does good in some cases in which the body-heat is above the normal standard, few Physicians feel sufficiently satisfied that *conclusive proof* has yet been adduced that this indication for treatment can be *with wisdom* fearlessly followed out in all cases.

This remarkable increased development of heat in all fevers is probably an immediate effect of another change. It is certainly associated with, and (I believe) due to, very rapid growth of bioplasm, principally in the slow-moving blood in the capillaries; but the bioplasm in the tissues external to the capillaries is also involved, and in some fevers in a very remarkable degree. The increase of the bioplasm continues, as does also the development of heat, for an hour or two (and in rare instances for a much longer period) after death, and occasionally up to a certain time even in an increased ratio; and for this reason: The bioplasm being perfectly still, and everywhere surrounded by nutrient pabulum, is under the most favourable conditions for appropriating rapidly all that lies sufficiently close to it, and therefore rapidly increases until the adjacent pabulum is exhausted. But when this happens, as no fresh nutrient fluid can possibly be brought to it, the bioplasm dies, and the temperature soon falls.

Now (as will be presently explained), there is reason to think that many of our remedies act beneficially by the direct influence they exert upon the process of increase of the bioplasm in the blood and in the tissues. In fever and certain low states of the system, the bioplasm of the tissues and blood invariably increases more rapidly than in health; and, as I showed many years ago, the classes of remedies which experience has proved to be beneficial are those which check the growth of bioplasm. Stimulants effect this change quickly, and many of the so-called tonics possess this property in a very remarkable degree though they act more slowly.

Cold reduces the temperature, and may also be instrumental in bringing about a state of things unfavourable to the growth of the bioplasm, which is the immediate cause of the rise. If, however, we could prevent the undue growth of the living matter, or check it before it has proceeded to a dangerous degree, the temperature might not rise so high as to render the external application of cold either necessary or desirable. If we proceed back yet another step in the inquiry, we shall find that the multiplication of the masses of germinal matter or bioplasm is due to the rapid appropriation of excess of pabulum. If, therefore, this pabulum were removed, or its constitution changed, or its formation prevented, there is reason to think that the increase of bioplasm would be stopped, in which case the feverish state could not be established. Or, if the blood could be urged more quickly through the capillaries, the heat developed would be carried off, and pass away latent in vapour from the skin and lungs. The rise in temperature would not take place so long as the increased rate of the circulation compensated in this manner for the undue development of heat. This remark will appear strange to many readers who attribute increased body-heat to increased oxidation; but there can be no doubt that the view is correct, for the temperature invariably rises as the capillary circulation becomes more feeble, and, as already remarked, often obtains its maximum some time after the blood has ceased to circulate at all—when, in fact, the functions of respiration and circulation, which are specially concerned in oxidation, have ceased.

In *health* any disturbing influences are compensated with remarkable rapidity. Thus, exposure to degrees of heat or cold differing considerably from the body-temperature will produce very slight change in the internal temperature. The same thing is observed as regards alcohol. In the experiments of Parkes and Wollowicz, moderate doses of alcohol appeared to influence very little the temperature of the body or the amount of nitrogen excreted. Traces of alcohol escaped, but there can be no doubt that the larger proportion was appropriated by the bioplasm of the blood, and afterwards eliminated in the form of excrementitious matters, especially carbonic acid and water. But in fevers and inflammations the mechanism by which this process of compensation is carried out undergoes deterioration or is seriously damaged, so that artificial efforts are required to put it into operation at all. In health it works perfectly well without the introduction of any stimulus from without.

The beneficial effects of external cold in febrile conditions have been long known, and cold was practically employed in the treatment of fever by Currie so long ago as 1789.

Of late years the more careful employment of the thermometer in disease, as proved to be of great value, especially by the observations of Traube, Wunderlich, Ringer, and others, and now carried out by the great majority of Practitioners, both in public and private practice, has, as might have been anticipated, led to a revival of the treatment of fevers by cold. In order to form a correct judgment concerning the influence of cold upon the phenomena of fever, I would refer to the results which have been observed to succeed its application in cases of extreme severity, and I doubt if any more to the point could be selected than those which have just been published by Dr. Wilson Fox.* I desire more particularly to call attention to these cases, because they also afford an illustration of the free use of stimulants in the very last stage of desperate fever, at a period of the disease when the temperature was so high that a fatal termination was imminent; indeed, it is almost certain that death would have occurred had not the most decisive measures been promptly taken.

Excessive and very rapid rise in temperature is perhaps seen more frequently in acute rheumatism than in any other febrile disease. In very severe cases of this affection it is not uncommon to find a rise of 4° or 5° within a period of a few hours, death usually taking place a short time after a temperature of 107° has been reached, and sometimes before so high a maximum has been attained. The high temperature may persist for some time after death, and a further (post mortem) considerable elevation has been witnessed. It is, indeed, seldom that recovery occurs after the temperature has reached 106° in a case of acute rheumatism; but Dr. Fox gives a full account of two remarkable cases of the disease which recovered after a temperature of 107·3° and 110° respectively had been attained. Both these cases were treated by the application of cold. In the first, when the temperature was 107°, the patient was immersed in a bath at 96°. The temperature, however, still rose until it was 110° in the rectum, when "ice was fetched; a large lump was placed on her chest, another on her abdomen; a bag filled with ice was tied down the length of her spine; and while two assistants bailed the warmer water out of the bath, two others poured iced water over the patient as rapidly as the pails could be filled." *Within half an hour the temperature in the rectum had fallen to* 103·6°, *and in less than another half-hour to* 99·5°. It is, however, worthy of note that in this instance the patient took *six ounces of brandy during the time* (one hour) that the ice and cold water were being applied, when the temperature fell more than 10°. Subsequently eighteen ounces of brandy per diem were given for several days.

In Dr. Fox's second case, also, in which the temperature was reduced from 107° to 98° within an hour, large quantities of brandy (from twenty-four to twenty-eight ounces in twenty-four hours) were given; and Dr. Fox expresses a doubt whether the man would have recovered at all had the stimulant been withheld.

In a case of Dr. Meding's,—quoted by Dr. Fox—the temperature fell from 108·6° to 99·5° *in five hours* during the application of ice-cold cloths to the body, and enemata of iced water given every half-hour. The pulse fell from 140° to 72°, perspiration ensued, and the patient rapidly recovered. In this case no stimulant at all was administered, but the temperature fell much more slowly than in Dr. Fox's cases. It was certainly slight as compared with the two cases reported by Dr. Fox, and many that I have myself seen, and treated successfully with frequently-repeated doses of brandy without recourse to the application of cold.

Dr. Wilson Fox considers that in acute rheumatism active treatment by cold should be commenced when the temperature reaches 107°, and all Physicians who have had much experience in the treatment of desperate cases of the kind will be disposed to agree with him; for although the opinion may be fairly entertained that in many cases the rise of temperature to the danger-point may be prevented by treatment, and particularly by the administration of alcohol, it appears to me to have been fully proved that cold (with or without stimulants) is the only remedy yet tried by which the rise in temperature can be not only quickly checked, but decidedly and rapidly reduced after the high body-heat of 107° has been reached. In moderately severe cases many will doubt if there is any necessity for adopting the treatment by cold; and it is certain that many most serious cases have progressed favourably, and have recovered, under other plans of treatment, and especially under the influence of alcohol. But in such very serious instances of disease as those reported by Dr. Wilson Fox, there can be no doubt that the application of cold should be resorted to. The objections that can be made to the plan are, in my opinion, far outweighed by the circumstance that, if not adopted, the patient will almost surely die, and within an hour or two.

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* "Treatment of H[illegible]ia." Macmillan and Co. 1871.

By merely reducing the temperature of the body, however, we do not remove the causes which have given rise to the increased development of heat. The rise in temperature is not the *cause* of the feverish condition, but a concomitant effect, or a consequence of a prior change—the increase of bioplasm. At any rate, in every one of the cases of fever that I have examined, both in man and animals, I have invariably found very considerable increase in the proportion of the bioplasm of the blood and tissues. If the bioplasm did not increase, there would have been no undue development of animal heat—no fever. This, then, seems to be the change of which the febrile or inflammatory state is an immediate consequence, and it is therefore of the utmost importance to consider carefully the various means at our disposal for preventing, or modifying, or cutting short the conditions which favour the occurrence of such a morbid change, or which may modify the intensity of the latter when it has occurred. It is also highly desirable to discuss the circumstances which, in severe forms of disease, occasion the fatal result, either suddenly or more gradually, by inducing structural changes which render impossible the continuance of life.

In all fevers and extensive general inflammations there is more or less failure of the capillary circulation, and the danger varies according to the degree of the disturbance, and the extent of tissues and important organs involved. In very severe cases, what we have to apprehend, and what our greatest efforts should be directed to avert, is *stagnation of blood in the small vessels, and cessation of the capillary circulation over a considerable part of the body.* To bring about this result, the following circumstances contribute:—

1. Failure of the force of the heart's action.
2. Alteration in the composition of the blood.
3. Growth of the bioplasm of the blood, vessels, and tissues.

1. *Failure of Heart's Action.*—In many severe attacks of fever the danger to life depends upon the weakness of the heart's action; and of those who succumb to fever, not a few are known to have had a weak heart, and that degree of power as regards the ganglionic nervous system which, although consistent with prolonged, steady, and equable work, is liable to fail if the demand for greatly increased work should arise. The cardiac mechanism may be well adapted for the ordinary requirements of the system in health, but, nevertheless, unable to bear a strain, and quite incompetent to discharge double duty even during a short period of time. There are many who are capable of performing steadily and constantly a moderate amount of labour without suffering, and may perhaps continue to do so, and without being laid up for even a short time, and may even reach in fair health the period of life when the power of active labour ceases, but who would be immediately deranged and in danger of being destroyed, if a double or treble amount of work were suddenly thrown upon them. In all cases of fever it is important that the Physician should endeavour to form an estimate of the heart's healthy power, for by good management we may be able to keep steady the force of the circulation throughout the whole period of the malady. During a critical period it may be necessary to excite the organ to increased action; and at another time it may be desirable to pour in nutrient food instead of stimulants, and thus slowly renovate the strength. There are cases in which, by the failure of the heart to propel the blood with sufficient energy through the capillaries, the patient's life is for a time in great jeopardy. By timely and active measures, we may succeed in exciting the heart to more vigorous action, and thus the patient may be successfully carried through a very dangerous period of the malady. In all cases of fever, it is important to watch very carefully for any indications of decided failure of the heart's action; but, at the same time, it is very necessary not to modify the treatment at every slight change that may be noticed in the force of the heart.

In slight febrile affections, the heart's action is usually sufficient for the work it has to do during the fever. If the feverish condition lasts more than a few days, and the heart contracts feebly, its vigour may often be restored by the administration of small quantities of easily digested food at very short intervals of time. Two or three teaspoonfuls of milk or strong beef-tea every two hours during the night as well as during the day may be given, and a little wine may be ordered if food alone does not have the desired effect. But if, in cases of fever which continue for a fortnight or more, we find the heart's action becoming decidedly weaker, it is necessary to stimulate artificially, at least for a time. A stimulating action is produced by certain remedies which act through the nerves. Stimulating liniments or turpentine externally will sometimes have the effect. Remedies which stimulate the olfactory and respiratory portions of the mucous membrane act in the same way; but the most direct as well as the most efficacious means is by the introduction of stimulants into the stomach; and these also act beneficially in other ways. Ammonia, chloric ether, and various kinds of spirits (when given not too much diluted) produce an immediate though indirect influence upon the force of the heart's contraction. This is proved by the fact that very soon after a little wine, brandy, or ammonia has been taken when the heart is acting very feebly, as during a partial faint, or from the influence of chloroform inhalation, the vigour of the heart's contractions becomes sensibly increased. The change is often noticed within a few seconds after the stimulant has come into contact with the mucous membrane of the stomach. This action, no doubt, depends upon the influence produced upon the ganglia of the sympathetic transmitted by afferent fibres from the skin, (mucous membrane) of the nose, throat, respiratory passages, or stomach, as the case may be. In consequence, the blood is urged with more force through the capillaries of the body generally, and the tendency to death from this cause is postponed, and perhaps averted.

2. *Alteration in the Composition of the Blood.*—In this place we have to consider how we may prevent the textures and organs of the body being damaged, and life destroyed, by the altered blood circulating in the vessels; and also to inquire whether we can, by judicious interference, promote and accelerate the restoration of the nutrient fluid to its normal state.

It was formerly supposed that the chief thing to be guarded against in the treatment of fever was undue oxidation; but later investigations have proved that fever itself is due indirectly rather to changes consequent upon insufficient oxidation; and it is, indeed, probable that a febrile state may be engendered by long-continued insufficiency of this most necessary change. It is doubtful if the feverish state could exist in an organism in which the oxidising processes were performed perfectly and at a proper rate. There is good reason to think that if the noxious materials which accumulate in the blood in fevers and extensive internal inflammations could be more fully oxidised, they would soon afterwards be eliminated in the form of urea, carbonic acid, and other excrementitious matters which at last result from the destruction of bioplasm and the oxidation of the products of its death; but while these excrementitious matters, or the imperfectly oxidised materials from which they are immediately produced, continue to accumulate in the blood, the excreting organs cannot eliminate them or alter them fast enough. The latter organs may in many instances be excited artificially to increased action; and it is in this way that many diuretics, sudorifics, and purgatives often afford great relief, and restore the patient to health. But in serious cases, in which the strength is already much exhausted, more especially if the fever has yet a long course to run, there would be danger in pushing too far the use of such remedies. Moreover, the excreting organs cannot excrete some of these unoxidised animal matters. In not a few instances in which the noxious materials have unduly accumulated, it will be found that the secreting organs *cannot be made to act;* as the afferent nerves are in part paralysed, our remedies are powerless. To give excessive doses in such cases would be very unwise, and by so doing we should render the condition worse. Under the circumstances indicated, however, there is danger of the imperfectly oxidised substances accumulating in the blood and in the tissues to such an extent as to place the patient's life in great jeopardy. And the danger is twofold. In the first place, many of the bodies are unstable compounds, and liable to decompose at the temperature of the body. The products of d[illegible]position set free in the blood would very soon destroy the living mat [illegible] od and tissues, and paralyse and destroy the nerve-fibres and nerve-centres, in which case the patient must die. Secondly, if the compounds which cannot be excreted in the form in which they exist are not to undergo decomposition, they must be quickly taken up and appropriated by living bioplasm of some sort. As the bioplasm of the excreting organs is already surcharged to such an extent as to damage the organs by increase of its bulk, and in other ways, the living matter of the blood, and then that of the tissues of the body, begins to appropriate the excess of pabulum. The white blood-corpuscles increase in size and divide and subdivide, the minute particles of bioplasm grow and increase in number, and the bioplasts of the tissues enlarge, and new centres (nucleoli) make their appearance in them. But these phenomena cannot occur without the action of the tissues being seriously impaired; and, worse than this, the increase of bioplasm in the blood inevitably leads to impeded capillary circulation, and to stagnation of the fluids in the substance of all the tissues of the body—the result of which, if local, must be destruction to the part of the tissue involved; if general, fatal to the whole organism.

But of the two circumstances just spoken of—the decomposition of albuminous matters, and the excessive growth of the bioplasm—the latter is by far the least dangerous, for the first is almost necessarily fatal, and rapidly so. But much of the noxious substances which have accumulated may be removed from the circulating fluid by the growth of the bioplasm, and temporarily stored in the form of living matter. Time may be gained for the excreting organs to right themselves, and that most favourable symptom in all cases—free excretion—may occur, in which case the surcharged bioplasm becomes at once reduced in volume, and is ready to appropriate more of the dangerous pabulum. The blood is thus relieved. Its bioplasm again takes up the excess of pabulum in the tissues, whose bioplasm gradually returns to its former volume, and there is every prospect of the normal balance of the nutritive and destructive processes being gradually restored. If however—as but too frequently happens—the proportion of the noxious matters already formed be very great, the bioplasm continues to increase unduly; and, as has been mentioned, this increase may lead to a fatal result. But still, the state of things established by the undue growth of bioplasm, unlike that resulting from putrefactive decomposition, is not immediately or *necessarily* fatal; and if by the process life is destroyed, that event is considerably postponed.

3. *Growth of Bioplasm.*—This, as will have been inferred from the remarks under the last heading, is a very serious change, characteristic of the febrile and inflammatory state, and it is that which most frequently leads to a fatal result, and when recovery takes place not unfrequently causes impaired health or chronic disease. The change was referred to in my "Report on the Cattle Plague" published in 1866, and before that time had been fully described in my lectures at King's College. The alterations are most remarkable, and have been figured by me in numerous illustrations. It is this process of undue growth of bioplasm, which from the first we must endeavour to control by treatment. Although it is some years since I pointed out this most important fact, and laid great stress upon the increase of bioplasm in all fevers and inflammations, it has not, I think, yet attracted much attention. The phenomenon has been discussed by me in several memoirs, and in this place I propose to consider the matter mainly in its practical bearing Not only shall I be able to show that the increase of bioplasm affords a highly important indication for treatment, but that many different remedies which experience has proved to be of use in febrile and inflammatory diseases are valuable, on account of the influence they exert in checking the growth of bioplasm and preventing the impending destruction of tissue; and although in too many instances the process has proceeded beyond our control, yet even in this case we can often retard the inevitable result.

In bad cases of fever and general inflammation the patient may die, as already indicated, from failure of a congenitally weak heart; but the life of those who have the advantage of a strong and vigorous organ is often destroyed at a later period of the malady by the excessive growth and multiplication of bioplasm in the blood, one consequence of which is plugging of so many capillary vessels of the tissues as to lead to complete suspension of their action, and to damage or destroy their structure. If life be preserved, the struct of the capillaries, nerves, and adjacent textures may be irreparably damag and in this way parts of organs of the highest importance to life may be altered that they can never regain their former healthy condition, in which c the organism will never be so sound, healthy, and vigorous as before.

In the tissues of many cases of death from fever of many different kin both in man and animals, I have seen the capillary vessels and small arte and veins completely obstructed by minute particles of rapidly growing plasm, and in not a few instances the minute vessels are dilated in the inter between two *constricted* points, which leads me to conclude that the biopl had actually increased in amount by growth long after the circulation had c pletely stopped. The walls of the vessels are much altered, and in some stances the bioplasts are five times as large as in the normal state, project much into the interior of the vessel, and dividing and subdividing freely—proliferating, as the saying is.

So far, therefore, from there being increased activity of the circulation (as ferred from the rapidity of the heart's action) associated with the hot fever state—as used to be supposed—the latter is characterised by restricted capill circulation and a tendency to complete obstruction of so many of the capilla vessels as to damage tissues and organs and to cause death. Now, it is t tendency to the increase of the bioplasm that he who treats fever must endeav to avert; and one thing which contributes in a most important degree to eff this end is the maintenance of the force of the heart's action, as has been ready pointed out. By promoting free circulation, so as to keep the whole ma of the blood constantly moving, and mingling, and changing, not only is t growth of its bioplasm impeded, but the bioplasm already formed is exposed oxidation and other changes, which lead to its disintegration, to be soon fo lowed by the removal of the products of its decay. Whenever the circulation flag the increase of the bioplasm is favoured, and the first abnormal augmentatio takes place in those organs, such as the liver, spleen, and lymphatic glands, i which the circulation is slowest in the normal state, and where the bioplasm is re newed according to the very moderate demands of the system in health. But be sides this advantageous effect, the free action of the circulation renders possibl the removal of many noxious materials tending to produce a paralysing or po sonous influence upon the nerves distributed to the capillaries, small arteries and veins of the skin, urinary organs, and bowels, and the nerve centres wit which they are connected, upon the integrity of which the condition of health i absolutely dependent.

Such, then, so far as I have been able to make out, is the interpretation the phenomena which may lead to the destruction of life if bad cases of fev are left to pursue their natural course. "Nature," it seems to me, manifes neither a conservative nor reparative action during the course of fever, an death may result long before the period arrives when natural repair becom possible. But, then, can "Nature" be held responsible for the developme of the feverish state? The natural history of febrile diseases is pretty we known by this time, and as in many cases we are liable to anticipate increase feverishness, we may place the patient in a more favourable condition to wit stand it than "Nature" unaided can achieve, and we may mitigate the force a blow, although we are powerless to ward it off.

"Expectant Medicine" in severe cases of fever is not justified by the fac known concerning fever, and an expectant attitude will no more save life th it will extinguish fever-poison, effect sanitary improvements, or preserve peo in a state of health which will enable them to resist the influence of disea germs. Expectancy, as a principle, is no more justifiable than is the giving harmless pilules or coloured water as a practice. In the treatment of real d ease, mere passive expectancy means the denial of knowledge, the ignoring broad facts of observation and experiment, a contempt for the lessons taught experience, and a disbelief in all that has been handed down to us by th who have observed, and laboured, and thought before we lived.—*Medical Tin and Gazette, Dec.* 16 *and* 30, 1871, *pp.* 731, 789.

7.—ON THE ADMINISTRATION OF STIMULANTS IN FEVER.

By Dr. Lionel S. Beale, F.R.S., Physician to King's College Hospital.

Alcohol.—The mode of action of alcohol upon the organism during the febrile state is very complex, and before discussing the nature of the modifications in the pathological changes probably effected by it, it is necessary to refer to the great distinction between the two objects for which wine and other stimulants are given during illness. Alcohol is prescribed—1, For the purpose of promoting digestion, improving the appetite, and relieving unpleasant sensations about the stomach; and 2, With the view of directly influencing those most active and serious abnormal changes which are taking place in the blood and in the tissues in all bad forms of fever, which, if they progress beyond a certain degree, will certainly lead to a fatal result.

I propose to defer the consideration of this latter part of the subject until the action of alcohol in moderate doses in the healthy state and in cases of slight fever has been discussed. The forms in which this substance is taken are very numerous, and nothing is more remarkable than the capriciousness exhibited by different stomachs as regards the reception of alcohol. Some persons like and can take without suffering, any form of alcohol. With others beer and malt liquors agree well—better than wine or spirits. A certain number can even take porter, but not ale, or *vice versâ*. With some dry sherry is the only wine that will agree. Port wine suits others; while not a few prefer, or can only take without suffering from derangement of the digestive organs, certain hocks or clarets, or perry or cider. Brandy or whisky diluted will often agree when every other kind of alcoholic drink fails; but even pure rectified spirit properly diluted will not always be absorbed by the stomach without exciting discomfort and favouring the development of unpleasant gases, with certain organic acids, among which butyric, acetic, and valerianic are found.

No one has yet been able to give any satisfactory explanation of the fact that a little wine will occasion in some stomachs the greatest disturbance. Within a few minutes, not only is the process of digestion stopped, but there is pain, an unpleasant feeling of nausea, not unfrequently accompanied by an actual desire to vomit. In other persons a glass of wine will occasion no inconvenience at the time, but may lead, in the course of from twelve to twenty-four hours, to the development of that unpleasant collection of symptoms which constitutes what is often termed a "bilious attack." Vomiting, purgation, and free diuresis afford relief; but sometimes the disturbance lasts for days, and is not allayed until the stomach has had twenty-four hours' complete rest from work, or until free action of the alimentary canal and all the glands that pour their secretions into it has been promoted by a dose of mercury. It is, after all, not improbable that this most unpleasant action of alcohol indicates a highly sensitive but not unhealthy action of the nerves of the stomach, and that tolerance of wine and spirits is due to a change which has been induced in the finest nerve fibres—in consequence of which their sensitiveness has been impaired. The tolerance of opium, tobacco, and some other poisons is probably to be explained in the same manner. Nor is tissue change limited to the nerves of the stomach; for it is an unquestionable fact that many of those persons who habitually subject their tissues to the influence of alcohol and tobacco, or both, at an early age, exhibit very distinctly signs of change in many tissues of the body. They look older; and indeed, physiologically speaking, their tissues are considerably older, and have deteriorated in a much greater degree, than would have been the case if they had not been exposed to the action of alcohol.

It is very remarkable how great a difference, as regards the capacity for the assimilation of alcohol, is observed in the same person when in ordinary good health, and when suffering from even a slight cold. I have observed this many times myself. When in health a very small quantity of wine will disagree, and not unfrequently give rise to a serious disturbance of digestion; but when one feels depressed and miserable from a feverish cold, three or four glasses of wine may be taken within a very short time with benefit, and with a feeling of immediate relief. Persons accustomed to alcohol in one form may take w advantage some other alcoholic fluid during illness.

If at the outset we have any reason to apprehend that an attack of fever going to be severe, it is very desirable to administer small quantities of alcoho early in the disease. In this way the stomach may be accustomed to the remedy; whereas, if its use is postponed until the patient is very ill, and alcohol required in very large doses, the stomach is often in so highly irritable a state as to reject it. The patient's life may be in jeopardy from this circumstance, or fatal exhaustion alone may actually destroy him.

Of Giving Alcohol to Young Persons.—My conclusions as regards giving alcohol to the young are in the main not at variance with the opinions of those who advocate extreme temperance. My own experience leads me to believe that the majority of young healthy people would do well without alcohol; and I believe the habitual daily consumption by young persons—even of a moderate quantity—of wine or beer, is quite unnecessary, and mere waste, while in some instances it is positively injurious to health. At the same time, there can be no doubt that in certain cases where the health fails in children, and even in infants, great benefit results from giving small quantities of wine daily for a short time. Hard-working people, students, professional men, and people actively engaged have been advised to take stimulants, as a general rule—and some, no doubt, require them; but I believe many would enjoy very good health without any alcohol at all, while the recommendation that they should take plenty of claret or other light wine is bad advice for several reasons. Not only is a bottle of light wine not required, but in many cases it is actually injurious. That people who can get it will often take a bottle of light wine, and more, is quite certain; but that they require it, or that it is good for their health, will not bear discussion.

Up to the age of 40 very little stimulant is, as a general rule, really desirable for healthy persons, and I expect most people of average health would get on better without any. My own personal experience is this :—I was never very strong, though always able to get through a very considerable amount of physical exertion without suffering from fatigue. Up to the age of 40 I hardly ever touched stimulants of any kind, and when I did take a little I not unfrequently experienced an attack of sick headache before my ordinary condition of health was restored. Lately, however, I have found the advantage of half tumbler of ale daily; and I can bear half an ounce, and sometimes three o four ounces, of wine without suffering. I dare say, as I grow older, I may like most persons, require a little more; but when in the country, and takin plenty of exercise, I feel very well and contented without any stimulants wha ever. The experience of some members of my family who have lived to b old, and that of many persons of whom I have enquired, accords with my ow In old age, I believe, stimulants are really necessary, and sometimes are eve more important than food itself. I feel sure the life of many old people is pr longed by the judicious use of alcohol, and I think that some, who have bee very careful all through life, take far too little stimulant when they grow old.

Of the Probable Action of Alcohol in the Body.—But we may now ver briefly consider the influence of alcohol upon the organism, and its probabl operation as an article of diet. What becomes of alcohol when it is taken int the stomach? There is no doubt that if the spirit is strong when introduce it is much diluted by the pouring out of fluid from the vessels and gland of the stomach, and that it is quickly absorbed, in its diluted state, into t blood. That this is so is proved by the familiar fact that the smell of alcoh is often very perceptible in the breath. Moreover, as is well known, alcohol h been detected by chemical tests in the breath, in the sweat, in the urine, an in the other secretions by a number of observers. Alcohol has also been pro ed to exist in the blood. There is, therefore, no doubt that alcohol, as alc hol, may not only be taken up by the blood, but may circulate with the nutr ent fluid, and eventually pass away from it unchanged. But it must not ther fore be concluded that *all the alcohol every person takes* is thus absorbed as cohol, caused to circulate through the body as alcohol, and at last excreted u *changed;* for such a conclusion would be opposed to the facts of observatio *and experiment.* The truth seems to be, that some of the alcohol taken is u changed in the system, but that a considerable and very varying proportion of the total quantity introduced is caused to disappear altogether as alcohol, and to pass through most important changes, escaping at last from the organism probably as carbonic acid and water.

A certain quantity of alcohol is *digested* and *assimilated;* and it is quite certain that the capacity for the digestion of alcohol varies very remarkably in different individuals. It is most probable that the alcohol is taken up by, and carried with, the portal blood to the liver. It is then appropriated with other substances by the bioplasm of the hepatic cells, and completely changed. Its elements are rearranged, and added to the constituents which form the liver-cell, and which gradually break up to form the ingredients of bile, the liver-sugar, and the so-called amyloid matter.

It is the living matter of the yeast-cell that splits up to form alcohol and carbonic acid, water, and a form of cellulose. We shall not be surprised to find that another form of living matter—that of the liver-cell—has the power of appropriating alcohol, rearranging its elements, and causing them to combine with other elements to form compounds having properties very different from those of the materials out of which they were made. And it seems probable that under certain circumstances other forms of bioplasm of the body are able to take up and appropriate alcohol; for it is certain that in some prolonged cases of exhausting disease a large amount of alcohol is readily assimilated, while ordinary foods can only be taken in such infinitesimal amount that we cannot attribute to them much influence in the maintenance of life. In severe cases of fever, as I shall again have occasion to state, the greater proportion of the alcohol introduced is probably not oxidised, as used to be supposed, but appropriated. Its effect is to lower, not to elevate, the temperature; and, so far from increasing the dyspnœa in bad cases of bronchitis, pneumonia, etc., by throwing increased work upon the lungs, as used to be affirmed, it has a directly contrary effect.

Dr. Parkes has shown that diluted alcohol, given daily in such proportions that not more than two ounces of absolute alcohol are consumed in the twenty-four hours, in most cases improves the appetite, and slightly quickens the heart's action; but that larger amounts have an opposite effect as regards the appetite, and greatly increase the cardiac beats.

Anstie and Dupré showed that if doses of alcohol sufficiently large to produce narcotic effects are taken, alcohol escapes in the excretions, but when smaller quantities are taken it is not to be detected. This may be the true explanation of the fact that alcohol in certain cases cannot be detected in any of the secretions at all. It is certain that the quantity required to produce narcosis varies greatly in different individuals, and perhaps this may account for the different results obtained in the course of different experiments.

Dr. Dupré has quite recently proved that, of the alcohol taken in moderate doses (48 to 68 grammes of absolute alcohol), only a minute fraction is excreted as alcohol, while by far the larger proportion is disposed of in the system in some other manner. Dupré's observations show that this alcohol is not stored up in the system as alcohol, and slowly evolved in the form of alcohol. He remarks that the amount of alcohol eliminated per day does not increase with the continuance of the alcohol diet, and that, therefore, all the alcohol taken daily must be disposed of daily, and converted into some other substance in the system.

We must therefore conclude that, of the alcohol taken, only a small but very variable amount is excreted as alcohol, but that the larger proportion, at least in the case of most organisms, is changed in the system; not simply acted upon by other things in a state of change, as may be effected out of the body, but actually taken up by the living matter or bioplasm, appropriated and converted into other substances. Though probably not applied to the nutrition of tissues, its elements may perhaps assist to form some of the constituents of bile, sugar, fatty, and amyloid matter.—*Med. Times and Gazette, Mar.* 2, 1872, *p.* 245.

8.—THE BOWEL LESION OF TYPHOID FEVER; ITS NATURE AND TREATMENT.

By Dr. T. J. MACLAGAN, Dundee.

[Dr. Maclagan's views on the bowel lesion of typhoid fever are considerably at variance with those usually entertained on that subject.]

This lesion consists in inflammation and ulceration of the agminated and solitary glands of the small intestine. These glands are scattered over the last few feet of that portion of the bowel, increasing in number as we pass downwards, until at the extremity of the ileum they are so numerous and closely aggregated together that but a small part of the mucous membrane remains free from them. They are embedded in the submucous tissue and covered over by the mucous membrane; they have no external opening, but are blind, ductless glands. The solitary ones are also found sparsely distributed over the large intestine, diminishing in number the further we pass down from the cæcum.

The nature and function of these glands in a state of health have been matter of some difference of opinion; some have supposed that they are secreting cells, which peoicdically burst and discharge their contents into the intestine; others regard them as absorbent glands, which have no share in the production of the intestinal contents, but are somehow engaged in the elaboration of the fluid which is ultimately carried away by the lacteals. The latter view is the more generally adopted, and is, no doubt, the correct one. The appearance which these glands present under the microscope is very similar to that presented by the medullary cords of the lymphatic glands; and Brucke succeeded in injecting them through the lacteals—a proof of their connection therewith. Their position in the intestine also supports this view; for if they were secreting cells, and if their function consisted in supplying to the intestine something requisite for the digestion of its contents, they would be situated high up in the gut (as are Brunner's glands and the bile and pancreatic ducts), where their secretions would be brought into play and utilised at an early period of the process of digestion. The sphincter ilei, or band of muscular fibres at the extremity of the ileum, probably has for its function the narrowing or temporary occlusion of the outlet of the ileum, and consequent detension of the chyle in contact with these glands.

The bowel lesion of typhoid fever consists in inflammation, sloughing, and ulceration of these agminated and solitary glands. What is the connection between this local lesion and the febrile symptoms? Is the constitutional disturbance a mere symptom of the bowel disease, or is this a result of the circulation in the blood of the poison of the malady?

The generally entertained opinion is that the bowel lesion is the result of nature's efforts to eliminate the poison from the system; that the deposit which takes place in and around the glands is specific in character; and that its deposition in and elimination by these glands is the means of ridding the system of the poison which produced the febrile symptoms. The sole facts in favour of this view are—(1) that inflammation of these glands is an invariable accompaniment of typhoid fever, and (2) that the discharges from the bowels largely contain the poison of that disease.

Such a view of the nature of this important lesion I believe to be altogether erroneous, and that for the following reasons:—

1. The glands are possessed of absorbent functions only, and are in no sense eliminatory.

2. Even if they did possess eliminatory functions, it is very improbable that the work of elimination would be thrown to so large an extent on the glands at the lower extremity of the ileum, while those high up escaped altogether.

3. Were they eliminatory, they would not probably be destroyed (as they are in typhoid fever) while performing a duty so necessary to the well-being of the body.

4. Were the elimination of the poison by these glands nature's way of relieving the system, the deposition in them of the morbid material ought to be *followed by* an amelioration of the general symptoms of the disease. We know

that the contrary holds good, and that the bowel lesion increases rather than diminishes the general febrile disturbance.

5. The opinion that the poison is thus eliminated is at variance with the fact that mild cases of typhoid fever do occur in which the glandular lesion terminates in resolution, and in which the so-called morbid deposit is reabsorbed into the system.

For the above reasons I reject the views generally entertained regarding the nature of this lesion, and hold that it is a specific inflammation of the agminated and solitary glands consequent on the action of the poison of typhoid fever on these glands, and bearing to that disease the same kind of relationship that inflammation of the tonsils does to scarlatina, and inflammation of the mucous membrane of the respiratory passages to measles.

I believe also that the sloughs and discharges from the ulcerated glands carry the poison of typhoid fever, and are capable of conveying that disease from one person to another, just as the discharges from the mouth and nostrils in scarlatina and measles are capable of transmitting their peculiar poison. Nay, I go a step farther, and maintain that the contagious nature of these discharges is not done full justice to by a simple recognition of their power to convey typhoid fever from one person to another, but that *their propensity to evil is coincident with their existence, and that they can, and often do, impart the specific inflammation of which they are the product to the unaffected glands with which they come in contact in their course down the intestine.* I hold, in short, that in typhoid fever there are two forms of bowel lesion, a *primary* and a *secondary:* the primary lesions commence coincidently with the onset of the febrile symptoms; the secondary result from the inoculation of healthy glands by the discharges from those primarily involved.

The grounds of this belief are various, and may be briefly stated as follows:—

1. It is consistent with all pathological analogy that the discharge from a specifically inflamed organ applied to a like organ in a state of health should there reproduce that specific inflammation to which it owes its origin.

2. Examination of the individual lesions shows that they are of two kinds—one in which the deposit is scanty and the mucous membrane granular in aspect, and another in which the deposit is more abundant and the mucous membrane smooth: to the former Louis gave the name of *plaques molles*, to the latter that of *plaques dures.* Besides this difference in appearance and in amount of deposit, there is also a difference in the mode in which they ulcerate. In the *plaques dures* the process commences in the interior, and the mass sloughs out bodily; in the *plaques molles* the process of destruction commences by a superficial ulceration which eats its way inwards through the mucous and submucous coats, and may even penetrate the muscular and peritoneal. No explanation of this difference has ever been given. The real distinction between them I believe to be, that the *plaques dures* are the primary lesions, the *plaques molles* the secondary. The explanation of the difference in their external characters is, that in the primary lesions (*plaques dures*) the inflammatory product is slowly deposited in the glands and surrounding submucous tissue, and continues to be deposited for eight or ten days before sloughing takes place; while in the secondary lesions (*plaques molles*) the direct application of the virus and putrid discharges sets agoing a smart inflammatory action which terminates in ulceration before there is time for the deposition of sufficient inflammatory products to give them the external appearance of the primary lesions.

3. The glands of the lower part of the ileum suffer more than those higher up. When considering the normal physiological action of these glands, it was noted that they became more numerous the further down the ileum we went. This peculiar distribution has for its object to secure that no waste takes place; while the contraction of the sphincter ilei muscle retains the chyle in the lower part of the small intestine, the presence of so many absorbent glands in that locality provides against the passage into the large intestine of any quantity of the nutrient material which ought to be taken up in the small. The contents of the ileum in a state of health consist of such nutritious ingredients of the chyle as have not yet been absorbed, epithelium, and the excretory

matter which goes to form the fæces in the large intestine. In typhoi they consist of the same materials *plus* the sloughs and discharges fi affected glands situated higher up. In a state of health the numerous in the lower part of the ileum, in the exercise of their absorbent fu take up the nutrient material remaining in the chyle, and the rest of contents pass into the colon. In typhoid fever they take up the same n but along with it they absorb the virulent discharges proceeding fr affected glands situate higher up. The discharges from the sec lesions are as potent for evil as those from the primary. Hence it the lower part of the ileum suffers so much more than the upper; further down the small intestine we go below the highest affected the greater is the amount of virus in a given quantity of the contents bowel and the less the likelihood of any gland escaping. On careful nation of the diseased intestine after death, it is found that "the tion between the diseased and healthy glands is usually rather and proceeding downwards after the first diseased patch all are usua eased" (Murchison). This fact can be satisfactorily accounted for in n way than that which I have indicated. The greater abundance of these at the lower extremity of the ileum doubtless leads to a correspondingl er frequency of primary lesions in that locality; but for the expl of the fact that a healthy patch is rarely, if ever, found below eased one, it seems to me absolutely necessary to recognise the influ an agency which acts from above downwards, and which comes into pla upper margin of the disease. Such an agency can be no other than the and discharges from the affected glands, which, in their passage down tend to inoculate those below, but cannot affect those above the highes at which they are formed. An apparent objection to this view, that th near the ileo-cæcal valve are as far advanced as those higher up, is explain by the fact that the secondary lesions ulcerate much more rapidly t primary, and are in a week as far advanced in their pathological course primarily affected a fortnight earlier.

4. The extent of the intestinal lesion, as revealed by examination afte bears no relation to the severity of the abdominal symptoms during lif severity of the abdominal symptoms is judged of very much by the am diarrhœa. In cases in which the bowels are moved several times a sloughs and discharges are carried off as quickly as they separate, and their minimum of harm. Where constipation exists they are retained, have an increased chance of inoculating healthy glands. Where cons exists there may thus be produced an amount of local disease great would have existed had the abdominal symptoms been more marked, discharges carried off more speedily.

When considering the difference between the primary and secondary it was noted, *inter alia*, that in the primary lesions the process of dest commenced in the interior, the diseased gland forming a slough which s ed bodily, while in the secondary the destructive process commenced surface, and proceeded inwards. As a consequence of this mode of fo of the ulcers, the primary lesions seldom extend beyond the base of the —that is to say, they involve the mucous and submucous coats, reachi not penetrating, the muscular. In the secondary lesions the ulcerative commences on the surface, and proceeds inwards, rapidly destroying the and submucous coats, but does not have the same reason for stoppin and hence is more apt to invade the muscular and peritoneal coats. In t is to be explained the fact that perforation most frequently occurs in c which, up to the moment of its occurrence, the abdominal symptoms ha by no means marked. This accident, indeed, often happens in cases in whi symptoms have been altogether wanting. How is this? It seems ver paradox to say that perforation, the acme of all that is severe in the bowel is most common in cases in which the evidence of that lesion is almost gether wanting. Paradoxical as it appears, it is nevertheless true. ' planation of this fact is that the perforation is the result, not of a prima of a secondary lesion; for if in these mild cases the number of glands pr involved is comparatively small (as I believe it is) there remains a larg ber of sound glands liable to inoculation; and as the risk of perforation is directly as the extent and number of the secondary lesions, it follows that the probability of that accident occurring as a consequence of such a lesion bears some relation to the mildness of the attack. It is to be noted, too, that perforation generally occurs low down in the ileum, where we have seen that secondary ulceration is most abundant and most active.

Hemorrhage, too, when it occurs to any extent, usually proceeds from a secondary ulcer. So long as the mucous and submucous coats only are involved profuse hemorrhage is not likely to occur, there being in them no vessels sufficiently large for the production of such a result. When the muscular coat is invaded, however, the risk is much increased, for between the transverse and longitudinal fibres there run vessels of considerably larger size than those found in the submucous tissue. These vessels run a great risk of being opened into by an ulcer which eats into the muscular coat. This it is which imparts to even a slight hemorrhage occurring late in the case an importance which would not attach to it at an earlier period. It indicates a still progressive secondary ulcer, and there is no saying where such an ulcer may stop. It may open into other vessels, or involve the peritoneum in its destructive course.

The time when the sloughs are separating is generally, I had almost said universally, regarded as the period of greatest danger, so far as the bowel lesion is concerned. Experience shows that the danger from that lesion increases the longer the fever continues, and that from the beginning of the fourth week onwards it is at its greatest. Now it can scarcely be supposed that a slough, which is formed during the second week (as most if not all of them are), which is necessarily of limited size, and which is seated in a very vascular structure, should take a fortnight or three weeks to separate. Yet such a conclusion is the inevitable outcome of the generally entertained opinion. The position seems to me quite untenable. I believe that the whole process of inflammation and destruction of the glands primarily involved, and separation of the resulting sloughs, never occupies more than three weeks, and that the prolongation of the disease beyond that period, and all the consequent risks and dangers, are attributable to the formation of secondary lesions, the distinguishing characteristics of which are the rapidity with which they are formed and the greater tendency which they show to extend beyond the submucous coat. I believe that hemorrhage seldom, if ever, occurs before the primary sloughs begin to separate, and that the bleeding which occasionally takes place at that time is rarely a source of danger. It is generally very slight. Dangerous hemorrhage, as already explained, comes on late in the case, when the primary sloughs are detached, and when the secondary ulcers are still pursuing their destructive course, threatening the larger vessels of the muscular coat.

In the vast majority of cases in which death is attributable to the intestinal complication, the fatal event is the result of the destructive ulceration of the secondary lesions.

When a patient dies of typhoid fever within the first fortnight, death is due to the intensity of the poison and not to the abdominal mischief. In most cases in which the bowel lesion is the source of danger, the urgent symptoms are developed late in the case—seldom before the middle or end of the third week, generally during the fourth or fifth, by which time it may be presumed that the sloughs have separated from the primary lesions; the exacerbation of the local mischief being due to inoculation of fresh glands by the poisonous discharges of those primarily affected.

If the discharges are so virulent, why, it may be asked, do they not affect the solitary glands of the large intestine? In the majority of cases these glands do escape. This is attributable chiefly to their small size and sparse distribution, it being evident that the risk of a drop of virus bearing discharge lodging over a small solitary gland in the capacious large intestine is very much less than the chance of its lodging over a good-sized patch in the attenuated small intestine; in the latter, too, the discharges are retained for a time in contact with the glands by the action of the sphincter ilei, while in the former they are in constant motion.

The successive inoculation of individual glands by the discharges from those already affected offers a ready explanation of the protracted duration of many cases of typhoid fever. The simultaneous inoculation of a number of gland accounts for those sudden and often alarming exacerbations of the local an general symptoms which so frequently convert an apparently mild into severe attack. No matter whether the aggravation of the febrile symptom takes place before the cessation of the primary fever, or some days afterward (constituting a true relapse), it is due to one and the same cause, inoculatio of healthy glands by the discharges from those already affected. The alarmin symptoms which are often developed in the fourth and fifth weeks of typhoi fever are the result either of the absorption by glands which had hitherto es caped of the putrid discharges coming from above them, or of the destructiv ulceration of the secondary lesions. Occurring while the primary fever sti exists, such inoculation gives rise to more alarming symptoms than are observe in a true relapse, in which the second seizure is generally mild, probably i consequence of the diminished scope for intestinal mischief resulting from th destruction of so many glands during the primary seizure.

The age and mode of life of the patient have a material influence on th severity and course of typhoid fever.

Age.—As already explained, typhoid fever increases in frequency and severit from the earliest period at which it occurs up to the time at which the vigou and elasticity of the frame are greatest; when that period is passed it begi to descend, and becomes less and less common as years advance, till we fi that after fifty it is a rare and not very fatal disease. In this respect it presen a marked contrast to typhus, which so increases in severity and fatality wi advancing years that recovery is far from common after fifty, and is scarce to be looked for at sixty and seventy. The period of life during which typh is most prevalent and fatal is that during which the intestinal glands which a specifically affected in that disease are most prominent and active, and that its turn, may be said to be the period of activity of the reproductive orga Typhoid fever is therefore most fatal in early manhood.

The mode of life of the patient has a very important influence on the severi of the attack. All who have seen much of the disease in both hospital an private practice will agree with me that it is more fatal among the better classe than among the poor. Why is this? Hospital patients are not so well nouris ed, are often the victims of considerable privations, and seldom come unde notice till they have been ill a week or ten days; one would therefore be dis posed to think that they would be less able to bear up against the disease. I hav seen it stated (I do not remember where) that people in the higher ranks d not bear up against such a malady so well as those in a humbler sphere becaus being in the daily habit of using a more generous diet and a considerabl quantity of wine and other stimulant, they do not so readily respond to stimu lants when these become necessary to counteract the depressing influence o the disease. Such an impression I believe to be quite erroneous. A drunkar of course suffers more severely from fever or any other ailment than a temperate man, and is less easily kept up; but such patients are more common in hospital than in private practice. The greater fatality of typhoid fever amon the better classes is due to quite another cause. It is due, not to any difficult in getting them to respond to the action of stimulants, but to a really greate severity of the disease; the malady takes a greater hold of them, and the consequently suffer more from it, independently of all treatment.

We have seen that the glands which suffer in typhoid fever are absorben and that their function probably consists in taking up from the chyle somethin which the system requires for the maintenance of the vigour and elasticity o youth. Now it may be taken for granted that the richer and more abundan the chyle the greater will be the work which these glands have to do. Of cours the chyle will be richest in those who live best (*cœteris paribus*), and hence i must happen that the absorbent glands of the better classes of society will b more actively and constantly employed than the same glands in the poore classes.

It may be regarded as a physiological law that an organ's capacity for wor increases with the demands made upon it: frequent exercise increases the siz and strength of the muscles; obstruction to the flow of blood from the hear produces hypertrophy of that organ; stricture of the urethra produces thick ening of the muscular coat of the bladder; when one lung is in abeyance the other does increased work; cessation of the function of one kidney results in increase in the size and efficiency of the other. So it is with these glands; the richer chyle which passes along the alimentary canal of a man who constantly uses a highly nourishing and stimulating diet supplies more of that substance which it is their function to absorb than does the chyle of a man whose diet is sparing. The continued use of such a diet for a length of time must lead to increase in the size and activity of these glands. The condition thus produced is much more favourable to the development of the poison of typhoid fever than that which obtains in the poorly-fed hospital patient. Should this poison be received into the system of one whose glands are so highly developed there inevitably results a severer attack of the fever than the same poison would produce in one whose glands were smaller and less active. The greater fatality, then, of typhoid fever in the higher ranks of life is the result, not of any difficulties in the way of treatment, but simply of the greater severity of the disease consequent on the more highly developed state of the intestinal glands and the necessarily increased facilities afforded for the growth and reproduction of the contagium in the system, as well as the exaggerated dangers of the local mischief.

Chomel and Forget have both expressed the opinion that debility from destitution is a favourable circumstance as regards prognosis in typhoid fever. According to the views generally entertained regarding the nature of that fever, this statement is little better than a paradox; to say that debility from destitution positively favours recovery from a disease which kills by asthenia is to make a statement which requires the high sanction of such eminent names to prevent us receiving it with scepticism, if not actual incredulity. The explanation just given of the greater fatality of the disease in the upper ranks of society supplies also a reason for its being less fatal among the poor and destitute, and affords evidence of the accuracy of the observation made by Chomel and Forget.

Treatment.—It may readily be supposed that these peculiar views regarding the bowel lesion of typhoid fever have some influence on the treatment which I adopt. They do influence it very materially.

The opinion generally entertained by medical men in this country regarding the treatment of typhoid fever is that it consists in keeping up the strength of the patient, in procuring sleep, and in checking every tendency to diarrhœa. Regarding the desirability of keeping up the strength and of procuring sleep there can be no doubt. To the general treatment I shall not further refer. I wish only to direct attention to the treatment of the bowel lesion. There are two methods of treating this. One is that recommended by Bretonneau and Louis, and adopted by most French physicians: it consists in encouraging rather than checking the action of the bowels. The other is almost universally adopted in this country, and consists in checking every tendency to diarrhœa. Dr. Todd thus summed up the treatment:—"Restrain diarrhœa and hemorrhage in typhoid fever; and when you have fairly locked up the bowels, keep them so. Patients will go for four or six days, or even longer, without suffering inconvenience from this state of constipation." Such is the treatment adopted in ninety-nine per cent. of the cases of typhoid fever which occur in this country. Dr. Todd's opinion is that of the profession in general.

The treatment proper for the bowel lesion of typhoid fever varies in different cases, and in the same case at different times. To know what is best for the patient, we must know the probable condition of that portion of the bowel which is the seat of disease. The general opinion is, that inflammation and irritation of the mucous membrane, by causing an increased flow of the secretions, produce a tendency to diarrhœa which it is well to check, and if possible prevent. Granting the soundness of the pathology, the treatment is right enough. I hold, however, that the pathology is unsound, and that the practice of "locking up" the bowels is one which leads to a prolongation of the disease, and increases the risk to the patient.

If all the lesions were primary, and if there were no chance of the secondary inoculation of other glands, constipation might be regarded as a favourable sign. *The recognition of the occurrence of secondary lesions alters the whole* question, and is the true pathological foundation of the treatment which I have to recommend; it supplies, too, a more satisfactory reason than has hitherto been advanced for the success which has attended the French practice.

In the early stages of typhoid fever it must ever be borne in mind that in many cases resolution of the intestinal lesions possible, and that judicious management during the first ten days may cut short and render comparatively trivial an ailment which the patient's own indiscretion, or injudicious treatment on the part of the physician, may convert into a long and dangerous illness. So long as such resolution is possible, the patient must be treated for that result. He must be confined to bed, and absolute rest and quiet enjoined. It must be borne in mind that some of the glands of the small intestine are inflamed, and consequently more than usually prominent and tender. Under these circumstances, every possible source of irritation to them must be avoided: for this reason no article of diet should be taken which contains much indigestible matter; for the contact of this with the glands on its passage down the gut would only serve to irritate them more. *Physiological rest must also be obtained as far as possible.* For this reason too much nourishment should not be given. The object in view is rest to the affected glands: to attain this object we must give only such articles and such quantities of food as will be digested and utilised in the upper part of the alimentary canal, leaving little or nothing to be done by the inflamed glands. The diet therefore should be mainly composed of milk, given, not *ad libitum*, but in stated quantities and at stated times.

The bowels should be kept as nearly as possible in their natural state. Any tendency to diarrhœa should be counteracted by the addition of lime-water to the milk, and frequently repeated small doses of Dover's powder. Constipation (which is more common in mild cases) is to be obviated by some gentle laxative. I prefer the phosphate of soda to any other; it is not at all irritating, acts very gently, and, having very little taste, is readily taken dissolved in a little warm milk or gruel: from half a drachm to two drachms may be given once or twice a day as required. At this stage of the illness ipecacuanha is very useful: given in small doses—one-fourth to one-sixth of a grain—every two hours, it acts beneficially not only on the skin, but on the mucous membrane of the bowel. When there is much heat of skin it is best given in the form of wine combined with liquor ammoniæ acetatis. In administering laxatives, it is best to give those which act on the upper part of the alimentary canal.

Such are the means calculated to favour resolution of the bowel lesion.

In all cases after the disease has continued for a fortnight without signs of improvement, and in severe cases at an earlier period of the illness, the hope of attaining such a favourable result may be abandoned. Treatment must now be directed, not to the prevention of sloughing of the glands already involved, but to the prevention, as far as possible, of secondary lesions by the inoculation of glands which had hitherto escaped.

It must be remembered that we have to deal with an intestine in which there are a number of sloughing glands and a number of healthy ones. The liability of the latter to inoculation by discharges from the former evidently depends upon the relative position of the two sets; the higher up the bowel the inflamed glands are situate the greater will be the risk of secondary lesions. It is impossible to tell the exact position of the affected glands. We must act on the supposition, I had almost said the certainty, that there are some glands exposed to inoculation, and do what we can to save them. How is this to be done? Certainly not by shutting up in the intestine the noxious sloughs and discharges. For what is taking place during this quiescent state of the bowels? The sloughs and discharges are being constantly separated from the primary lesions, and the healthy glands situated further down are being exposed to the influence of a poison which has a specific action on them, and which is hourly increasing in strength and quantity the longer constipation exists. By encouraging and keeping up this condition of the bowels we positively increase what we fancy we are guarding against—extension of the bowel lesion. To prevent altogether the formation of secon[illegible]ions is impossible, but it is evident

at no means which tend to keep the discharges from the primary lesions in
e neighbourhood of healthy glands can have other than a deleterious effect,
d that any means calculated to ward off this danger must have for its object
e carrying off of the sloughs and discharges as soon as they are separated.
ence the objection to the practice advocated by Todd, and acted up to by
ost physicians in this country; and hence the reason for adopting the oppo-
e line of treatment. After the inflammation of the primary lesions has gone
to sloughing, the patient's welfare demands that the sloughs should be got
l of as soon as possible; this end can only be attained by keeping the bowels
en. If constipation exists it should be overcome by small doses of castor
l, phosphate of soda, or other laxative. Looseness should not be checked
less the stools are so frequent or copious as to affect injuriously the patient's
rength. It may be stated generally that so long as there are not more than
ree stools a day astringents should be withheld. The diarrhœa is not entire-
due to the actual lesion of the bowel, but is in part due to the irritation
used by the fetid discharges, which it tends to carry off. It is there-
re salutary so long as it is not so severe as to prostrate the sufferer.

Space forbids me to enter on the consideration of the question whether or
t the typhoid symptoms of enteric fever are at all due to the absorption of pus
putrid discharges from the bowel lesions. I would only remark that, while
ey are not due to that cause in every case, it is quite possible for such an ac-
dent to occur, and that the recognition of such a possibility is an additional
ason for preventing their undue retention within the bowel.

When diarrhœa is excessive it must be restrained, not stopped. For this
rpose, acetate of lead, dilute sulphuric acid, liquor ferri pernitratis, or other
tringent, with or without opium, may be administered. The liquor ferri
rnitratis is particularly serviceable; it is possible that some of its beneficial
tion may be due to a corrective action on the fetid alkaline contents of the
wel.

The opinions which I have expressed are at variance with those generally
tertained; but they are the result of years of careful study of the subject,
d of a large experience in the treatment of typhoid fever.—*Lancet, Jan.* 20
nd 27, 1872, *pp.* 75, 110.

—THE TREATMENT OF HYPERPYREXIA BY WITHDRAWAL OF HEAT.

By the EDITOR OF THE BRITISH MEDICAL JOURNAL.

he careful study of the temperature in disease within the last twenty years
more and more led to the conclusion, that in all febrile diseases the in-
ased blood-heat is in itself one of the greatest dangers to life; and this
viction has been the predominating idea in all the recent attempts at a more
ctive treatment of fever. As the revival of the use of the thermometer
k place in Germany, so has also the practical application of the principles
ived from its indications chiefly been worked out in that country. The
ool of the late Niemeyer stands foremost in the ranks of workers in that
d; but to E. Brand of Stettin, who first treated a considerable number of
hoid patients on the hydrotherapic plan, and to the enthusiastic proclama-
n of his success, is mainly due the impulse to the zeal with which the sub-
t has been taken up in the clinical hospitals all over Germany, and by which,
last, the cooling treatment of the pyrexial state, by the external application
cold, has been placed on a thoroughly scientific basis. Not only in most of
e German civil and military hospitals is this method of treatment now the
neral rule; it has also, to a great extent, found its way into private practice.
e statistics published by Jürgensen, by Liebermeister and Hazenbach, by
emssen and Immermann, and by many others, comprising over a thousand
ses of typhoid fever, treated in various hospitals in the north and south of
ermany, show that the mortality has, under this plan of treatment, been less

than half of what it had been previously, varying now only from three to ten per cent.

With such results before us we ought no longer to stand by as passive spectators, surrounded as we constantly are by various forms of continued fever. Typhus being of rare occurrence on the Continent, the observations on the effects of the cooling treatment in that disease are as yet not very numerous; but where it has been tried, the results were no less encouraging. There would be ample opportunities in the fever hospitals of this country to supply more extensive experience in this disease; and we would urge upon the managers of our various fever hospitals the great importance of giving this method of treatment a careful trial, on a large scale, in typhus as well as in typhoid fever. There might be further scope for its employment in scarlet-fever and small-pox, not to speak of cases of high symptomatic pyrexia, as in pneumonia, in which baths have recently been employed by Lebert and others.

[The treatment of fevers by the withdrawal of heat is nothing new in this country; it was practised by Dr. T. Currie, of Liverpool, at the end of the last century, with great success.]

In contagious fever, such as typhus or scarlatina, in which we may assume that the specific poison is thrown out on the skin, the effect of baths in diminishing the danger of infection must not be overlooked—a point to which Currie has already called attention. We must always bear in mind that this method of treatment is not directed against the disease itself, but simply against a symptom of the disease, from which a series of other symptoms depend. No means having as yet been discovered to neutralise any of the supposed poisons of the specific fevers after they have once begun their work in the system, we must content ourselves with counteracting, as much as possible, their effects, not only by carefully feeding the patient, but also by removing, as much as lies in our power, the chief symptom, which undoubtedly exerts a pernicious influence on the most vital tissues—namely, the febrile heat of the body. It is not, therefore, by some mysterious operation that, as some water-doctors in old and modern times would make us believe, the treatment by baths and other "hydropathic" measures acts. The effect of these proceedings is, it need hardly be said, a purely physical one, which can be analysed just as any other physical phenomenon. The subject of the animal heat in health and disease, and under various external influences, is, no doubt, a most complicated and difficult one to deal with; yet, although some important points are still undecided, it has, thanks especially to the labours of Liebermeister, been sufficiently cleared up for all practical purposes. The effect of a single cooling proceeding on a patient with high pyrexia has been found to be this. In the first few minutes of the external application of cold, the internal temperature, as measured in the rectum or axilla, rises a few tenths of a degree; then a more or less rapid decline of the mercury takes place, as the heat is being rapidly withdrawn from the skin and subjacent tissues. This diminution of the internal temperature does not stop with the termination of the application of cold, but continues for several hours afterwards, the blood giving up heat to the cooled surface of the body; and the difference between internal and peripheral temperature, at first great, becoming gradually more and more equalised. Then the temperature rises again, and the previous, or even a higher, degree of heat may be reached after some time. The degree of cooling in and after a bath—*i.e.*, the effect of the bath, which may amount to from two to eight degrees F., depends upon the temperature and duration of the bath, the temperature and size of the patient, the period of the disease and the time of the day —*i.e.*, the period of natural fluctuation of the temperature at which it is given. Practically, the principle of this treatment lies in a nutshell. We know that the fever-heat is a most dangerous agent; there exist certain means to keep it down, and we have evidence that these means prove eminently successful in practice. To keep the average daily temperature below a certain range—say below 100°—must, therefore, be the aim of our treatment. It is quite clear that such a treatment cannot be carried out safely without the constant use of the thermometer. The temperature of the patient must be observed *every two or three hours* during the day and night; and whenever a certain degree *is reached*, which according to the circumstances of the case may be fixed at 103°, or 103·5° in the axilla (103·5 or 104° in the rectum), we must interfere. The number of baths or other cooling measures which thus becomes necessary varies according to the severity of the fever, the efficiency of the proceeding, and the stage of the disease. In several of the recorded cases of typhoid fever, as many as twelve baths in the twenty-four hours have been necessary in order to keep down the temperature. In milder cases, four baths daily were sufficient. As the fever declines, the daily number of baths becomes less.

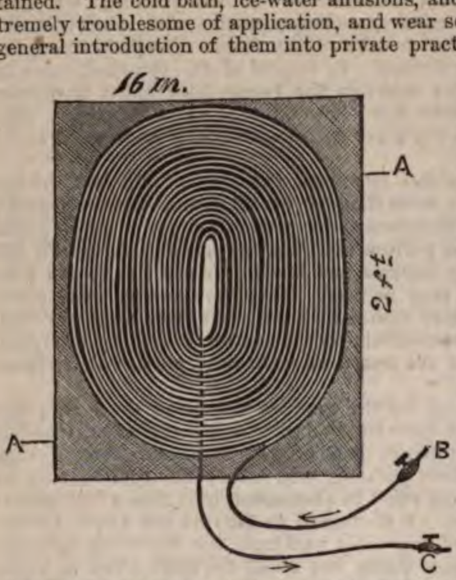
The only contraindications to this treatment are, first of all, hemorrhage from the bowels in typhoid fever, even if it be present in the slighest degree; perforation also precludes its continuance, for which, moreover, there would hardly ever be any indication, as peritonitis following perforation, even if life last, is generally accompanied by collapse and low temperature. Should it be high, ice-bags on the abdomen may be used. Some rare nervous symptoms, such as epilepsy or paralysis, may also forbid the use of the more powerful means of cooling the body. Menstruation, on the contrary, has not been found to necessitate the interruption of the treatment; nor does pneumonia or bronchitis contraindicate it.

Various methods have been used for the purpose of cooling the body; and it is of great importance to know their relative value. Liebermeister, who has experimentally approached this difficult question, ranges them in the following order; cold affusion, cold wet sheet packing, cold bath. The cold affusion, as practised by Currie, has much less effect in abstracting heat than a full bath of equal temperature and duration. With regard to the cold wet sheet, Liebermeister has found that a series of four cold packings, one following upon the other as soon as the sheet becomes warm, has about the same effect as a cold bath of ten minutes' duration, and double the effect of the cold affusion. Although some patients are fond of, and even ask for, cold baths, yet it cannot be denied that the majority of them have a dislike to sudden great differences of the surrounding temperature. This objection has been successfully removed by Ziemssen, who recommends to begin the bath with a temperature only about ten degrees lower than that of the patient, and then gradually, by adding cold water, to reduce the temperature of the bath to about 68°. Such baths are extremely grateful to the patients, who remain in them twenty-five to thirty minutes. By these various methods ample scope is given for adapting the treatment to the circumstances of the case. For further particulars on the various methods we must refer our readers to the treatises of Liebermeister and Ziemssen, of which the latter especially contains a good deal of practical advice.—*British Medical Journal, Feb.* 3, 1872, *p.* 131.

10.—ON THE TREATMENT OF PYREXIA BY A COOLING PAD.

By Dr. WILLIAM ROBERTS, Physician to the Manchester Infirmary.

At a meeting of the Manchester Medical Society, I exhibited, on October 4, a pad designed to apply regulated and continuous cold or heat to the surface of the body. The annexed figure will aid description. The pad consists of a close coil of very thin india-rubber tubing, three-eighths of an inch bore, cemented to a strong canvas backing. The terminal portions (composed of stouter tubing) reach a couple of feet from the margin of the pad, and each is closed by a stopcock, *B* and *C*. The inner end of the coil pierces the canvas backing at its centre, and passes behind the pad to its stopcock, *C*. For the purpose of experiment, three sizes were made—one a foot in diameter, another two feet by sixteen inches, and a third three feet by two feet. My principal object in constructing these pads was to obtain a convenient means of applying external cold in cases of fever and inflammation. The observations made in Germany on the treatment of typhoid fever by cold baths, and the more recent trials of the same treatment by Meding and Wilson Fox in the hyperpyrexia of rheumatic fever, seem to hold out hopes that we may be approaching a more effective treatment of pyrexia than has hitherto been attained. The cold bath, ice-water affusions, and iced cloths, are all of them extremely troublesome of application, and wear so heroic an appearance, that any general introduction of them into private practice seems well-nigh impossible. But if some such means of applying cold as I am now proposing could be made effective, it would open an easy way to the study of this method of treatment, and also make it available in private practice. The procedure I have followed has been to place the pad either under the back of the patient or round the trunk next to the skin. A stream of water of any desired temperature is then conveyed through the pad, as it lies under or around the patient's body. This is accomplished by placing a reservior of water, holding about six gallons, a foot or two above the level of the bed, and connecting this with the stopcock *B*. The most convenient form of reservoir is a tin can pierced near the bottom for a stopcock, to which a length of indiarubber tubing is attached for the purpose of connecting it with *B*. In the absence of a can of this sort, an ordinary toilet-can or large jug may be used, and the water made to flow from it into the pad by a suitable length of tubing arranged as a syphon. When the connection is made, and the stopcocks opened, a stream of water flows through the coils of the pad, and issues at *C*, whence it is conveyed by an exit-tube to a vessel on the floor of the room. In this way a moving sheet of water, of any desired temperature, is brought into near contact with the surface of the body—only separated from the skin by a layer of indiarubber of one-thirty-second of an inch thick.



Description of the Tubular Pad.—The coil is cemented to the canvas backing A A. The stream enters the circumference of the coil at B, and makes its exit at the centre, where the tubing pierces the canvas, and passes along the back of the pad to C, and thence to a vessel on the floor.

Before placing the pad under the patient, it is absolutely necessary to fill the coil completely with water. To accomplish this, the pad is placed on the floor of the apartment, near a table some four feet high. The can or jug which acts as the reservoir is then filled with warm water, and placed on the table. Connection is now made by means of the attached lengths of indiarubber tubing, and the current is set a-going, the exit tube being placed in a shallow vessel—such as a wash-hand bowl—on the floor. The empty coil soon begins to fill, and the air within it to be expelled. The stream should be allowed to run freely for some ten minutes, in order to drive out all the air from the coil. The two stopcocks are then shut, and the pad is ready to be placed under the patient. To prevent a too sudden shock, the pad should be first filled with warm water, and then gradually cooled by adding pieces of ice to the reservoir, or by filling it up with cold water as the warm water flows away.

My trials have already satisfied me that it is easily possible to reduce the temperature of the body by the means here proposed; but whether the method will prove of practical use in the treatment of fever and inflammation must depend on further experience. The subject is of such great and urgent importance, and demands such multiplied and varied experiments to test its utility, that I have thought it desirable to make public my results at once, in the hope that some other inquirers with more leisure at command will take the matter up and institute a more elaborate research.

[A number of tables are then given, showing the effect of the use of the pad on the temperature of the patient. This was always lowered by 2 to 3 or 4 de-

grees; sufficient for most purposes, but evidently not so effectually or rapidly as by immersion of the whole body in water. The temperature of the effluent water was found to be raised 20 ° when the stream ran freely, and 30 ° when it ran more slowly.]

It will be observed that in all the cases the depression of the temperature was only temporary. This has always been found to be the case after the use of the cold bath. It is also seen that as a rule the temperature rises after the withdrawal of the pad to a higher point than before the experiment began. But this appears to be only a temporary jump. The impression on my mind was very decided that the use of the cooling-pad was beneficial on the course of the disease.

It will be interesting to ascertain the effect of a more gradual and more sustained cooling, and to contrast the effect so obtained with the effects of a more rapid and more brief cooling frequently repeated.

In conclusion I must record my obligations to Mr. Buckley, Physician's Assistant in the Infirmary, to whose zeal and accuracy I am indebted for most of the temperature observations.[*]—*Medical Times and Gazette, Dec.* 16, 1871, *p.* 738.

11.—APPARATUS FOR APPLYING HEAT OR COLD TO DIFFERENT PARTS OF THE HUMAN BODY.

Designed by Dr. ALEXANDER ROBERTSON, of Glasgow.

These apparatus are constructed for the application of regulated temperatures, from that of ice to the highest that can be borne. They consist of two or more compartments, and have an inlet and an outlet-pipe from three to four feet in length, through which water is admitted and discharged. The current is regulated by a tap on the discharge-pipe, and may be either constant or intermittent. There is no difficulty in maintaining the temperature of the circulating water to within three or four degrees of a given point. They may be worked either in connexion with a vessel of water which has a tap at the bottom—the end of the inlet-pipe being attached to the top—or on the principle of the syphon, in which case no special vessel is required.

Head Bag.—It consists in some of eight, in others of ten, compartments, each a little more than an inch in diameter. These are arranged in parallel lines over the head, those at the sides being much shorter than the central ones. The whole set of compartments are cemented along part of their under surface to thin vulcanised cloth, which fits the head smoothly, and they are covered externally by thin woollen or waterproof cloth, to which thin vulcanised cloth is stitched or cemented along the margin. The cap is secured to the head by straps under the chin. When sufficiently distended it contains only about eight ounces of water; and if a free current be allowed to circulate, this will all be displaced by a fresh supply, admitted by the inlet-pipe, in less than half a minute. Dr. Robertson states that by circulating ice-water, or a solution of ice and salt, cold is more conveniently and efficiently applied to the head than by the ice-bag; and he hopes that by the use of water at definite degrees of heat a soothing influence may be produced on the brain in certain forms and conditions of disease.

Chest or Abdominal Bag.—It consists of six compartments, which are enclosed in the same way as in the head bag. When sufficiently distended the weight both of bag and contents is less than that of an ordinary poultice. It may be used alone, or on the outside of a light poultice or fomentation. The heat of the two latter agents may be renewed without in the least disturbing the patient, by simply admitting warm water at the inlet-tube, which displaces what is in the bag when the tap on the outlet one is open. Dr. Robertson says that besides preventing patients from being exposed and annoyed by the renewal of the poultice or fomentation, this bag is useful in certain cases of disease when a uniform high temperature is maintained for one or more hours continuo and he expects that it will reduce hyperpyrexia through the circulati ice-water.

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[*] The pads were made by *Charles* Macintosh and Co., Manchester. The price of the medium pad, *which promises to be the most* useful, is about 35s.

Uterine Bag.—It consists of two compartments. On the outer surfac small pouch for a director to assist insertion; and at the sides and are little loops, to which a piece of lint may be tied. It is rolled up le ways, and can then be pushed up behind the neck of the uterus witl greatest ease. After its introduction, water is allowed to flow into the expanding it, and then passes away by the outlet-pipe. Should a moi sedative fomentation be desired, the lint is previously soaked with v or other suitable liquid. According to the height of the column of wat the inlet-pipe, the bag may, or may not, be fully distended internally.

Dr. Robertson states that this instrument is useful in inflammatory ditions of the uterus, in certain states of uterine cancer, etc., when v water is circulated; and that in menorrhagia the cold of ice-water with pressure of the distended bag usually stops the bleeding.

Spinal Bag.—This bag is about a foot in length, but it may be long shorter, and consists of two compartments which communicate freely be Along their inner margin they are connected closely together, at interval bands. At the top, the one terminates in an inlet-, the other in an outlet- As in the other apparatus, the current through this bag may be either stant or intermittent. Besides its obvious use in applying either a low or temperature to the spine in ordinary inflammatory and painful affect Dr. Robertson thinks that the effect of maintaining an equable high perature in such diseases as tetanus, locomotor ataxy, etc., is worthy of b tested.

Throat Bag.—This instrument exists only in pattern. It is anticip that it will be found useful in laryngitis, &c.—*Medical Times and Gaz Jan.* 13, 1872, *p.* 55.

12.—ON THE TREATMENT OF HYPERPYREXIA BY WIT DRAWAL OF HEAT.

By Dr. T. Clifford Allbutt, A.M., Physician to the Leeds Infirmary, and Lecturer o Practice of Medicine.

[The water-treatment of fever was first "invented" in England; it is, how little known here, although an immense body of experience has been gath together on the subject by some German physicians.]

I cannot conceive anything more desperately risky than the casual pl ing of a fevered patient into a tub on the mere conclusion of a single vis even of one careful consultation. I feel satisfied, both from analogy and observation, that those physicians are in great measure right who contend such rapid withdrawal of heat is followed by a corresponding or a very siderable increase of heat-production. It is not yet familiarly known tha standard of animal temperature does not by any means represent a uni production of heat, but represents the difference between the amount set and the amount dissipated. Heat-production probably varies enorm from hour to hour, but heat-dissipation varies exactly as production vari long as the balance of the organism is steady; in like manner, if we inc the dissipation, we by the same movement increase the production, in virt the action and reaction between surface and interior. This, which is true t monstration in health, holds probably in great measure in disease also; so when we abstract heat largely, even in disease, we probably increase pr tion in something like a corresponding ratio, if not actually to the same de In plunging a patient with a temperature of 106°, say, into water at 60°, w up an enormous outflow, the body being quickly reduced, say, to 100°; *the* beautiful adjustment of the body being lost, we certainly do not s *continual* adequation of heat-production, but instead of numerous and infin *small* compensating oscillations, we seem to get fewer and larger oscillat so that the organism, when removed from the bath, tends to "react"—that is, to swing back to, and even beyond, its former place. Ziemssen seems to have seen this difficulty more clearly than any of the writers on the subject, and to have drawn from it the practical conclusion that if we are to modify reaction we must modify our withdrawal of heat, and rather coax than compel the organism to enter into an equilibrium. Hence he lays down a rule with which I cordially agree—namely, that the withdrawal of heat must be the slowest possible for the due attainment of our end. We know that a heat of 104° is slowly killing the intimate tissues of the body and arresting molecular movement, and that a heat of 106° is doing this rapidly. It is clear, then, that we must bring the body below this point; but this may be gained by using baths of a temperature far higher than 60°. Theoretically, it would seem sufficient to use a bath of 100°, and to cool it artificially during use to about 96°; practically, however, the temperature falls so slowly as to be scarcely perceptible when the patient is placed in a bath of 100°. I still think, however, that a bath of this temperature, which, when fully prepared, will stand but little above the normal blood-heat, is the best for a commencement, and that its temperature should then be kept down, and, indeed, slowly lowered during the immersion of the patient. This lowering must depend upon the state of the patient; but a long immersion and a slow lowering is in all respects, I am sure, more desirable than a short immersion and a rapid fall. If the patient can bear an immersion of an hour, or even of two hours, the lowering may be made very gradually, and need not be carried below 80°. On removal from such a bath, the further diminution of temperature which generally succeeds the immersion will not be followed by a violent reaction—that is, its effects, more slowly induced, will be more permanent. In addition to this treatment, directed to increase the dissipation of heat, I always endeavour to prevent its rapid reproduction by administering quinine in repeated doses of from five to twenty grains, according to circumstances, as advocated by Professor Binz.

The next point I would urge is the continued presence of a medical man and the incessant use of the thermometer. No case can really be counted as evidence either for or against the bath treatment in which the thermometer was not watched as incessantly and as anxiously, during the whole treatment, as the steam-gauge of a rotten boiler. To speak nearer to the point: the doctor can not take an observation, order a bath, and wait to take another until his next visit. The whole treatment must be managed with the utmost precision, and all tendencies to shiver or syncope should be watched by a skilled observer, or irreparable harm may be done in five minutes. No farther reasons need be urged on this important point; but I am satisfied, if more reasons are not wanted, that practical warnings, on the other hand, are much wanted, and will be needed more and more if the bath treatment becomes popular. If modern medicine is to be precise, and if its criticism is to be equipped with keener weapons, the awkward truth will begin to stare us in the face that much more will be required of us than the casual routine of the past.

[The following case occurred to the writer last September, and was fortunate in its results.]

Dr. —, a middle-aged man in large practice beyond Keighley, was under the care of Dr. Cockcroft, of Keighley, when I was called to him on account of the symptoms of "cerebral rheumatism," which had supervened upon an ordinary attack of rheumatic fever without heart complication. From the 11th to the 14th of September his state had gradually become very serious; sleeplessness and alarming head symptoms had set in. On the 13th, there had been incoherence and delirium; no true sleep for some days; and there was an increasing tendency to coma. Under these circumstances, on the evening of the 13th Dr. Cockcroft had shaved the head and applied large quantities of ice very freely to the head, forehead, and nape. This application had been followed by a slight but decided improvement. Hitherto no temperatures had been taken. I saw Dr. — on the afternoon of the 14th in consultation with Dr. Cockcroft, Mr. F. Greenwood, and Dr. —'s assistant. The patient was then unconscious and delirious in a low and *muttering* way, his countenance turgid and oppressed, *his pulse 130, and his temperature* 105·8°. There was no inflammation of the joints or heart. This condition was reported to me as one of slight i provement; and I had little doubt that the temperature had been lowered the free use of the ice. While obliged to give the gravest prognosis, I ga my full adhesion to the plan commenced already, and urged, on the groun before explained, that the withdrawal of heat should be systematically carri out under the guidance of the thermometer. As plenty of ice was at han and the use of a bath not so convenient, it was determined to fill wide-mouth bottles with ice and lay them along with the patient in bed. I think six bott were so applied at first, and their number modified subsequently according the temperature. The ice was also continued in a large bladder covering t head and neck, and dripping a good deal about the pillow. After settling these details and others of less immediate interest, I look my leave. Greenwood and the patient's assistant remained in the house and superinte ed all the proceedings, skilled supervision being absolutely necessary in t case. Mr. Greenwood kept and forwarded me the report of its progre and I here set down extracts from his diary. At 6 p.m. (about three hou after the commencement of the full ice treatment) the temperature was r duced to 102°; at 7, it was 101·4°; at 11, it was 100·2°; and at this tim the patient fell into a quiet sleep of four hours' duration, only broken by th occasional administration of food. The bottles were continued more or les according to the fluctuations of the thermometer during the day and night and I may pass on to Sept. 15th, 6 a.m., when the temperature was 99·4°. A this time the patient was intelligent enough to complain of rheumatic pains During the remainder of the 15th, the temperature fluctuated between 100·2 and 101·2°, the ice being regulated accordingly. He had "a very fair sort o night;" and I may sum up the observations of the next five days by sayin that the temperature was kept down to points between 100·2°, and 100·8° Sleep was not repeated in the same satisfactory way; there was much exhaus tion and therewith restlessness. In addition to the usual stimulants, morphi draughts and chloral were continuously tried to relieve this restlessness, bu produced only short disturbed dozes, with starting, moaning, and muttering and stertorous breathing. It was not thought desirable therefore to push th draughts. I should have added that during all this time, in addition to the ic bottles, the patient was regularly shifted from one bed to another, and sponge from time to time with cool water. No medicine was taken. On the 19th th symptoms of exhaustion began to pass off, and the hard fight was continue with renewed hope. On the 21st the temperature was easily kept to 99° 100°, and farther improvement was visible. During the next few days les and less ice was used until the 25th, when a steady temperature of 98°–9 was easily maintained without ice, and no more regular observations we made. From the 21st, indeed, the improvement was steady and satisfactor and the patient entered on the 25th into a convalescence which has since pr gressed favourably.

In this case, although the applications were very cold, on the other har their application was partial; nevertheless I am still disposed to hold that complete immersion in warm water is better than a partial application of ve cold water or ice, except in the coma of hot scarlatina, when douches or ve cold water to the head and neck are of great service.—*Lancet*, *Dec.* 23, 1871, 881.

13.—TREATMENT OF HYPERPYREXIA BY BATHS.

Under the care of Dr. Sutton, at the London Hospital.

The governors of the London Hospital, on the suggestion of their reside medical officer, Mr. Mackenzie, have made arrangements, on a scale of gr liberality and completeness, for carrying out the treatment of hyperpyrexia baths. In a small ward, containing two beds, there has been placed a bath, wi its head only against the wall, so that a patient can be lifted in and out of with the greatest facility. This bath is fitted with a large supply-pipe both hot and cold water, and a rapid waste. The cold water pipe comes from a tank, into which, if need be, lumps of ice can be placed, so as to reduce the temperature with the greatest rapidity. By these arrangements the temperature of the bath has actually upon one occasion been reduced fifty degrees in seven minutes. Not the least of the advantages of these arrangements is the fact that the treatment is robbed of all its terrors, and that the patient is not frightened by an array of buckets and lumps of ice, &c.

We append two cases taken from among Dr. Sutton's patients, which illustrate the mode of treatment. These cannot be spoken of as hyperpyrexial, neither of them having reached that temperature (106° F.) at which, in the opinion of Dr. Wilson Fox, the cold bathing should be commenced. It is interesting to remark that, in the case of rheumatic fever, a fall of temperature followed immediately upon the bathing; while, in the case of typhoid fever, large doses of quinine apparently accomplished (?) what cold bathing failed to effect. It will be remembered that in Dr. Wilson Fox's remarkable cases, which were both of them rheumatic fever, large doses of quinine utterly failed to reduce the temperature in any appreciable degree. In the first of these cases recorded below slight diarrhœa and albuminous urine seemed upon one occasion to follow as a consequence of the bath.

Case of Rheumatic Fever with moderately high temperature successfully treated by baths.—The patient, a girl sixteen years old, was admitted to the London Hospital on September 16th, on the twelfth day of her first attack of rheumatic fever. On admission her condition is thus described by Dr. Sutton: "Patient lies on her back, her mouth open, her eyes staring in a vacant manner; seems to pay no or very little attention to what passes around her. If spoken to she says, in a very excited, irritable manner, 'I want my mother.' Her nostrils expand very much during inspiration. Her breathing is laboured, and her cheeks are very flushed." There was a well-marked mitral systolic murmur. At 3·15 p. m. (her temperature being 104°, pulse 140, respiration 26) she was placed in a bath at the temperature of 96°. She was kept in the bath twenty-five minutes, the temperature of the bath being reduced to 83°. When the patient was removed her temperature was 102·9°, pulse 126, respiration 22. When first put into the bath she seemed very comfortable, but when the temperature of the bath was reduced she complained of feeling cold, and cried out a good deal. When removed to bed she said she felt very comfortable; she had lost her wild expression, answered questions rationally, and, closing her eyes, passed off into a sleep. At 9 p. m. the same evening her temperature again rose to 104·4°, pulse 126, respiration 30. She was bathed again as before, and in twenty minutes her temperature was reduced to 102·2°. At midnight on September 17th (temperature 104·1°, delirious and fretful) she was placed in the bath (temperature 98°, reduced to 82°), and when removed her temperature was 102°. When removed from the bath she was observed to shiver. After the bath during the night she passed three liquid dark-yellow stools. On the morning of the 19th her temperature was 101·6°; her general condition had improved; her urine was loaded with lithates, and contained a fourth part of albumen. From that date her condition gradually improved, and on October 17th she was sent into the country.

Case of Typhoid Fever with high temperature; the bath unsuccessful; large doses of quinine quickly followed by a fall of temperature.—The patient, a woman aged twenty-two, appeared to have reached the eighth day of a mild but well-marked attack of enteric fever. She progressed favourably in all respects until the end of the third week, when the evening temperature, instead of falling, showed a persistent tendency to rise. On the evening of the twentieth day the temperature in axilla was 104·4°, pulse 110, respiration 28. Between the twentieth and twenty-eighth days she had in all nine baths (the baths were similar in temperature and duration to those used in the last case, but the reduction was usually made to 70° instead of 80°). These baths had hardly any effect in reducing the temperature, which, at the most, within a few hours of the bath was as high as it was before. The first bath seemed more effectual than the last. Three quarters of an hour after her last bath the temperature was 104·8°. On the twenty-ninth day it was 105·9°, and on the evening *of the thirtieth 105·5°*. The administration of quinine was now commenced in doses varying from ten to twenty grains. A fall of temperature immediately followed the exhibition of the quinine, but the fall was very gradual and slow, and it was not till the forty-fifth day that the normal temperature was reached. With the exception of a slight return of pyrexia on the fifty-third day, which quickly yielded to quinine, the patient made a good recovery, and is now recruiting her health at the sea side.—*Lancet, Jan.* 13, 1872, *p.* 46.

14.—THE SYNTHESIS OF ACUTE RHEUMATISM.

By Dr. BALTHAZAR W. FOSTER, Professor of Medicine in Queen's College, and Physician to the General Hospital, Birmingham.

[The facts recorded in the following paper, when added to the arguments which have been adduced by Prout, Richardson, and other writers, will strengthen considerably the evidence which points to lactic acid as the poison of acute rheumatism.]

In the *British Medical Journal* of February 25th, 1871, I read with much interest an account of Dr. Cantani's observations on the lactic acid treatment of diabetes. At that time, I was engaged in completing an inquiry into the effects of different drugs on the sugar-excretion in diabetes. I determined to add one more drug to my list, and to complete my research by observing the effects of lactic acid.

A man (Wright) who had just come into the General Hospital under my care, suffering from diabetes, offered me the opportunity. His age was 31, and he had been ill some four months before his admission. By trade he was an iron-caster, and up to this attack of illness he had been a healthy man, and had never suffered from rheumatism. He was married, and had several strong, healthy children. On a mixed diet, he passed during the first week of his stay in hospital an average of 180 ounces of urine daily, containing 49 grains of sugar in the ounce. On a strictly animal diet, continued two weeks, the sugar fell to an average of 36 grains an ounce, and the urine passed to an average of 116 ounces daily. The skin was dry and branny. The sugar excretion remained pretty stationary on strict diet, but lung-symptoms began to manifest themselves, and steadily increased.

On March 8th, I ordered the patient fifteen-minim doses of lactic acid dissolved in an ounce of water four times a day. The dose was doubled the next morning, and in the afternoon he complained of acute pains in his joints, and flying pains about his limbs. In the evening, as these pains had increased, the medicine was discontinued by order of the resident medical assistant.

On March 10th, no lactic acid mixture was taken, and the pains gradually ceased.

On March 11th, I saw the case; and, regarding the occurrence of the joint-pains as a mere coincidence, repeated the lactic acid in fifteen-minim doses three times a day. On the evening of the 12th, he again felt pains in his joints; and on the morning of the 13th, "the small joints of the fingers of both hands, the wrists, and, in a less degree, the elbows," were noted by the resident medical assistant, Mr. E. A. Elkington, to have become "red, swollen, and painful." On my visit I was much struck by the appearance of these joints, which were typical specimens of acute rheumatic arthritis. In the evening, both wrists, the small joints of the fingers, and the elbows were all red, hot, swollen, tender, and painful. The heart-sounds were clear. The temperature in the morning was 100; in the evening, 101 F. He had moderate perspiration. Pulse 90, soft and full. The joints were wrapped in cotton-wool, and the lactic acid was discontinued.

On March 14th, in the morning, there was a decided improvement in all the joints; the swelling had much diminished, but heat and pain were still present. Temperature 100; pulse 84. In the evening, all the small joints of the fingers were much better. The wrists were still affected, and he complained of a good deal of pain in the knees, which had hitherto escaped. The heart-sounds were clear. Pulse 90. Temper

On March 15th, the joints were better. The temperature in the morning was 98·6; in the evening, 99·4.

On March 16th, he said that his arms were quite well; his legs nearly so. He had slept much better.

On March 17th, all pains in the joints were gone. Temperature 98·2. Pulse 72.

During the next twelve days, no lactic acid was administered. The case was put clearly to the man, and, as he had felt benefit from the acid mixture and had passed less urine during its use, he elected to run the risk of acute rheumatism. Accordingly, on March 29th, I prescribed seventy-five minims of lactic acid dissolved in twenty ounces of water. This was to be taken as a drink in the course of the twenty-four hours. During the next five days, no rheumatic symptoms appeared. The pulse rose twelve beats on and after the third day; the temperature, which had been previously elevated by the lung-complications, showed no marked change, but on the fourth and fifth days remained steadily at 99°, instead of varying, as it had done for some time previously. On the morning of the sixth day (April 4th), he complained of having had a bad night from joint-pains, which had disturbed him very much, and which came on suddenly after midnight. On examination, the metacarpo-phalangeal and first phalangeal articulations of the first and second fingers of each hand were found to be red, swollen, hot, and painful; the slightest movement aggravated the pain, and he could not on this account pick up anything with his fingers. The pulse was 102. The temperature, which on the previous evening had been 98·2, had risen to 99·4. The heart-sounds were clear. The acid mixture was stopped, and in the evening the pain in the knuckles was less, and the redness had diminished; they were, however, still stiff. No other joints were affected. Temperature 99·2.

April 5th. His hands were much better, and of his own accord, he resumed his lactic acid drink, and took about thirty minims of acid in the course of the forenoon. In the evening, the pains had returned in the knuckles, which were swollen, red, and tender. He discontinued the acid, had a fair night, and on the morning of the 6th, found his hands free from pain. He again resumed the lactic acid, and took up to 4 p.m. the remainder of the bottle, containing about forty-five minims of acid. In the evening, at 9 p.m., the pain and swelling had returned in his knuckles, and his left wrist was also affected. He now gave up the acid for two days, and the joint-symptoms gradually disappeared.

The acid drink was resumed on the 9th, and continued to the 13th, but he only took about thirty-five minims of acid a day. He experienced no inconvenience except flying pains about his joints, till the night of April 13th, when he was disturbed by severe pain in the right wrist, which was found in the morning to be red, swollen, painful, and hot, and was a typical specimen of rheumatic joint. Pulse 98, full and soft. There was copious perspiration, of acid reaction. The heart-sounds were clear. The elbows and knees became painful and stiff the next day. The joints were all wrapped in cotton-wool as before; and in the course of four days nothing remained except a little stiffness in the right wrist. After a week's interval, the acid was again taken, with like results.

The man now had gained so much experience as to the first indications of a coming attack in his joints, that he was allowed discretionary power as to the time and manner of taking the mixture. By trying it first in small doses, so as not to take more than twenty minims of acid a day, and stopping it for a day or so whenever the joints threatened, he managed to continue the acid for some weeks. Gradually he increased the dose, as advised, and early in June was able to take from forty to fifty minims daily. During this month, he had two sharp attacks of rheumatism in the hands and wrists. By the end of June he was taking seventy-five minims of acid daily; and on July 6th, this was increased to 100 minims. On the 7th, he began to experience considerable pain and stiffness in his joints, and kept his bed (he had been up daily previously) on account of the pain caused by walking. On the 8th, these symptoms were worse, and in the evening his wrists and elbows were very stiff and painful, but the knees were less so. The temperature had risen to 100·6. The acid was stopped. On the *next morning, he was better.* Temperature 99. The joints were less painful and stiff; there was no redness and no swelling. On the 10th, he again took the acid, his joints feeling much better, and the temperature being only 98·4. In the course of the day, he took 100 minims of the acid; and in the evening the pains had returned in his wrists, elbows and knees. Temperature 100·6; pulse 100, full and soft; skin moist and perspiring. On the morning of the 11th, his right wrist was red and swollen; the left less so. The knuckles of his right hand were also red, swollen and painful. His left knee was red, swollen and very painful and tender. He complained also of pain in the left side, but the heart-sounds were found to be clear; pulse 88; skin still moist. The mixture, which had been stopped on the previous night, was discontinued till July 17th, by which date all the rheumatic symptoms had subsided. After this the man only remained in hospital seventeen days. During this period, he, of his own desire, resumed the acid drink, and on one occasion took as much as 125 grains of acid in the course of twenty-four hours. During the last fortnight of his stay in hospital, he had no severe pains in his joints, and whenever flying pains warned him, he discontinued the medicine for a day.

While the above case was under my care in the hospital, it so happened that another diabetic patient of mine, in visiting the wards, met Wright and compared notes with him. From him he heard such a favourable report of the acid treatment, that he requested me to order him the same medicine if I thought it suitable. I did so. A drink consisting of seventy-five minims of lactic acid in a pint of water was prescribed. Of this he took daily as much as contained thirty to fifty minims of acid; and on the fourth day he came to me complaining of a sharp pain in his right knee, which rendered the joint stiff, and made walking very painful. He also mentioned that he had less severe pains in his other joints, and expressed his opinion that he had caught a cold, which had produced rheumatism, a disease from which he had never before suffered. There was no swelling or redness of the knee or other joints. His skin, which had hitherto been harsh and dry, was soft and moist. The acid mixture was discontinued, and in two days the pains had entirely ceased. During the next month, he made several attempts to take the acid mixture, but it was always followed in a day or two by pains in the joints. Early in May, he managed to take the mixture for a week, and then was laid up with such severe joint-pains, that I was called to visit him, and found him in bed with pains in his elbows, shoulders, ankles, and knees, and, as he said, all over him. None of the joints were swollen except the right knee, which was faintly red, decidedly swollen, and very tender and painful. The other joints were simply stiff and painful on movement. The skin was freely perspiring. Pulse 96, full and soft. The acid mixture was stopped, the joints were wrapped in cotton-wool, and alkalies administered. In the course of a week, all the symptoms had disappeared, and the patient was able to walk about, and resume his ordinary habits. This patient had never passed more than twenty-four grains of sugar an ounce while under observation. The excretion was generally not over fifteen grains an ounce.

Remarks.—The above record contains an account of the joint-symptoms which were observed in two cases to follow the administration of lactic acid. In the first case, at least six well marked arthritic attacks occurred; in the second case, under conditions less favourable for observation as to duration of treatment and place, one well marked attack occurred. The phenomena corresponded in all respects to those which are characteristic of acute articular rheumatism. They came on when the acid was taken, and ceased when it was discontinued. When moderate quantities of the acid were tolerated, an increase in the dose was succeeded by the painful inflammation of the joints. Coinciding with the development of the articular affection was the appearance of perspiration, at first only slight, but afterwards, in the more severe attacks, copious and acid.

These facts have dispelled the last lingering doubt in my mind as to the truth of the lactic acid theory of rheumatism. At first I doubted the connexion between the administration of the acid and the production of the rheumatic phenomena. In my scepticism, I regarded it as an accidental combination. The recurrence of the joint-symptoms, however, on March 13th, following distinctly on the repetition of the lactic acid mixture, shook my disbelief. The coincidence of joint-attacks with the use of the drug might occur once, and I thought even a second time; but, when I found it occur over and over again, there was no room left for the hypothesis of coincidence. To refer Wright's attacks to a series of accidental combinations requires, in my opinion, a much livelier faith than to accept the lactic acid theory of acute rheumatism. If to some, Wright's case presents not evidence enough in the beautifully typical character of the artificially produced disease, and in the precision with which it could be manufactured at the will of the experimenter, then the second case comes in to refute any explanation founded on the assumpton of an idiosyncrasy on the part of one patient.

In health, no doubt, much larger quantities of lactic acid than any given in my cases would be excreted without producing any perceptible disturbance in the bodily functions. The acid would escape by the skin, the kidneys, or, after oxidation, as carbonic acid and water. It cannot be justly argued that the quantities of acid taken by my patients were too small not to have escaped in this way. The conditions under which the drug was given must be borne in mind. In diabetes we have a state of suboxidation very unfavourable to the conversion by oxidation of new compounds; and in Wright's case this was aggravated by the serious pulmonary complications. Associated with these, there was a dry and branny state of the skin highly unfavourable to the elimination of the lactic acid by one of the common channels. Lastly, the well known persistent acidity of the urine in diabetes points to a pre-existing hyperacidity of the fluids. These considerations are, I think, important, as defining the conditions under which the experiments were made—conditions most favourable to the development of the specific effects of the lactic acid. It was the combination of all these which rendered Wright so susceptible to the action of the drug. By the absence of one of them (the lung-complication), and the minor degree of glycosuria, we may probably explain the slighter susceptibility in the second case. The larger doses of acid which Wright was able to take occasionally, towards the close of his stay in the hospital, find an explanation partly in his more careful management of the remedy, partly in an acquired toleration of it, and partly in the great improvement which occurred under treatment in the state of the respiratory organs and in the sugar-excretion.

I refrain for the present from discussing the bearings of my observations on the therapeutics of rheumatism. The effects of the lactic acid on the excretion of sugar will be considered, with other modes of treatment, in a future paper. In this communication, my object has been to lay before the profession facts which have an important bearing on the origin of a common and serious malady. If, by pointing out the nature of the poison of acute rheumatism, they help in the smallest degree to improve therapeutics, they will not have been observed in vain.—*British Medical Journal, Dec.* 28, 1872, *p.* 721.

15.—SOME NOTES ON THE TREATMENT OF SMALL-POX.

By Dr. William Stokes, Regius Professor of Physic in the University of Dublin.

[In reference to the probability of pitting in small-pox we must take largely into account the character of the disease. A case which is sthenic in character is more likely to result in pitting than one which is asthenic, with the same amount of eruption and confluence.]

Looking at the frequency of the occurrence of pitting on the face as compared with that of other parts of the surface, may it not in part be accounted for by the fact that while the rest of the body is kept covered by the bedclothes, and in a state of comparative humidity, the face remains in a dry and heated condition from the influence of the external air, and the increased vascular action. This we have seen sometimes so intense that no oily application could be used without immediately drying on the surface. Hard and hot scabs were formed, and the suppurative process made its way downwards to greater or less degree.

It was in the year 1849 that I saw the most severely inflammatory or sthen case that I have had to deal with before or since. There was great tumefa tion, accompanied with extraordinary heat of the face, and, in the hope of sa ing the eyes, poultices were applied over them. The patient recovered, b with deep and permanent pitting; but at the time when the crusts we formed ,with their usual dark colour, the countenance presented an ext ordinary appearance, the integument of each orbital region being free an almost white, while the rest of the face was everywhere covered with dee black crusts. There was no pitting on the eyelids, or in their immediate neigh bourhood. From the date of this case, I have adopted as a routine practic the application of light poultices over the entire face, or of a mask of lin steeped in glycerine and water, and covered with a corresponding mask of oil-silk, and have found, but with one exception, that pitting was effectually prevented. In that case the patient was delirious, and could not be prevented from tearing the poultices off his face. I believe that if from an early period we protect the surface from the air, and keep it in a permanently moist condition, marking will seldom occur.

Experience entitles us to hold that, other things being equal, the tendency to pitting, or, in other words, the virulence of the postulation, is directly as cutaneous vascularity and heat.

This method fulfils three important indications of treatment—

1st, The exclusion of air.

2nd, The keeping of the parts in a permanently moist state so as to preven the hardening of the scabs.

3rd, The lessening of the local irritation.

I need not remark that the value of the treatment is best seen in the inflam matory or sthenic type of the disease, although in the asthenic or typhoi form the occurrence of marking may be observed, particularly in the confluen cases.

In reference to the importance of lessening the local irritation, the followin case, to which I have alluded in my paper, is interesting:—A strong an healthy young woman was admitted with symptoms of an early stage of th ordinary epidemic fever, but of a very active type. The skin was hot and dr the pulse strong and full, and the tongue loaded. She complained of inten headache, much more severe than that commonly observed in the commenc ment of ordinary fever, so that I thought it necessary to apply leeches free to the temples. The headache, heat of the head, and flushing of the face we completely relieved in consequence. Within the next two days small-p vesicles appeared on the neck and bosom. The case proved a most severe e ample of confluent variola over the trunk and limbs, while on the face n more than two or three small aborting pustules made their appearance.

Who can doubt that in this instance the depletion of the face influenced t local progress of the disease?

This remarkable case, together with our knowledge of the good effect poulticing in lessening the irritation, and the consequent pustulation of t part, has lead me, even after the vesicles have appeared, to apply small nu bers of leeches once or twice to the inflamed skin, and with excellent resul The heat, vascularity, and swelling were much diminished and the pustulati clearly modified. We have here, then, a further evidence *that the activity pustulation is directly as the inflammatory state of the surface.*

It may be observed, parenthetically, that the influence of local depletion o the development in the part of the secondary lesions of essential disease, ma explain the theory, and, in some degree, excuse the treatment of fever, by th so-called physiological school. The benefit of leeches to the epigastrium an ileo-cœcal regions, so strongly appealed to in defence of the theory, may hav been that it lessened the preceding irritation which favoured the follicular en gorgement and consequent ulceration of the intestine.

In a paper in the *Quarterly Journal* for May, 1860, a case is given whic had been commented on by Dr. Graves. A man was for some time unde treatment in the surgical ward for a chronic affection of the knee-joint. I ca not say whether it was or was not rheumatic in character, but it was treated by strapping with mercurial plaster. After some time he showed symptoms of fever, soon followed by the appearance of variolous vesicles. He was transferred to the medical wards, and went through an attack of severe and confluent small-pox. At the period of the *stadium decrustationis* of Hebra the plaster fell off, when the whole region which had been covered by the straps was found smooth and of a glistening whiteness, contrasting strangely with the black and rugged surface of the limb above and below the joint.

In explaining this I incline much more to the effect of the pressure on the cutaneous capillaries than to any specific action of the mercury.

I have come to this conclusion, that in all cases, whether of the asthenic or sthenic form, the local treatment of poulticing is superior to any other. Two great conditions are by it fulfilled—one the lessening of the cutaneous irritation, the other the prevention of the drying of the scabs. It will be obviously more applicable in proportion to the inflammatory state of the surface.

In one case thus treated the result was satisfactory so far as the prevention of pitting over a large portion of the face was concerned, but the tardiness in the falling off of the scabs on the cheeks was remarkable; the character of this case was asthenic or typhoid. The eruption came out slowly, and was not well formed until after stimulants had been used. These, too, had to be employed liberally and for a long period of time. Many of the scabs remained strongly adherent for two months after the first invasion of the disease. As they fell off, however, the skin was found to be uninjured. When portions of the loosening scab were detached, long filamentous processes, very similar to *asbestos*, were discovered running from the under surface of the scab into the skin. At the date of this patient's discharge from hospital a few of the scabs were even still adherent. Could these filamentous processes have resulted from a pathological transformation of the purulent matter?

We have seen that the treatment by poulticing or other moist applications may be said to owe its success, 1st, to the diminution of the cutaneous irritation; 2nd, to the keeping of the pustules moist; and, 3d, to the exclusion of air. This leads us to the treatment by the warm bath practiced by Hebra, who seems to have been directed to it by observing its efficacy in the management of burns. It is clear that in the case of the continued warm bath we have the conditions just mentioned completely fulfilled, and that, too, as regards the entire person of the patient.

Not many years since one of our students, a very large and robust man, was attacked with small-pox, which soon showed itself in its worst characters. The fever at first was very high, and the head swelling and vascularity of the face intense. The *eruption was universal*, while the pustules on the face became confluent at an early period. Delirium set in, and the patient tore off the dressings from his face so often that we desisted from their further application. After the tenth day the condition of the patient was most appalling. The delirium continued, the circulation became every day weaker and more rapid, notwithstanding the free use of stimulants; the crusts were not only black, but on the legs, where here and there was less confluence, the blackness of the worst purpura appeared—a condition held by Hebra to be always fatal. The body was one universal ulcerous sore, and the agonies of the patient from the adhesion of the surface to the bed-clothes were not to be described. In addition to the usual fœtor of small-pox in the stage of decrustation which was present in the highest degree, there was an odour of a still more intensely pungent and offensive character, which seemed to pass through the bystander ke a sword. I never before or since experienced anything similar. Stimunts alone, free and constantly employed, seemed to preserve the patient ive; the pulse was rapid, weak, and intermitting, and for several days we espaired of his life.

At this juncture I happened to describe the case to my colleague Mr. Smyly, ho suggested the trial of the warm bath, with the view of relieving the terrie suffering. A bath in which he could recline was speedily procured, and llows being adjusted in it, we lifted the sufferer in and placed him in the cumbent position. The effect was instantaneous and marvellous. The deium *ceased as if by magic*; it was the delirium of pain, and the patient ex-claimed, "Thank God! thank God! I am in Heaven! I am in Heaven! why didn't you do this before?" The fœtor immediately and completely disappeared, so that on entering the ward no one could suppose that there was a case of small-pox in it. He was kept at least seven hours in the bath, during which time brandy was freely administered, and omitted only when he showe symptoms of its disagreeing with the brain. He was then removed to be The surface was clean, and in many places the sores looked healthy and whi The bath was repeated next day, after which he fell, for the first time, into tranquil slumber. From this time his recovery was progressive, delayed o by the formation of abscesses and the great soreness of the feet.—*Dublin Journal of Medical Science, Jan.* 7, 1872, *p.* 10.

16.—THE ANTISEPTIC TREATMENT OF SMALL-POX.

By Dr. ARTHUR ERNEST SANSOM, Physician to the Royal Hospital for Diseases of the Chest, and to the North-Eastern Hospital for Children.

[Though, of course, individual instances can have little value as evidence, the following case, to a certain extent, illustrates the fact that the course of zymotic diseases can be modified in intensity and in duration by the internal administration of antiseptic agents.]

I was called on October 13th, 1871, to visit Miss E. T., aged 18. She had high fever, subdelirium, and vertigo. The pulse was 128. Temperature, 105 deg. Fahr. The tongue was red, dry, with brown streak down the centre; the pupils were widely dilated. A few spots closely resembling those characteristic of typhoid existed on the abdomen, but there was no abdominal tenderness, distension, nor diarrhœa. The diagnosis was complicated, for the young lady had lately been staying in a locality wherein typhoid was prevalent. There was no lumbar pain. I gave a guarded diagnosis, but considered it most probable that the case would prove to be one of typhoid. I ordered cool sponging; milk and beef-tea; half an ounce of brandy every four hours; a scruple of sulphite of sodium in solution every four hours. On Oct. 14th, the pulse was 124; the temperature was reduced to 102 deg. Fahr. Several papulæ were present upon the face. The case now assumed the aspect of variola. On Oct. 15th, papulæ were abundant over the face, arms, and legs. On Oct. 16th, all signs of general discomfort had subsided; the patient only complained of irritation of the papules, which began to become pustular. On the 17th, I touched the centre of each pustule on the face (over sixty in number) with a fine camel's-hair pencil, dipped in strong liquid carbolic acid, taking care not to allow any to reach the sound skin, and ordered a solution of one part of carbolic acid in three of olive oil to be applied over the individual pustules night and morning. On Oct. 18th, the patient slept well, all irritation from the pustules having subsided. The carbolic liniment was continued; the surface of the skin was sponged now and then with oatmeal water. The temperature from this date never rose above the normal; there was no discomfort whatever; the pustules all dried up, and on the eighth day, a large number had completely fallen off, leaving no cicatrices; the others were quite dry and scaly. On Oct. 23rd, the sulphite of sodium was discontinued, and sulphocarbolate of iron given in five-grain doses three times a day. The surface of the body was washed with coal-tar soap to aid disinfection.

The double principle of the antiseptic method of treatment is the arrest of the disease-process in the individual, and the prevention of spread to the community. The same class of agents which common experience declare to be disinfectants, can be administered to the living body with at least a fair hope of their accomplishing that destruction of disease-germs which they accomplish externally to it. Carbolic acid has been administered by many, especially by French, physicians. But I believe that, in many cases, carbolic acid and its compounds can be advantageously replaced by other antiseptics—*especially* in those wherein head-symptoms are prominent. I think it very *probable* that we shall find certain antiseptics are best suited to the treatment

tain diseases; in scarlatina, diphtheria, and all zymotic ailments in which
roat is involved, I have found the sulphocarbolates especially valuable.
successful cases of variola, Dr. Hjaltelin employed the ordinary sulphu-
cid in fluid-drachm doses every third hour. I have rather inclined to the
the sulphites recommended by Polli—they are powerful and direct anti-
s, easily administered and readily absorbed. I believe that in this country
ror has been made of administering them in insufficient doses, or else of
ying the hyposulphites—purgative salts, and far less efficient as antiseptics.
external treatment of the pustules is most important. No agent seems
so valuable as carbolic acid; its application in the pure form to the sum-
f each pustule is perfectly painless. It is not necessary to touch each
dually at one visit, but at subsequent times to touch those which
been omitted previously. I have found nothing so effectually disguise the
of carbolic acid, without impairing its antiseptic efficacy, as oil of wild
e (oleum origani). Thymic acid is itself a well-known antiseptic. The
of the application of the carbolic acid is at once apparent; the pustule first
nes white, and then dries up. The carbolic oil afterwards applied, pene-
amongst the purulent crusts, and is far more efficient than any watery ap-
ion. The general surface of the body may likewise be sponged with any
le antiseptic. I believe the coal-tar soap to be very valuable for washing
urface of the body. By the antiseptic method of treatment, external as
as internal, the patient is really disinfected from the onset of his malady,
he benefits are manifest alike upon himself and upon those subject to the
gion.—*British Med. Journal, Nov.* 25, 1871, *p.* 611.

17.—ANTISEPTIC TREATMENT OF SMALL-POX.

By Dr. Arthur Wynne Foot, Physician to the Meath Hospital.

. Foot has recently communicated to the Medical Society of the College of
icians, Dublin, during their discussion of the subject of small-pox, a paper
h we deem worthy of being made as widely known as possible. A full re-
of the proceedings of this Society appears in the current number of the
in Monthly Journal, to which our readers may refer, should they desire
er detail. The subject is the "Antiseptic Treatment of Small-pox," and
e more important inasmuch as it introduces a principle into the chaotic
ties of treatment generally adopted. The ordinary plan of dealing with
l-pox, as with most other diseases which run a course, is to let it alone, to
symptoms as they arise, and to ward off evil effects should they make their
rance. In the ordinary sense of the word this is not treatment at all, and
re by no means sure that it is sound in principle. At all events, Dr. Foot
ttempted, with a certain degree of success, to apply a system which has
attended with good results in Surgery to a loathsome disorder, and one of
me severity. We are happy to make his ideas and his system more wide-
nown than they now are, and we hope that others will test both, so that we
know their true value.

e total number of patients treated was fifty-nine; but these were so care-
studied that their value is much greater than a multitude less closely ob-
d. Of the fifty-nine; twenty-four were confluent and six semi-confluent,
out of these eleven died—a mortality of more than one-third. But it must
membered that confluent small-pox in a severe epidemic is an extremely
malady, its fatality amounting to quite 50 per cent; moreover, the cases
ed were in badly fed patients, brought late to Hospital, some unvaccinated—
ort, a most unpromising series. Dr. Foot says—

The way in which I endeavoured to carry out the antiseptic treatment was
ving carbolic acid internally in the shape of the sulpho-carbolate of sodium,
when more suitable the sulpho-carbolate of iron, giving the sulphurous acid
e Pharmacopœia, diluted with water, as the usual drink, using gargles of
hurous acid, spraying the larynx with it, washing the nares and upper sur-
of the soft palate with solutions of sulphurous or of carbolic acid, keeping

carbolic oil to the face, washing the body with solutions of sulphurous acid or vinegar and water, throwing pure sulphurous acid about the bed and bedclothes of the patient, and burning sulphur in the room, so that the sick might breathe for some portion at least of the day an atmosphere charged with some sulphurous acid gas in it.

"After much consideration of the subject I have adopted the opinion that the secondary fever of confluent and semi-confluent cases is due to the presence in the body of products of decomposition, which commence to be formed as soon as the lymphy contents of the hitherto vesicles become purulent, rather than that it results from the dermatitis which springs into existence at that period, and which I consider to be the necessary consequence of the irritation of the now numerous subepidermic abscesses; and believing that in carbolic acid, used both externally and internally, there is an agent capable, when it can get fair play, of checking the decomposition of the pus, or of paralysing the effects of the products of its decomposition, I considered the first thing was to ascertain the best mode of its administration.

"I have given the sulpho-carbolate of sodium, in thirty-four cases of small-pox, in doses of from seven grains occasionally, to sixty grains every third hour; it is very soluble, and can be taken in plain water, or if its earthy-saline taste must be disguised, it can be given with some infusion of orange-peel or of cascarilla. During its administration carbolic acid is eliminated by the lungs, its odour being very perceptible in the breath, and the sulphuric acid and soda pass off by the kidneys. I have not observed it to cause any sickness of the stomach or unpleasant feeling in the head, even in very large doses; children have no objection to it. I have also verified the observations of Dr. Sansom, that subsequent to its administration the fetor of the evacuations from the bowels is greatly lessened, the urine is unusually slow to decompose, and the flesh resists putrefaction. I had opportunities of remarking the latter fact in making post-mortem examinations. I never found any one to complain of the usually nauseous variolous odour of the skin.

"At the same time, in all confluent and semi-confluent cases I use sulphurous acid in every form and way in which it can be applied.

"One of the simplest and most effective ways of exhibiting sulphurous acid is in the gaseous form; flowers of sulphur dropped on a heated shovel and carried about the room with its pale blue flame, forms by its combustion sulphurous acid gas, which, diluted with the nitrogen of the atmosphere, can pass into the lungs of the patients; and I consider that there is reason to believe that this practice three or four times a day is beneficial to the attendants and other inhabitants of the house as a prophylactic. Irritation of the bronchial membrane soon gives notice when there is as much sulphurous acid gas in the atmosphere of the room as is consistent with health; it specially and soonest affects anyone with bronchitis, and I should say it ought to be used very carefully if the variolous patient were labouring under that affection. One of the nurses at the Hospital who suffered from chronic bronchitis used to be greatly affected by the daily fumigations if in the rooms when it was being done, and used to feel its effects long before and long after any of the patients or other attendants. Some persons in health also have by idiosyncracy a condition of the lining membrane of the air-passages which makes them peculiarly sensitive to this gas, even in a very much diluted form, and in such a reflex cough is very quickly excited. It is not to be supposed that the principal object in burning sulphur is to disinfect the room—this is an after-consideration, and would require an amount of sulphurous acid gas dangerous if not fatal to life; but it is to develope as much of this antiseptic agent as may be safely inspired, with the view of checking the multiplication of the small-pox poison in the person of the patient, very minute quantities of sulphurous acid being capable of arresting fermentation.

"The sulphurous acid of the Pharmacopœia undiluted I frequently apply in an atomised vapour to the nares and pharynx, through vulcanite tubes, curved or straight as may be required, in the manner recommended by Dr. Dewar, of Fifeshire. I have found that patients like it, and eagerly ask to have the operation repeated. It removes disagreeable tastes from the mouth, keeps the nose free from obstruction by accumulated crusts, and much of it must reach lungs. A few whiffs open the nose when it is stuffed, or when, as hospital ients say, the head is stopped; the spray has not the suffocating odor of the tled acid.

'I give them the acid internally several times in the day, or for a drink both r and night when there is much thirst, in drachm or two drachm doses at a ie, diluted with water. One drachm of the acid in two wineglassfuls of iced ter is a very pleasant drink. Less water may be used—a drachm may be ten in a wineglassful of water; but if the acid has been freshly prepared, very well kept, the drinking such a solution may catch the breath. If the rson drinking the acid in the strength of one drachm to two ounces of water ll avoid inspiring through the nose when the glass is brought to the lips, and allow the liquor in gulps, the vapour cannot irritate the air-passages. I rays have the acid added to the water which the confluent and semi-conflut cases drink, and they like its acidity very much. I frequently take it rself as an agreeable and wholesome beverage, and advise the students to do , as a prophylactic. I prefer the sulphurous acid to the sulphites or bisulites, because they are so unstable in solution—the form in which they must be ren—rapidly absorbing oxygen, and passing to the state of sulphates. ither have they much to recommend them in preference to sulphurous acid the score of cheapness, purity, or flavour."—*Medical Times and Gazette*, pril 6, 1872, *p.* 400.

18.—THE HYPOSULPHITE OF SODA IN VARIOLA.

Dr. William A. Corwin, Assistant Surgeon U. S. Navy, U. S. S. Benicia, Asiatic Squadron, Harbour of Yokohama, Japan.

he value of the internal antiseptic treatment of zymotic diseases has, we believe, en clearly demonstrated. It was the knowledge of the action of sulphites arresting fermentation out of the body which first led to Prof. Polli's experints on their action on fermentation within the body.]

The U. S. S. Benicia arrived in the Harbour of Yokohama, Japan, November , 1870. At Yokohama, and at other prominent Japanese seaports, small-pox y be said to be *endemic*, occasionally prevailing among the foreign residents, l then assuming an epidemic form. It has so prevailed there during the past ter to an unusually severe extent, causing considerable mortality not only ong the permanent foreign residents, but also among the English and French ops stationed there, and the merchant and navy shipping in port. We were stined to have our share, the first case occurring in a seaman on the 18th of cember, 1870.

The patient was at once separated from the rest of the crew, until arrangents could be made for transferring him to the General Hospital ashore, ere the case proved confluent, and resulted fatally in a few days. Measures re at once taken to secure a suitable building for our own hospital purposes, l thus have our sick under the immediate care of our own medical officers.

A second case followed on Christmas day, and the disease gradually prossed until sixteen of the ship's company, including two wardroom officers, re "on the list." The cases were of more than average severity, four proving d. Of the latter all were confluent, three of the patients being men of inperate habits, and the fourth a negro, who died during convalescence from disease, from a sudden attack of œdema of the lungs.

A large roomy dwelling-house, selected with excellent judgment by Surgeon C. Nelson, U. S. N., and situated on the bluffs to the rear of the city, had n secured as a temporary hospital, and thither the cases were transferred as as they declared themselves, beginning with the second.

In the occurrence of the third case it occurred to me to try the effects of the *lphite* of soda; but this drug could not be obtained. So, with the concurce of Surgeon Nelson, the *hyposulphite* was employed in drachm doses, and had every reason to be gratified with the result. This treatment, used in *premonitory fever only*, was commenced with the *fourth* case, and its effects carefully watched. They were those of an *alterative*, mild *hypnotic* and *laxative;* its administration being in most cases followed by a subsidence of the fever, a tardy or incomplete development of the eruption, and relaxation of the bowels with watery stools. Upon the full development of the eruption the remedy was generally discontinued, and a supporting *regime* adopted, egg and brandy mixture, with easily digested food. The good effects of the salt were generally manifest after the first dose; the patient losing the heat and dryness of the skin, expressing himself as much more comfortable, and passing a good night. In two or three of the cases the eruption was delayed from twelve to twenty-four hours after the usual time for its appearance, and in one patient the eruption consisted of irregular erythematous patches with successive crops of minute vesicles in the flexures of the limbs. The average duration of the first three cases (fatal) was six days. Of the cases that recovered one was *malignant*, the rest of all degrees of severity. Their average duration was *twenty days.*

What I particularly wish to have noted is the fact, that in direct proportion to the early and free use of the remedy, really harmless for evil while so potent for good, was the disease ameliorated and its average duration shortened, and this in an epidemic of more than usual severity.

It may be objected that the cases enumerated are too few to establish the value of the remedy. This is more than is claimed for the results given. My only object is to add a trifle to the evidence constantly accumulating in the columns of the various medical journals as to the value of sulphur and its lower combinations with oxygen in the treatment of the zymoses, and to elicit, if possible, from the profession at large the results of wider observation and experience. (*Vide* note on tropical use of Sodæ Sulphis in Erysipelas, by Dr. Addinell Hewson, Transactions Coll. Phys., Phila., 1867; also note on Sodæ Hyposulphis as a Prophylactic in Scarlatina, Dr. N. L. North, Aitken's Practice, Am. Ed., page 318).—*New-York Medical Record*, *Aug.* 15, 1871, *p.* 267.

19.—DISINFECTANT TREATMENT OF SMALL-POX, FOLLOWED BY DISINFECTING BATHS.

By J. N. Stephens, Esq., Plymouth.

Having lately had charge of two small-pox hospitals in Plymouth, I have adopted the disinfectant treatment of Dr. Sansom, of London, viz., the administration internally of bisulphate of soda every four hours, and the application of olive-oil and carbolic acid to the pustules externally. By these means, he contends, that "the patients are themselves disinfected, and rendered innocuous to the community at large." I must say this plan has been attended by more favourable results than any other with which I had previously been acquainted, and can most strongly recommend it to my professional brethren. I have, however, to make certain that no infection should be conveyed to the public after leaving the hospital, caused each patient to be put for a quarter of an hour on three successive days into a bath containing a pint of chloralum. This has been done immediately convalescence has been established, and the patients have been discharged from the institution forthwith. By this proceeding much expense has been saved, and the beds made available for other sufferers in quick rotation. I have also employed chloralum in solution as an external remedy before the application of the olive-oil and carbolic acid. This has been the means of cooling the inflamed surface and of allaying the itching.—*British Medical Journal*, *Feb.* 3, 1872, *p.* 127.

0.—BALSAM OF COPAIBA IN SMALL-POX AND SCARLATINA.

By Dr. A. Rowand, Quebec.

From our knowledge of the effects of the balsam on the skin and mucous membranes, I was induced to try it, in four- or five-drop doses, mixed in ʒ ij. syrup, and ʒ ij. mucilage of gum arabic, three or four times a day, in the confluent small-pox of a person who had never been vaccinated. It caused no nausea, but, on the contrary, created a keen appetite, which continued till recovery. No pitting took place, and no local application was used but glycerine and water. I tried the same mixture in scarlet fever, with most satisfactory results. Under its use, the tongue and sorethroat got rapidly clean and well, followed by a keen appetite, and by none of the usual sequelæ. The secretion of urine was copious and began to increase in quantity after two or three doses. At first it was of the colour of ale and a little ropy, but by the third day quite clear and normal. My theory of the action of the remedy is, that it alters or destroys the character of the virus, and eliminates it out of the system by the skin and kidneys more particularly; for the recoveries have been unusually rapid. In both cases I prescribed milk, beef-tea, wine, and spirits, according to need.—*Medical Times and Gazette, Feb.* 17, 1872, *p.* 206.

21.—THE CORRELATION OF VARICELLA AND VARIOLA.

By Frank Thorp Porter, Esq., Dublin.

I communicate the particulars of two cases which illustrate the correlation varicella and variola. I consider that the two diseases are more intimately ated than has been hitherto admitted by pathologists. I shall also offer some marks on the treatment of variola generally, including the subject of re-ccination; and I shall conclude with some suggestions on a subject of public terest. I allude to the question of the speedy establishment of a suitable ilding for a convalescent home for small-pox patients, who suffer much from e want of such an establishment.

The first case is that of Ellen Lawlor, æt. 14, who was admitted into the uth Dublin Union small-pox sheds on January 18th, 1872. She was sent in y my friend Dr. Mapother, who diagnosed the case as one of varicella. On dmission she was weak, her countenance was dull, the pulse was 90, the owels were confined, and the tongue was white and moist. The primary vac-ine marks were visible. A few isolated vesicles, which wanted the areola and itted appearance of small-pox, presented themselves on face, neck, and arms. he only treatment resorted to was a mild purgation and generous diet with orter. She left hospital well on the 22nd of January, 1872.

The second case is that of the late porter at St. Vincent's Hospital, Michael yrne, æt. 37, who was admitted on the 17th of January, 1872. He stated at he felt in good health up to the 18th of January, and that after being in mmunication with the patient Lawlor, he was seized with headache and ivering fits, followed by loss of appetite, sleeplessness, pains in the bones, d heat of skin. On admission his pulse was 100, tongue of beef-steak colour, junctivæ were suffused, bowels constipated, urine scanty and high coloured. erythematous rash appeared over the body, followed by vesicles, which n became confluent, and purpuric, giving exit to sero-bloody pus. The only tment consisted of a purgative, wine, and beef-tea, with a sinapism to the s. His intellect was clear up to January 21st, when he became so delirious o require restraint. He died on the following day. The fact that he be-e suddenly ill after being in contact with the previous patient, clearly sug-ts that varicella may communicate variola. This is an important question investigation. I regard the two diseases as modifications of the same blood son; an opinion which is strengthened by the fact that they often appear same *family, and sometimes* in the same person.

With reference to the treatment of variola, every effort should be based the principle of isolation. A number of warm, well-ventilated huts would carry out this object better than sheds. Mild cases could be better treated at the patients' homes. Rigid enforcement of the Sanitary and Vaccination Acts, combined with a more extended system of out-door relief, would limit the spread of infection. I do not coincide in Professor Haughton's opinion that the use of spirits is prophylactic in epidemics. Teetotallers recover soonest. Increased facilities for taking baths should be placed at the disposal of the poorer classes, with a view to prevention. I have but little faith in drugs, although I think the muriated tincture of iron is beneficial in some cases. Flour or starch is a better application for the face than other preparations which cause stench. The use of brewers's balm lessens the tendency to abscess. I was led to try it from seeing its effects in anthrax; it seems to prevent putrescency. The bed sheets ought to be made of some carbolised material, such as the tissue used by Professor Lister in wounds.

With reference to re-vaccination, I have no faith in it. Not one of the thirty-six attendants at the South Dublin Union sheds have taken small-pox. Only seven of the number were re-vaccinated, and as the remaining twenty-nine enjoy the same immunity, wherein is the necessity of the operation? I have known gouty inflammation, abscess of the breast, and angioleucitis to result from the operation. I cannot, in the face of such facts, approve of it, and moreover the sense of the Profession is against it. It is only to be employed when there is no evidence of the success of infantile vaccination, and even then it seems to do more harm than good; at least so far as I have seen.—*Med. Press and Cir. March* 27, 1872, *p.* 269.

22.—TREATMENT OF CHOLERA BY SUBCUTANEOUS INJECTION OF MORPHINE.

By Dr. John Patterson, Surgeon-Superintendent of the British Seamen's Hospital, Constantinople.

In an article in this journal on the cholera in Cairo in 1865, I expressed regret at not having tried the subcutaneous injection of medicines, having special reference to quinine. A recent severe epidemic in parts of this city and at Hasskieu, a village on the Golden Horn, the residence of a large English colony, has given me an opportunity of trying the effect of the subcutaneous injection of morphine on a sufficiently large scale to judge of its value. The first cases were treated by the usual remedies; everything rational was tried, and with the usual want of success. Completely disheartened at the inutility of treatment, I went prepared with the instruments and morphine, and, after consultation with my colleague, Dr. Wherry, determined to give it a trial. A most unpromising case was selected. The man had been previously suffering from inflammation of the liver, was in deep collapse, pulseless, with rice-water purging, severe vomiting, and cramps. I injected a quarter of a grain of acetate of morphine. The result was beyond our expectations. In a quarter of an hour the cramps and vomiting ceased, the patient fell asleep, the skin gradually became warm and moist, the pulse returned. In two hours he awoke, and said he felt much better. The injection was repeated; he again slept for three hours. The reaction was perfect. He lived three weeks, and sank from typhoid exhaustion, as much produced by his old liver complaint as from the reactionary fever. The same good results followed in almost every case in which it was tried. In ordinary cases one or two injections of from one-quarter to one-half of a grain sufficed. In a few cases three injections were given, and only twice have I had occasion to give four. It was given even to very young children in doses proportioned to their age and condition. I speak more of well-marked cases. In the milder form, where the purging, vomiting, and cramps were severe, and collapse just commencing, we never wasted time on other treatment. An injection of a quarter of a grain was given, perfect quiet enjoined, and bottles of hot water placed in the bed. The patient fell asleep.

and, as a rule, awoke nearly well. Many cases were thus nipped in the bud. I do not, of course, maintain that this treatment is a specific against cholera; I only claim for it that its action is more decided than any other treatment I have seen or practised, and that in the race against death we gain time for further treatment when it is necessary. It is long since I lost hope of the ordinary treatment influencing much the course of the disease, and, after treating more than a thousand cases at various times of epidemic, I am glad to report that this has really been of great service. My colleague, Dr. Wherry, speaks equally favourably of the results obtained in his practice. I regret that want of time prevents me at present from giving the cases in detail; but the subjoined table gives at a glance the main results:—

	Number of Cases.	Recovered.	Dead.
Treated in the usual manner	10	1	9
Treated by morphine injections	42	22	20
Total .	52	23	29

But of the cases treated by injection, 8 were perfectly helpless from the first, being *in articulo mortis*, 1 had severe liver complaint, and 1 was far advanced in consumption; so that, in reality, we had 32 cases where the treatment had a fair chance, reducing the mortality to 10 in 32; and of these ten, 1 was 60 years of age, 1 within a few days of her confinement, and 8 hard drinkers.

In reference to the subject of cholera, I may here state that I am engaged in a series of experiments on animals, the result of which I hope soon to communicate. So far I have subjected dogs to the action of cholera-matter from the human subject. Injections under the skin, into the rectum, and feeding them with food sprinkled freely with cholera dejections have given negative results. —*Med. Times and Gazette, Jan.* 27, 1872, *p.* 95.

23.—ON THE EMPLOYMENT OF NITRITE OF AMYL IN THE COLLAPSE OF CHOLERA.

By Dr. T. Lauder Brunton, Joint Lecturer on Materia Medica, and Casualty Physician, at St. Bartholomew's Hospital.

For the purpose of relaxing the contraction of the pulmonary capillaries, nitrite of amyl would seem to be the very remedy. Dr. George Johnson observes that nitrite of amyl may not act on the pulmonary capillaries as it does on the systemic ones. I have, however, tried it on animals, and find that it does. When given to rabbits, it causes the systemic capillaries to dilate enormously, the blood flows rapidly into the veins, and the pressure on the arterial system sinks in a corresponding degree. If it did not produce dilatation of the pulmonary as well as of the systemic capillaries, the blood which pours rapidly into the veins could not pass with equal rapidity out of them through the pulmonary vessels, and it would consequently accumulate in and distend the right side of the heart. In order to see whether this was the case or not, I thoroughly narcotised a rabbit with chloral, put a canula into its trachea, and kept up artificial respiration. I then opened the thorax, and, after carefully noting the appearance of the heart, passed the vapour of nitrite of amyl, mixed with air, into the lungs. When this was done, the cardiac pulsations become a little quicker, but not the slightest distension of the right side of the heart or of the jugular vein could be observed. I repeated the experiment several times with the same result. There is no reason to suppose that the chloral with which the animal was narcotised altered in any way the action of the nitrite of amyl on the pulmonary vessels, for I have found that other vapours, as well as certain poisons, injected into the jugular vein cause accumulation of blood in the right side of the heart after the animal has been completely narcotised. I have also observed that this accumulation of blood, which I believe to be due to contraction of the pulmonary vessels caused by the poison or vapour, has disappeared in the case of one poison at least, after the injection of another drug, which had been previously found to be an antidote to it in other respects. With the prosecution of these experiments I am at present engaged; and I take this opportunity of thanking Dr. Burd[illegible] Sanderson for his kindness in allowing me the use of his laboratory and apparatus.

Dr. Johnson also says that, even if nitrite of amyl were shown to possess the power of relaxing the pulmonary capillaries in health, it might not do so in disease, or at any rate not to the same extent. Some observations which I have made on its action in angina pectoris lend probability to this idea. In the paper already referred to, which I published in 1867, I mentioned my belief that angina was due to spasmodic contraction of the capillaries; and in a paper in the Clinical Society's *Transactions* for 1870, I gave copies of one or two of the sphygmographic tracings which led me to form this belief. In it I noticed that occasionally after the anginal pain had disappeared from every other part of the cardiac region, it remained persistently at a spot two inches inside the right nipple. This local persistence of the pain was accompanied by a peculiar condition of the pulse, which seemed to me to indicate that the pulmonary capillaries had not become relaxed to the same extent as the systemic ones. As sphygmographic tracings, especially when taken with an instrument like the one I used in which the pressure exerted by the spring could not be estimated, sometimes admit of different interpretations, I append copies of some of them. I may mention, however, that these tracings are strictly comparable with each other, although they may not be so with tracings taken at a different time; for while these were being taken, the instrument remained attached to the arm of the patient, and the pressing of the spring which pressed upon the artery remained unaltered. When the nitrate was inhaled, the pulse in most cases quickly regained its normal character; but when the pain remained in the region of the right nipple after the inhalation, the arterial tension fell as usual, indicating dilatation of the systemic capillaries, but the volume of the pulse remained small. The small volume is, I think, due to the pulmonary capillaries not dilating, or only imperfectly, under the action of the nitrite; and if I discontinued the inhalation before the volume of the pulse became normal, as well as its character, I felt nearly certain that the spasmodic pain would speedily return with all its former intensity.

The observation of Drs. Hayden and Cruise that the pulse became perceptible in their patients after the inhalation, seems to show, however, that the nitrite did relax the pulmonary vessels during the collapse, and allowed a fuller stream of blood to pass into the left ventricle, and thence to the body. Why, then, did it increase the difficulty of breathing? One would have thought that, by allowing more blood to flow through the lungs, it would at least have alleviated this symptom, if it did not remove it altogether. The experiments of Dr. Arthur Gamgee at once enables us to answer this question. In a paper in the Royal Society's *Transactions*, he showed that nitrite of amyl combines with hæmoglobin, the oxygen-carrier of the blood, and prevents it from giving off oxygen after it has taken it up. When the blood is exposed to the vapour of the drug as it passes through the lungs, the oxygen becomes locked up in it, and cannot be given off to the tissues for their use, so that however much oxygen the blood in the arteries may contain, it acts very much as if it were venous. In consequence of this, I have found that, when the vapour of the nitrite is passed into the lungs of rabbits, they are seized with suffocative convulsions. The greater part of the blood which passed through the lungs having been acted on by the vapour, it affects the brain just as it would have done if the trachea had been closed, and no air allowed to come near it during its passage through the pulmonary vessels, and therefore convulsions occur as soon as it reaches the brain. Professor H. C. Wood, of Philadelphia, who has lately written a valuable paper on the action of the nitrite, found no convulsions after injecting it subcutaneously. By giving it in this way, the blood was not acted on during its passage through the lungs, and asphyxia was prevented. This, I think, shows that if nitrite of amyl is to be used at all as a remedy in cholera, it must be given internally or by subcutaneous injection, and not by inhalation. This mode of administration seems to me to be indicat ed also in spasmodic asthma, when the arterialisation of the blood is also impaired. In angina pectoris it will act when given internally, but a larger dose is required than when inhaled; and it remains to be seen whether it can be given in cholera in sufficient doses to relax the capillaries without, at the same time, injuriously impairing the respiratory changes in the blood and tissues.—*British Med. Journal, Jan.* 13, 1872, *p.* 44.

24.—ON THE THERAPEUTICAL IMPORTANCE OF RECENT VIEWS OF THE NATURE AND STRUCTURE OF CANCER.

By Henry Arnott, Esq., Assistant-Surgeon to St. Thomas's Hospital.

A calm survey of the two opposite views of the nature of cancer, and of the facts which are adduced in support of them, seems to leave us in about this position:—that while it is impossible absolutely to deny the existence of a special dyscrasia as the cause of the appearance of malignant tumours, the evidence in its favour will not bear careful scrutiny, although it may be admitted that certain individuals and even families exhibit a remarkable predisposition to the occurrence of these growths, just as a similar predisposition may be shown to the appearance of fatty growths, warts, or other such local defects of nutrition; that owing to appreciable physical causes—as the special arrangement of structural elements, the part of the body attacked, &c.—certain tumours are more prone to malignancy than others: that whether the original growth be absolutely local or depending on general changes in the system, a stage may be at length reached in which there may be, indeed, an ineradicable and necessarily fatal blood contamination: and finally, that as all the real evidence before us goes to prove that it is in any case the *local tumours or ulcers which kill*,—either by invasion of vital organs, or by the breaking-up of health caused by excessive hemorrhages, pain and exhausting and fœtid discharge from the primary seat of the disease—these should be completely removed when possible.

Of course the zeal and hopefulness with which the surgeon sets about this task will depend in a great measure upon the particular theory which he may hold, but it seems illogical to contend that because a man believes that profound constitutional changes have caused the appearance of a terrible local mischief, therefore he need not trouble to attempt the removal of the dire result of a cause which is beyond his control. Even if he feels confident of the system of his patient being saturated with the poison, he will yet endeavour to obviate the more tangible manifestations, whenever this can be done without incurring a yet greater risk.

If, on the other hand, the surgeon be persuaded that all the formerly mysterious phenomena of cancer are now explicable by local changes readily appreciable to his senses, and to a great extent under his control, he will arduously seek for some means by which to eradicate the malady in its more harmless stage.

In the endeavours to establish a sound therapeutical application of these modern doctrines of the pathology of cancer, one most comforting assurance is at once presented to us. It is this: We have seen that malignancy is by no means the exclusive property of one form of new growth, but it is shared to some extent by nearly all the tumours with which we are acquainted. We have further seen that the degree of malignancy appears mainly to depend upon such physical conditions as the minute structure of the tumour, its consistence, and the amount of moisture and movement of the affected locality. Clearly, then, we need not isolate cancers from the other new growths in the selection of our remedial measures. There is no apparent reason why a remedy, which will remove an innocent tumour, will not be equally efficacious, as regards its action upon local over-growth, when applied to a malignant one; and any circumstances (as extensive local infiltration, or early gland contamination) likely to interfere with the successful issue, may be readily taken into consideration, and the *treatment* and prognosis varied accordingly.

Hence there is nothing specific distinguishing cancerous from other tumours which should prevent us from applying other methods of treatment to such cases as for any reason happen to be ill-suited for operation. To speak of the knife as some distinguished surgeons have done, as the only trustworthy resource for the cancerous patient—and this, too, in the face of the unsatisfactory results hitherto achieved by this means—and to rank with quacks those who earnestly seek for better remedies, is calculated to advance neither our knowledge of disease, nor our faith in therapeutics.

Lastly, there is no reason in the world why we should refuse to admit that a cancer has been cured or permanently removed. It has become quite common to hear surgeons say of a tumour which has not returned after extirpation some years previously, "I suppose it was not really cancer, as it has not come back, but it had certainly all the ordinary characters of cancer." In like manner it is frequently said of reputed cures of cancer by pressure, or caustic, or by other means, that the tumours could not have been cancerous simply *because* they disappeared under the treatment. It is surely time that we give up these notions of the specific and inevitably mortal character of malignant tumours, for such doctrines are not only to be deprecated on account of the insufficiency of pathological facts to be urged in their support, but for the far more serious reason that they stand in the way of any advance in rational therapeutics.

Nevertheless, when we come to consider the question of treatment more carefully, complete removal of the tumour with the knife is the first and obvious expedient of the surgeon, and indeed, where it can be safely and effectually accomplished, this seems to be the distinct indication. But it is unfortunately the fact that complete removal is only certainly possible in a very early stage of the disease.

Partial removal is clearly inutile—if not absolutely hurtful—from any point of view, save under very exceptional circumstances.

But since patients seldom seek advice until the earliest stage has already passed, what is now indicated?

Still, as it would seem, complete removal if possible; but now a large amount of apparently healthy tissue must be taken away with the tumour, and any infected glands must be extirpated at the same time.

This is the operation which is most frequently called for, and the defective performance of which is to be blamed for much of the opprobrium attaching to surgical interference in cancer.

It is, however, very important to bear in mind that this operation, if effectually done, is often necessarily hazardous to life. How hazardous we have no statistics to show, for its danger has been misrepresented by the *invariable* custom of grouping together in the same statistical statements these larger with the comparatively trifling incisions of less permanent value, or which are sufficient in an earlier stage of the disease.

This point has been always overlooked in quoting the mortality from the operation of amputation of the scirrhous breast, and the consequence has been that a variety of useless operations have been added to a few effective ones with the result of lowering the rate of mortality of the whole.

But this grave error, which has deceived surgeons by permitting them to under-estimate the risks to life of these operations, is not the only evil which has resulted from this custom. The profession being thus guided by the results of operations clearly inadequate for the removal of the complaint (as when enlarged glands are left in the axilla, or portions of the breast or thickened skin are left behind, not to speak here of the strange practice of intentionally removing *most* of a tumour only, and leaving the nipple or hardened muscle to spread the disease again), a great and unjust prejudice has been excited against the use of the knife at all, the erroneous impression of the small risk to life being overweighed by the almost constant return of the disease after a longer or shorter interval.

It is difficult to over-estimate the importance of these considerations, for they strike at the root of all our present practice for the relief or cure of the most terrible disease with which the surgeon has to grapple. Taking the most favourable view of the nature of cancer—that it is an absolutely local change, but extending far beyond its obvious limits, and very prone to disseminate itself widely unless removed at an early period—it is yet evident that we have to combat a malady which will yield to no half measures. The knife which is to cure it must cut widely and deeply to effect its purpose. There must be no piece-meal removal which may scatter the seeds of the disease in the wound; nor must any the least suspicious bit be left behind. The practice of sponging into the fresh operation wound a solution of chloride of zinc, as first suggested by Mr. De Morgan, seems also to be strongly indicated, unless the surgeon prefers to use carbolic acid, or any other of the numerous substitutes which, under the title of "disinfectants," have been zealously advocated since chloride of zinc was first used in this way at the Middlesex Hospital. It wil. do no good to shut our eyes to the gravity of such an operation. Let its increased risks be set against its greatly increased efficacy, and if the risks are found to outweigh the benefits, then let other means less dangerous be sought.

It is probable that in a large proportion of the cases coming under the surgeon's care, some less immediately dangerous therapeutical measure might be profitably substituted.

Caustics have seemed to some to meet this end to a great extent, and their employment is certainly attended with infinitely less risk to life than extensive cutting operations. The intolerable and enduring pain attending their use has hitherto contributed mainly to their neglect by English surgeons. From some reason not easy to comprehend the practice of congealing the part before the application of the caustic, although found to be completely effectual in the instances in which it has been tried, has not yet been sufficiently widely tested to permit this combination to be ranked amongst the reliable measures for the relief of cancer; but much remains to be done in this direction, more particulary in those cases where the disease is conveniently located for such an application, and where the patient has an unconquerable dread of the knife.

No doubt any remedy which affects the system generally must, to some extent, affect any local ailment which may be present, and in this sense general remedies may be of some service in cancer. But it does not seem likely that we shall readily succeed in finding a drug whose action shall be so concentrated upon a given limited portion of the body as to arrest in that part the excessive and ill-directed cell proliferation which is so vigorously going on. It seems to be a peculiarity of all morbid growths (as distinct from mere inflammatory tissue changes), that they flourish quite independently of the general nutrition of the body, and at the expense of the normal tissues from which the necessary pabulum is diverted.

Failing, then, general remedies, we should seek such a measure as will aim at correcting the local growth, by supplying such condition or combination of conditions as are found to retard physiological growth and development.

The present despairing custom of anointing the surface of the swelling or ulcer with soothing unguents, or of fomenting, or of the application of the various bland lotions or liniments in common use must be regarded as mere *placebo* treatment—useful, indeed, in its way for the alleviation of some of the aches and pains of the growth, and for the calming of the minds of the sufferers, but unworthy the serious consideration of pathologists who, holding either of the theories of the nature of the disease, yet hopefully seek some rational means of removing the local mischief.

For we may again remind ourselves that the mystery of the nature of cancer has almost entirely passed away. That we still know nothing of the proximate cause of the appearance of a malignant growth is an admission we may fearlessly make, since we know so little of the proximate cause of most of the morbid changes whose results are now so minutely studied by the morbid anatomist. But we no longer vex ourselves with the fruitless search for a specific cell-form or chemical test which shall distinguish cancers from other tumours. If our microscopic researches have taught us nothing more, at least they have taught us this,—that the most malignant tumours differ from the most innocent and benign only in certain physical conditions of structure or position which vary greatly in different specimens, and which by their variations seem to explain with sufficient clearness the corresponding varieties of malignant properties.

More than twenty years have passed since Dr. J. Hughes Bennett published his work on cancer, with its able and (considering the period at which it was written) heretical chapter on "rational treatment," and to-day we may still learn many a valuable lesson from its perusal. The operation which that distinguished physician on purely pathological grounds, so warmly advocated, we on the like ground, and strengthened by greatly extended histological researches still contend for as the most certain means of eradication of the disease where it can be sufficiently boldly performed. Ablation by means of caustic has been vastly improved since then, and now forms one of the recognised modes of treatment of certain cancers.—*St. Thomas' Hospital Reports*, 1871, *p.* 115.

25.—ON INHALATION, FOMENTATION, DISINFECTION, &c., WITH MEDICATED STEAM AND VAPOUR.

By Dr. J. Williams, Malvern.

[When medicated vapours are used with the ordinary inhaler, too rapid cooling takes place. Some means of maintaining the heat is required in order to get the full benefit of this mode of treatment.]

A more complete investigation of the subject has enabled me to construct an inhaler, with a lamp, which fulfils nearly every possible requirement for all parts which we have mentioned, and certainly combines many points of importance unattainable by any other system with which I am acquainted. My invention enables the Practitioner to introduce nearly all the substances and fluids which could be given by the atomiser, and at the same time continues the inhalation of all vapours and medications at a uniform heat to the lungs and breathing surface, fomenting, as it were, the whole mucous lining of the air passages, which has a very important and soothing influence. The influence upon the nervous system is equally certain in spasmodic cough; and in nervous asthma, especially, the action is very striking and satisfactory. I may mention that volatile substances, such as camphor, carbolic acid, hyssop, anthemis, &c., are peculiarly efficacious and agreeable when introduced in the form of vapour, which, by the arrangement I have proposed, does not in any way interfere with the digestive organs. In fact, if any doubt could possibly exist as to the action of medicated inhalations upon the system, the common daily use of chloroform, nitrous oxide, methylene, &c., would answer the question in the affirmative. Besides, the introduction of poisonous gases—hydrocyanic and carbonic acid—are well known to be rapidly fatal in this mode of administration; therefore, by a parity of reasoning, we may assume that the introduction of vaporised medications must, if pursued and investigated, become an extremely valuable means for the introduction of numerous medicines into the human body.

In localised neuralgia, face-ache, ear-ache, &c., a jet of steam directed against the part affected gives almost instantaneous relief, entirely superseding fomentations, mustard and other burning plasters; these latter remedies, in many instances (in my own experience), serve only to increase the original pain. After the action of the steam-jet, the part affected may be gently rubbed with chloroform, aconite, or camphor liniment, to prevent undue action upon the skin by damp, cold air, &c.

As a disinfecting and fumigating agent I find this lamp superior to all oth means, as the chemicals are converted into subtle vapour which seems inst y to destroy unpleasant odours in the sick room, and in a far more per manner than by sprinkling the fluids or powders about the room.

Directions for Using the Inhaler or Fomenting Lamp.—Having first the wick in the lamp, pour into it about an inch deep of methylated spirit; in the pan over the flame place about half a tumbler of warm water, and perforated plate a few herbs, such as hyssop, senega, camomile, pennyroy *These* may be used several times without renewing. The apparatus now *ready for use*, light the spirit-lamp, and as soon as the water boils a str steam will issue from the spout. As the steam would be next the spout, it is better to use a tin or paper tube, expanding to about two inches at the open end. This tube has holes in it to allow the atmosphere to mix with the steam. By this arrangement, and by turning the holes round, the temperature may be very nicely regulated. As the steam ascends rapidly, the open end must be held below the mouth or part to be acted upon, and at a little distance from it. A folded piece of paper from the tube resting on the nose, or a loosely rolled tube in the mouth, in some cases is an extra advantage, and excludes the vapour from the eyes, if desired; but in many cases it is of most service when inhaled rather hot. In using carbolic acid and some other ingredients it is better to add them to the water before applying the lamp-flame to it. The tube used for conveying the steam has holes in it; these should be turned round to the lower side, but if too hot, brought half round, and if required still cooler, to the upper side.—*Medical Times and Gazette, December* 9, 1871, *p.* 704.

Lamp-inhaler made by Mr. Sparkes, Malvern.

DISEASES OF THE NERVOUS SYSTEM.

26.—ON THE HEALTHY AND MORBID ANATOMY OF THE PERIVASCULAR SYSTEM OF THE BRAIN.

By W. W. Wagstaffe, Esq., Joint Lecturer on Anatomy at St. Thomas's Hospital.

[What is known as the perivascular system was discovered by Robin, in 1855, and the only two subsequent investigators in this field have been Professor His, of Basle, and Dr. Bastian, who translated Professor His's paper, and appended his own observations upon it.]

So little notice has been taken generally of the subject, that I am glad the occurrence of three peculiarly interesting cases gives me the opportunity of offering the following remarks upon what must be considered a most important physiological structure.

Healthy Anatomy.—The perivascular canals are seen with the greatest ease in the fresh brain. They are sometimes visible even with the naked eye. Remove a small vessel from the substance of the brain by means of a fine pair of forceps, and it will be seen upon examining it under the microscope, that ensheathing the wall of the vessel is a very transparent membrane, broader in some parts than in others, and distinguished from the vessel by its almost structureless character, and by the absence of the ordinary contents of vessels. In some places the membrane lies close to the vessel-wall, so that it is with difficulty detected; in other places a large space exists between them; and, as a general rule, it may be stated that the size of the perivascular space increases with the size of the vessel.

That this membrane forms a complete canal around the vessel is certain, from the result of examination microscopically, and also from the result of injection. The space is so well defined, and the membrane limiting it so distinct, and its occurrence is so constant when ordinary care is used in the examination, that it would be useless to question its existence. Moreover, Professor His has been able to inject the system, and has shown that everywhere these canals surround the blood-vessels, and that they are quite sharply defined externally.

The structure of the sheath is peculiar, and the description of it given by His, and Robin, and Bastian, differing as they do from one another, cannot be considered altogether correct. Robin and Bastian describe it as a delicate hyaline membrane in its normal condition, but His states that it is composed of a striped basis-substance very similar to the walls of the splenic veins. Bastian allows that it frequently undergoes a fibroid change, but he apparently considers this the result of some morbid process, and not its constant and normal condition in any part.

It may be easily shown, however, that the structure of the sheath differ very considerably in different parts, and that a description of a sheath su rounding a small vessel will not at all apply to that enclosing a large one. a small vessel be examined after removal from the human brain ten or twel hours after death, the wall of the sheath appears to be structureless, or o faintly granular, when a one-sixteenth object-glass is used, and no epithe lining is visible. But, if such a vessel be examined immediately after de the appearances are different. The transparent membrane is lined by a v definite layer of epithelium, composed of cells of very unequal sizes pac together closely, and containing nuclei which occupy nearly the whole of th cells. Both the cell outline and the nucleus are extremely faint, and no space appears to be left between this epithelium and the wall of the vessels; where the cells are large, the perivascular canals are bulging, and where they are small they project but slightly. The transparent membranous wall too gives indication of a transverse and longitudinal striation.

When a rather larger vessel is taken, evidences are then obtained of a somewhat different structure. Examined from the human brain some hours after death, the sheath is seen to be marked by faint lines branching and anastomosing, starting here and there from more definite fibres, and now and then there appears a faint nuclear corpuscle embedded in the wall, and connected with the fibre-markings on every side. The intermediate wall appears still almost structureless, but I have noticed both in the fresh preparations and in those which have been treated with nitrate of silver, that there is a tendency to a transverse striation sometimes very marked, but usually difficult to trace. I have been unable to detect any connection between the fibres of the wall and the exterior of the vessel within, and Prof. His states positively that "no connection exists between the wall of the vessel and the wall of the perivascular canal."

A similar vessel being examined next from a dog immediately after death, it was found that the walls were composed of an outer set of longitudinal wavy fibres, inside which was a circular set of delicate but distinct branching fibres. Inside this again was a quantity of corpuscular material of which I was unable to trace the anatomical relations.

Lastly, when a large vessel is examined, the structure of the perivascular wall is again different. It is composed of bundles of wavy fibres, similar to not identical with those of white fibrous tissue, and no trace of the structureless membrane remains within this fibrous longitudinal layer. Indications still exist of the transverse markings, but they are much more obscure than in the smaller vessels, and they are only at all readily traceable in the perfectly fresh brain. The examination of the larger sheaths is difficult, and it is not easy to determine the existence or absence of epithelial lining, or of the connection between the wall and the exterior of the contained vessel, although such connection appears to me highly probable.

Perivascular canals exist without question throughout the whole of the brain. They may be traced, as I have said before, upon any vessel pulled from the substance of the brain. But it is not so easy to follow them in the membranes, and there is reason for it.

I may finish these introductory remarks by alluding to the probable function of these canals, and in doing so it will be necessary to bear in mind the fact that, although so distinct in the central nervous organs, they are ex tremely difficult to trace in the majority of tissues, and that in many texture their existence is at the most only a matter of conjecture. Where they ar most easily seen, as in the brain substance, there is no surrounding connecti tissue, whereas in the structure in which they are either absent or their e istence is doubtful, loose connective tissue surrounds the vessels. It seem therefore, probable that the fluid which exudes from the blood for the p of nutrition, particularly in tissues undergoing rapid nutritive chan such as nerve substance, and, possibly, muscular and gland tissues, is held in these canals ready for immediate use, whereas in other structures the fluid which exudes is held in the abundant spongy connective tissue which is so evident in these situations. Add to this that in the case of the central nerve organs we have in the perivascular canals a means of reducing to a minimum the mechanical effects of alternate dilatation and contraction of the vessels, and of protecting the delicate nerve substance from the more gradual alterations in pressure which might otherwise result from the sudden, extensive, or permanent alteration in the blood supply. A further function might be suggested in their influence upon the vessels themselves. In principle these are floating within the perivascular sheaths, and the chances of rupture or other injury from concussion are thereby materially diminished. The physiological function of the contents of these canals is, therefore, probably identical with that of the cerebro-spinal fluid with which it is in direct communication. It may be looked upon, in fact, as both protective and compensatory.

Morbid Anatomy.—Case 1.—The first case which I have to mention is that of a child who died at the age of a year and a half of tubercular meningitis in November, 1869. There was a family history of phthisis. The mother had suffered from phthisical symptoms before and at the time of the child's birth, and had died of this disease when the child was a few months old. Since the child's death an elder sister has also died of phthisis. He had the usual symptoms of cerebral meningitis, and died at the end of rather more than three weeks after the first appearance of the symptoms.

The *post-mortem* appearances coincided with what was suspected before death. Tubercular deposits were scattered freely over the surface of the brain, and a large mass of similar structure was found occupying the fissure of Sylvius on the right side. The tubercle was always related to the vessels definitely; on the upper surface of the hemispheres clusters of miliary deposits were arranged along the sides of the larger vessels: in some cases the naked eye could trace small vessels running into single deposits: in the large mass in the Sylvian fissure the carotid and the middle cerebral arteries were surrounded by the new growth: and it is worthy of notice that both carotid arteries were very markedly thickened before they entered the brain. On slicing the brain it was found to be congested, as were the membranes outside, and there were small points of extravasation in the ventricles, as there also were rather frequently in the membranes. Everywhere the brain substance was rather soft, but the distinction between the white and gray matter was well defined; the softening was, if anything, rather more marked on the right side.

The relation of the deposits to the vessels has just been referred to above; but when they were examined microscopically a much more beautiful and exact relationship could be traced. To explain this I will premise by saying that both the large and the small deposits were composed of nucleated corpuscles—of nuclei, in fact, surrounded by an irregular amount of plasma, the nuclei being of pretty uniform size. In some places they were mixed with a large quantity of granular and oily matter; sometimes with the so-called compound granule cells—these additions being, of course, only the evidences of degenerative changes. Now, when a portion of the membrane was examined, which was but little obscured by the large masses of deposit, but in which it was still easy to see the small miliary tubercles standing distinct, the first point which attracted attention was the marked distension of the perivascular canals. These, which are often difficult to demonstrate in the meninges in health, were now very distinct, and distinct from being distended with abnormal contents. They contained corpuscles exactly similar to those found in the masses of tubercle. Sometimes there were many particles of oily and granular matter, and other evidences of degeneration, and such appearances were usually more marked in the neighbourhood of the larger masses of deposit. But, besides the diffusion of the elements of tubercle in these perivascular canals, and their great abundance near the masses of the deposit, further evidence was obtained of the relation between them. One of the very small miliary deposits was examined, and showed that it was situated entirely within the perivascular sheath. The sheath was bulged considerably by it; and although the large amount of molecular and oily matter prevented the artery itself being seen, yet it may be fairly inferred from the evident accun lation of tubercle corpuscles between the vessel and the sheath in other par that such miliary deposit was here situated in the same position—that is say, simply in the perivascular space.

This specimen was taken from the pia mater, and affords, it seems to me, conclusive evidence of the existence of the perivascular system here.—*St. Thomas's Hosp. Reports*, 1871, *p.* 149.

27.—ON NERVOUS OR SICK-HEADACHES.

By Dr. P. W. Latham, Physician to Addenbrooke's Hospital, Cambridge.

[The pathology of nervous or sick-headache is a defective supply of blood to some portion of brain, owing to contraction of one of the cerebral arteries, probably the middle cerebral. There is generally loss of tone of the cerebro-spinal system, from overwork, anxiety, or some similar cause. The headache is frequently preceded by a glimmering of some portion of the field of vision of one eye. If the patient will lie down this glimmering not unfrequently passes off or becomes much less intense, and the headache which would have followed is averted or correspondingly modified.]

Let us consider separately the remedial measures to be adopted (1) during the stage of disturbed sensation, (2) during the stage of headache, and (3) during the intervals between the attacks.

1. *During the Stage of Disturbed Sensation.*—In the forms attended with disturbance of vision, you will find that in the same individual the longer this stage lasts, the greater will be the headache; and therefore we must endeavour to shorten it as much as possible. If the condition, then, depend upon deficient supply of blood to a part, such means must be adopted as shall assist and increase the flow of blood to the part; and this can be done in some measure by posture and stimulants. Directly the glimmering appears, the patient should lie down with the head as low as possible, and if the glimmering be on the right or left of the field of vision, he should lie on the *opposite* side. Let him take at once a full-sized glass of sherry; if at hand, half a bottle of soda water is a useful addition. Champagne would be preferable, being more diffusible; but its administration would often involve a little delay, and at the commencement of an attack it is a great point to save time. A large tablespoonful of brandy diluted may, if the patient prefer it, be substituted for the sherry. If alcoholic stimulants be objected to, or if it be not advisable to recommend them, then a teaspoonful of sal volatile in water may be prescribed instead. If the patient be chilly or his feet cold, the couch should be drawn before the fire and a hot bottle applied to the feet. By these means the heart is enabled to drive the blood with greater force to the brain, and the duration of the vibratory movement is thereby materially lessened. After it has passed off, the patient should lie still for a time, so that the glimmering may not return. This injunction will only be necessary when the headache is slight; if it be severe, attended with much nausea or vomiting, the patient will be little disposed or able to leave the recumbent position. If, instead of the disturbance of vision preceding the headache, there be a feeling of depression or irritability, fidgets, &c., the administration of such cerebro-spinal stimulants as henbane, valerian, assafœtida, spirit of chloroform, or ether, will often cut short the attack; ten or fifteen drops of the tincture of henbane with the same quantity of spirit of chloroform, will soothe the nervous irritability in the slighter forms, and may be repeated in three or four hours, if necessary. If there be great mental depression, then valerian or assafœtida should be tried. Stillé says: "Nothing is more astonishing in the operation of remedies than the promptness and certainty with which a dose of valerian or assafœtida dispels the gloomy visions of the hypochondriac, calms the hurry and agitation of nervous excitement, allays commencing spasms, and diffuses a soothing *calm* over the whole being of one who but an hour before was a prey to a *thousand* morbid sensations and thick-coming fancies of danger, wrong, or

.” I give the preference to valerian, and prescribe from half a drachm to
rachm of the ammoniated tincture. The assafœtida may be given in the
n of the spiritus ammoniæ fœtidus of the *Pharmacopœia*, also in half-
chm or drachm doses. As a rule, alcoholic stimulants are not advisable
e. A small quantity will cause flushing, heaviness, slight confusion of
ught, &c., without relieving the depression; and though the severe head-
e may be averted, alcoholic stimulants do not answer so well as the reme-
s previously mentioned.

. *During the Stage of Headache.*—If the headache be slight and the patient
n able to sit up, there is little to be done; a cup of coffee or tea, cheerful
versation, a walk, drive, or ride, may often help to remove the pain. If,
vever, the headache, nausea, &c., be severe, then the administration of further
nedies is called for. The patient should keep perfectly still and quiet, with
room darkened; for every sound or sight causes pain, and the slightest
vement is sufficient to produce gastric uneasiness. Sometimes free evacua-
n of the contents of the stomach, especially if it contain undigested food, is
lowed by relief. Dr. Fothergill says, "an emetic and some warm water
n wash off the offending matter and remove these disorders," which may be
y well where there is any offending matter to wash off, but it is not very
en that this is the case; the nausea frequently continues long after the con-
ts of the stomach have been discharged; an inverted action of the duodenum is
up; the bile appears in the fluids excreted; the patient believes that all his
ubles are due to "its overflow;" "it's all liver," he says, and it is sometimes
ficult to persuade him to the contrary. Generally, then, you should try re-
ve and check the vomiting. Iced soda-water, with or without two or three
ops of dilute hydrocyanic acid, or spirit of chloroform; cold tea; the effer-
scing citrate of potash, with hydrocyanic acid, may often afford marked re-
f. The headache may be lessened by applying cloths dipped in cold water,
evaporating lotions to the head; if the extremities be cold and the headache
vere, a warm stimulating foot-bath can be tried so soon as the nausea will
ow the patient to sit up. If the attacks occur in the early part of the day,
soon as the pain has subsided, it is generally better for the patient to sit up,
move about, or take exercise in the open air. A young lady, on consulting
for this disorder, said: "Nothing relieves these headaches except a good
llop on my pony. I have sometimes to lie still for three or four hours before
e pain is bearable; but directly I am able, I mount my pony and always re-
rn home better." During the attack the appetite is diminished, the idea even
food provoking disgust. Still, after the nausea has passed away and the
adache has continued a few hours, a plate of soup or some easily digested
od will often have a good effect in equalising the cerebral circulation. A
medy which may very often be given with advantage if the headache be se-
re, is bromide of potassium, in doses of 5, 10, or 15 grains, to which 30 or 40
inims of sal volatile may in some cases be added with advantage; and if the
usea still continue, these may be given in combination with the effervescing
rate of potash. A saline purgative at the commencement of an attack is
metimes an effectual remedy; but, as a rule, the use of purgatives is objec-
nable.

So far, the measures which I have suggested are only palliative. We come
w to the consideration of such as are preventive, or to the treatment neces-
ry during the intervals between the attacks. First of all, you must try to
d out the exciting cause and endeavour to remove it. Hours of study or
rk must be abridged; excessive bodily fatigue, loss of rest, everything in
ct, must be avoided which the sufferers know from individual experience will
t as exciting causes. Where the attacks are associated with excessive men-
l work, they should be regarded as danger-signals, showing necessity for re-
tation. In the next place, you must endeavour to improve the tone of the
dily and nervous systems by proper medicinal and hygienic means; and the
ief remedies which I employ are steel, strychnine, and cod-liver oil. The
ccess, however, following these remedies depends a great deal upon the way
which they are administered. For a day or two after the attack the sto-
ach and bowels may possibly be disordered, and not in a fit state to tolerate
ch remedies. This must first be corrected. The simple vegetable bitters,

such as gentian, with small doses of henbane and some aromatic, may be o service, and, if necessary, one or two grains of blue pill, with four or five of compound rhubarb pill, may be given at night. We may then try steel. If the attacks have been very frequent, or if there be any scrofulous tendency, I give the iodide of iron in the following form. ℞. Ferri et ammon. citrat. gr. v; potassii iodidi gr. ij; aquæ ℥ j; and I add, according to circumstances, 15 to 20 minims of tincture of henbane, or 20 to 30 minims of aromatic spirit of ammonia. If the stomach be at all irritable, I give this in the effervescing form, adding to each dose 20 grains of bicarbonate of potash, and directing it to be taken with a tablespoonful of lemon-juice or a corresponding amount of citric acid: the dose to be taken twice a day, about 11 and 4. I soon leave off the effervescing form, and then add to each dose five minims of liquor strychniæ, omitting the henbane and sal volatile, and continuing the iodide of potassium according as it seems to be indicated or not. In other cases, I give the citrate of iron and ammonia with strychnine at the beginning, and sometimes combine them with infusion of calumba. The iron is indicated by the greater or less anæmia of the patient; but the strychnine is, in my opinion, a very important remedial agent in the disorder. In small doses, it acts as a simple tonic, increasing the appetite and improving the digestion; it dilates the vessels, and thus increasing the supply of blood, it augments the activity of the spinal cord (Harley). It promotes the capillary circulation, and therefore its use is advisable for persons troubled with cold hands and feet (Anstie); and if it fulfil these conditions, it is clearly indicated in the disorder which we are considering. Cod-liver oil also often acts very beneficially. "It has been found by experiment that great exertion and prolonged labour can be endured without fatigue when starchy and fatty foods are alone eaten . . . and there is reason to think that cod-liver oil is more easily absorbed than other similar substances" (Ringer). "It improves the digestive process, increases the proportion of red corpuscles in the blood, and invigorates the whole nutritive function" (Wood); and I believe it particularly sustains the energy of the brain during prolonged mental exertion. A gentleman in the foremost rank at the bar told me that, whenever he was engaged in a jury-trial which was likely to tax his energies to a greater degree than usual, the thing which best sustained him was a good dose of cod-liver oil taken in the morning before going into court; and others engaged in mental work have confirmed this view. I therefore regard cod-liver oil as having, besides its other properties, a nutrient and tonic action on the cerebro-spinal nervous system. As a remedy for these nervous headaches, I only prescribe it once a day, beginning with a small teaspoonful immediately after breakfast, and gradually increasing the quantity to a tablespoonful, but not beyond, unless in exceptional cases.

You must take care to regulate the action of the bowels, but by no means have recourse to strong purgatives. Five grains of the Socotrine aloes pill, given at night, are generally sufficient. If the bowels be habitually constipated, then no remedy seems to answer so well as the aloes and iron pill. Five grains given twice a day, half an hour before meals, will act freely; and in a few days you will have to diminish the dose, for the remedy possesses this advantage, that its effect is augmented instead of being lessened by continual administration, especially when strychnine is given at the same time. The natural waters of Friedrichshall or Marienbad may in many instances be of service, given as laxatives.

Besides the remedies to which I have called your attention, others have been recommended, such as arsenic and quinine, caffein, &c. Where anæmia is not a prominent symptom, they may sometimes be of service.

Lastly, you must lay down stringent rules for your patients with regard to diet and exercise, and you must impress upon them the importance of these rules being strictly observed.—*British Medical Journal, March* 30, 1872, *p.* 336.

28.—ALCOHOLIC PARAPLEGIA.

By Dr. Samuel Wilks, Physician to Guy's Hospital.

[Cases of paraplegia resulting from an excessive use of alcohol are stated by Dr. Wilks to be not at all uncommon. All medical men are doubtless familiar with the occurrence of epilepsy from this cause. But paraplegia must be, except in the large experience of such a man as the writer of this paper, very rarely seen.]

"*Drunkards' or Alcoholic Paraplegia.*—I do not know that this is deserving of a distinct name from its possessing any pathological peculiarities; but as arising in connection with a very well marked exciting cause, it may deserve your especial attention, and I refer to it the more especially because I believe authors have generally overlooked it. I have already told you that long-continued habits of intemperance in alcoholic drinks tend to the production of a fibrous or fatty degeneration of the various tissues of the body, and that, as a consequence, the membranes of the brain and spinal cord become thickened, and the organs within wasted. This, of course, would give rise to what might be called a general paralysis of body and mind. But besides these general results we often meet with more direct effects on the spinal cord, and to these I particularly refer. I have now seen so many persons, especially ladies, who have entirely given themselves up to the pleasures of brandy-drinking, and been rendered paraplegic, that I have become pretty familiar with the symptoms. From what we hear of our continental neighbours, it would seem that the diabolical compound styled 'absinthe' is productive of exhaustion of nervous power in even a much more marked degree; it would appear that the volatile oils dissolved in the alcohol give additional force to its poisonous effects. Of course, drunkards of all descriptions suffer from nervous and muscular weakness; but, as I before said, it is more especially in the legs that the effect is most striking. A loss of power is first observed, accompanied by pains in the limbs, which might indicate a chronic meningitis of the spinal cord, and in some cases there is anæsthesia. There is, at the same time, some amount of feebleness of other parts of the body as well as the mind, and thus an approach to general paralysis is produced; but sometimes the symptoms are almost confined to the legs, and resemble in character those of locomotor ataxy."

Since this was written I have seen several cases of a similar kind; and it is worthy of remark that they occurred mostly in women. I would repeat that something more definite is intended by the term "alcoholic paralysis" than that general muscular and nervous debility which is as well known to the public as the profession; for no character is more easily recognised on the stage of the theatre than the victim of chronic drunkenness. That which I wish more particularly to draw attention to, is the case where alcohol is seen exerting its influence more directly on the spinal cord, making paralysis the leading symptom. Although there is a tendency to a particular form of degeneration in alcoholism, it is not very evident why one person should be affected in one way and another in another; or why one person should have cirrhosis of the liver, another Bright's disease, and a third atrophic meningo-cerebro-myelitis. I use this term since the changes which the brain and spinal marrow undergo are probably identical with those which are found in the two other affections just mentioned. As regards the brain, the tissue degenerates and the membranes become thickened, and thus the mental condition of the "brainless sot" is familiar to all. It very often closely resembles that observed in the general paralysis of the insane; which is not remarkable, seeing that the two affections may have in some cases a similar pathology. Now, in alcoholic paraplegia there is every reason to believe that the spinal cord is affected in the same way as these other organs, and the following is the usual condition of the patient:—She lies in a bed or on a couch, complaining of severe pains in all the limbs, more especially in the lower ones, which are much wasted, or of a sensation like electric shocks running through them, together with numbness and considerable anæsthesia, and at the same time only slight power of movement, or total inability to stand. With the addition of the akinesia, the symptoms are not unlike those of ataxia, which in its ordinary form appears to be comparatively rare in women. In one case there was hyperæsthesia. In nearly all the cases the liver has been enlarged; there has been sickness, and all the other usual signs of chronic alcoholism. I alluded in my lecture to the existence of pains in the limbs from which drunkards often suffer before any marked signs of paralysis have shown themselves.

As regards treatment, this is hopeful, and should always be attempted; for I think it must be confessed that it is impossible to tell what kind and amount of change has occurred in the centres to produce paralytic symptoms. If the patient be young and the tissues not much degenerated, I should recommend the ordinary treatment, and especially such medicines as the iodide or bromide of potassium, before commencing galvanism and a tonic course. What, however, I would especially insist upon is the removal of the primary cause of the complaint. This seems a common-sense recommendation; but, I am sorry to say, is one not always enforced. Sometimes the reason is that the doctor fears for his position as health-proprietor of the family should his hints be offensive, or he has not moral courage to unfold an unpleasant truth. Sometimes, however, he will most conscientiously refrain from recommending a discontinuance of the alcohol for fear of the results, such as the sudden dissolution of the patient, or an attack of delirium tremens. He need not have the slightest fear on these grounds, for no harm, but only good, will ensue from its withdrawal. I am aware that opinions are at variance in this matter, but nevertheless I enforce my own views strongly, with a large amount of experience to support me. I have never yet seen a person die or suffer from delirium tremens or any other disorder in consequence of the complete withdrawal of alcohol when the system was being poisoned by it. On the other hand, I have seen such remarkable recoveries after its total discontinuance as would certainly surprise those who had never ventured upon the plan. I have seen persons, more especially ladies, lying on their beds surrounded by friends waiting for their last moments, where scarcely a mouthful of food had been taken for months, where the prostration, increased by urgent sickness, was extreme, and where they were constantly plied with brandy to keep them alive a little longer, and yet in such cases, where the diagnosis was clear, from the blood of the patient being overcharged with poisonous elements, and the room stinking with the fetid vapours of the body, I have recommended a withdrawal of every drop of the so-called "stimulus," and with effects more marvellous than anyone who has not adopted the plan can conceive. Unfortunately, these cases of alcoholism are so bound up with moral considerations, and in women so often associated with bodily or mental suffering, that it is most difficult to publish them in all their details. Three cases which I have witnessed during the last year have made a great impression upon me. A lady not far from my residence, the unfortunate subject of alcoholism, and having considerable weakness of the legs, was reduced at length to the utmost stage of prostration by want of food and constant retching. A little brandy was put from time to time between her lips, in order, as was hoped, to eke out her existence a little longer. The end apparently approaching, straw was laid down in front of the house, and her children were sent for in order to take a final farewell, when, after repeated urging on my part, all stimulus was suspended. The sickness soon ceased; the blood became gradually depurated; after a few hours a little food was taken; and in a fortnight's time, this lady was sitting at the dinner-table with her family. I can say nothing, of course, about the likelihood of relapse, as this opens up another subject. But lately, also, I have been seeing a tradesman and his wife, with Mr. Joyce, of Notting-hill, both of whom were addicted to intemperate habits, and both, most remarkably, had almost complete paraplegia. We prevailed on the husband to relinquish his drink, and he began at once to improve; but we had no influence on the female, who was approaching the state of delirium tremens. In another case, that of a medical man, who after drinking hard, became so ill that he took to his bed, had epileptiform attacks, ate nothing, and was constantly retching, his wife standing over him administering brandy and champagne from time to time to keep him alive a little longer, I succeeded, after several attempts, in inducing his wife and two medical attendants to stop *every drop* of alcohol. When this was done the patient soon cried out *for drink*; but, after imploring in vain for some time, he was violently sick, and then sank into a sound sleep. Upon wakening he took a little beef-tea, in a few hours ate some solid food, and within a week was back again in his practice.

I do not wish to discuss in this place the merits of alcohol as a remedy, as it is one I constantly use with the utmost advantage; but simply to state my experience that I have never seen the slightest harm accrue from the sudden deprivation of the accustomed stimulus, but, on the other hand, have had the satisfaction of knowing that I have saved fellow-creatures' lives as certainly as if I had plucked them drowning out of the water. I cannot conceive what the objection is to the withdrawal of the alcohol under the circumstances I mention, any more than I can conceive why, when the system is saturated with poison, and the patient dying from the effects of it, such a gross burlesque on the homœopathic doctrine should be practised as to continue administering it.

The purport of this paper is more especially to draw attention to the fact of paralysis, and more particularly spinal paralysis, occurring as a result of alcoholism; and therefore that when a medical man is called in to see a case of this kind, he should remember intemperance in drinking as a possible cause, just as he would if he found an enlarged liver.

If the affection should turn out to be in any way peculiar in its pathology it will certainly deserve a distinct appellation; but even should the morbid changes in the cord, together with the resulting symptoms, resemble what is seen in other forms of paralysis, I will still recommend the adoption of such a term as alcoholic paralysis as significant of its cause, for we are warranted in so doing by the use of the expression puerperal, syphilitic, or uræmic epilepsy (eclampsia) in reference to the origin of the fits when arising under special circumstances.—*Lancet*, *March* 9, 1872, *p*. 320.

29.—TRAUMATIC NEUROMA, CAUSING PARALYSIS: RAPID AND COMPLETE RESTORATION OF THE FUNCTION OF THE NERVE AFTER REMOVAL OF THE TUMOUR.

By T. R. Jessop, Esq., Surgeon to the Leeds General Infirmary.

The following case affords an interesting example of the rapidity with which, under favourable circumstances, a divided nerve will reunite, even when there has been considerable loss of substance. The ready manner in which the nerve resumed its functions, after these had been in complete abeyance during nine years, gives to the case additional physiological value.

Emma Ross, aged 19, residing at Bramley, was admitted into the Leeds General Infirmary on Febuary 9th, 1871. When ten years old, she cut the inner side of her wrist with a glass bottle. This was followed immediately by loss of sensation on the inner side of the hand, and by diminished power in the entire hand. When, after many weeks, the wound had at length healed, the resulting scar remained painful and tender. At times, the pain was so great as entirely to prevent sleep, and was not always confined to the seat of injury, but occasionally extended up the forearm and arm, and frequently downwards through the hand and two inner fingers. During the few months immediately preceding her application to me, the pain had increased in intensity.

The cicatrix was situated about two inches above the right wrist, and extended across the half of the anterior surface of the forearm; it was hard and very painful on pressure. The hand, as compared with the left, was thin and wasted. The muscles which should have formed the balls of the little finger and thumb had disappeared; and between the metacarpal bones, both back and front, were deep hollows. Over the inner third of the hand, the whole of the little and the inner half of the ring fingers, the skin had completely lost its sensibility, and sensation was imperfect over the radial half of the ring finger. The movements of the hand were so feeble that the limb was of little use.

On the day of admission, Feb. 9th, the ulnar nerve in the neighbourhood of cicatrix was laid bare to the extent of about an inch, by an incision along the inner border of the forearm. Just where it traversed the cicatrix, the nerve presented a globular enlargement of the size of a pea. This, together with about half an inch of the involved nerve in its entire thickness, was removed, when the divided ends immediately retracted. In order to approximate the cut ends of the nerve, it was found necessary to flex the hand upon the forearm. Whilst the hand was so held, the ends of the nerve were brought accurately together by means of a fine carbolised silk suture, and so retained. The operation was performed, and the wound dressed, in accordance with the strictest antiseptic principles.

For three days after the operation, there was considerable pain along the course of the ulnar nerve, as high as the middle of the upper arm, which was relieved by repeated subcutaneous injections of morphia. The wound healed by first intention.

On Feb. 17th, there was the first indication of returning sensation on the ulnar side of the little finger; and the patient now complained of severe pains shooting from the tips of the little and ring fingers to the wound. From this time the pain gradually diminished, whilst sensation extended and increased.

On Feb. 24th, sensation seemed to have fully returned everywhere except at the extreme tips of the two fingers, and the pain had all but ceased. The hand was now released from its flexed position, and gentle passive motion of the wrist commenced.

On March 11th, as there was inability fully to extend the wrist, the limb was bound down upon a straight splint. On March 20th, she was made an out-patient, and ordered to attend twice a week for galvanism of the muscles of the hand. On April 20th, she could use the hand well; had no pain; could feel perfectly everywhere, except at the tip of the little finger, which on examination, was found to be covered with a thick layer of cuticle; and the muscles of the hand showed signs of returning development. She can now (November) use the hand for all purposes; the muscles have to a great extent re-formed, but the right hand is still somewhat thinner than the left, and sensation is still wanting at the tip of the little finger, though it is everywhere else perfect.—*Brit. Med. Jour., Dec.* 2, 1872, *p.* 640.

30.—THREE CASES OF NEURALGIA CURED BY THE USE OF THE CONSTANT GALVANIC CURRENT.

By JOSEPH STEAD, Esq., Manchester.

Case 1.—The first occurred to a well-known merchant in Manchester. He was forty years old, and came to me on the 5th of June last, with intense frontal neuralgia, which had lasted a fortnight. It was most severe in the supraorbital branches of the frontal nerve. Five minutes' gentle application of the continuous current by means of small wet sponges attached to small conical conductors sufficed to cure him of his torment on the spot, and it never returned. "I wish," he said the next day to me, "I had known you a fortnight ago. I should have had a very different Whit-week to what I have had!"

Case 2.—The second case I shall mention occurred in a young lady, a governess with a leading merchant's family in this neighbourhood. She had intense neuralgia of the left side of the face, which she attributed to the partial extraction of a tooth ten days before. I applied the current as in the last case but with slender hopes of relieving her. One application, however, completely freed her from pain for a fortnight; and then she returned with it again, when a similar application effectually removed it, and there has been no return.

Case 3.—My third case occurred on the 4th of this present October, to a valued female servant of an old patient of mine. She was a girl of about twenty-four years of age, and had been suffering from facial neuralgia of both sides for three months. For two nights previously to coming to me, she had been compelled to get up from bed and come down stairs and spend the night awake, and in great agony. Five minutes' application cured one side of the face, and a second five minutes' application cured the other, and then, to my surprise, "tired Nature's sweet restorer" overwhelmed the poor girl, and she fell fast asleep in her chair at my house. And here, again, there has been not the slightest attempt at return of pain.

These three cases have been selected from the records of my practice in the use of electrics during the last two years in the cure of disease to illustrate the truly marvellous power which the constant galvanic current has over pain. The battery used in all three was Weiss's, sometimes known as Foveaux's. I used about eight cells of this splendid battery, with very small sponges (about as large as would fill the end of a thimble) soaked in warm water, and fixed, as I have just said, to those small conical electrodes which are used for the localisation of the current in paralysis of the interossei and lumbricales muscles. I applied them to the part where the pain was, keeping them about one inch apart from each other. I moved them about, but did not *remove* them, or either of them, from the skin for about two minutes. I then rested for one minute, and applied them again for two minutes more. If the pain had not been removed I should have gone on for another five minutes. But as soon as the pain ceases in a case of neuralgia I make it a rule immediately to discontinue the application. In each of these cases I directed the patient to come daily for a repetition of the operation. But, happily, there was no need for this.—*Medical Press and Circular, Nov.* 1, 1871, *p.* 384.

31.—THE PHYSOSTIGMA VENENOSUM (CALABAR BEAN) IN EPILEPSY.

By Dr. S. W. D. Williams, Medical Superintendent of the Sussex Lunatic Asylum, Hayward's Heath.

[The use of Calabar bean in epilepsy was suggested to Dr. Williams by reading Dr. Fraser's paper on its use in tetanus and chorea (*Retrospect*, vol. lviii, p. 59). Dr. Fraser shows that it "directly and powerfully diminishes the reflex activity of the cord." It appears to act chiefly on the motor tract of the cord and on the motor nerves, destroying their conductivity. The great remedies for epilepsy, bromides of ammonium and potassium, also destroy the reflex activity of the cord, but by acting chiefly on the sensory tract. Animals poisoned with it retain voluntary movement, whilst reflex movement is destroyed. Calabar bean has been successfully used in tetanus, and it will be seen from the above that *a priori* we might expect it to be of service in epilepsy.]

My experiments were conducted as follows. I chose twelve ordinary epileptics in the Sussex County Asylum, and noted the number of fits daily for six months; I then had them all weighed, and put them on 1 gr. of the bean each twice daily. This I continued for six months, weighing them every month, and increasing the dose of the drug by ½ gr., until the sixth month, when they took 3½ gr. twice daily. I then left off all the medicine, and registered the number of fits for another six months. I also, during the months the patients were taking the drug, paid special attention to the pulse and temperature, and any unusual phenomena.

For convenience of reference I have arranged the results of my observations in a tabular form.

From one table it will be seen that in six of the cases under treatment there was a very considerable decrease in the number of fits during the six months they were taking the bean, as compared with the six months previously; and, what is still more remarkable, that when the bean was omitted the fits, without exception, began to increase again in every case. This was very marked in some of the cases.

In another table we have recorded the weight of each patient taken on the first day of every month whilst they were taking the Calabar bean. It will be seen that seven increased in weight, four lost, and one was stationary.

The third table compares the results of the two previous ones. From it we see that out of six patients whose fits were lessened, three lost weight; whereas only one lost weight amongst those who were not benefited by the drug. But this fact, if worth anything, seems contradicted by the case of C. A., who gained 10 lbs., and yet is the very patient whose number of fits experienced the greatest diminution in numbers. And again, A. B., whose fits increas from 6 to 27 when taking the drug, gained 7 lbs. Therefore I fear that the o fact these observations as to weight prove is that, as a rule, the Calabar b given in doses up to 7 grs. a day does not interfere with the nutrition processes, or, at all events, not materially.

It has already been pointed out that the increase in the number of fits of the six who were not benefited during the use of the drug was much less than the increase of the number of those improved; so that the improvement in the one class of cases considerably counterbalances the deterioration in the other.

Such being the case, it was reasonable to suppose that, assuming the drug to be the cause of the improvement, *cæteris paribus* we should find the cases in which this improvement was manifested to be of one peculiar class and to differ either in their etiology or pathology, or in the phenomena accompanying the epilepsy, or the intensity of the prodromata, from the other cases, in which there was no improvement. But I am bound to record that, notwithstanding most careful examination and a thorough knowledge of each case, gained by years of intimate watching, no such distinction could be marked.

If the result of the administration of Calabar bean is to deaden reflex action, I surmised that if I could demonstrate the epilepsy in the improved cases to be due to eccentric irritation, we might here have the explanation of the improvement, and I directed my attention specially to this point, searching carefully in each case for indigestion, masturbation, worms, catamenial irregularity, &c., and for prodromata, but without result. I cannot but think, however, that these are the cases in which the bean is likely to be of most service. Unfortunately they are not often met with in asylums, so that I have had no chance of trying.

Another point to which I directed my attention was the fact as proved by Dr. Fraser, that the action of the bean tends to expand the arterial walls. Now it is a well-known fact that in some cases of epilepsy a remarkable pallor often accompanies, and in some cases precedes, the period of tonic spasm of the seizure together with a lessening of the volume of the pulse. I could not, however, find that this pallor was, as a rule, confined to those cases benefited by the drug; neither could I prove that the length of time it lasted, or its intensity, was diminished. Indeed it was an ordinary accompaniment of the fits alike in some of the unimproved as the improved. Epilepsy is believed by some to be due, as Dr. Needham reminds us, to primary irritation of the sympathetic, causing the vaso-motor nerves to increase arterial tension, and so producing sudden cerebral anæmia; and Dr. Fraser's experiments prove that the effect produced in the heart by the bean is not through the vagi nerves, but through the cardiac ganglia; so that the bean may relieve epilepsy by its power of affecting the sympathetic, and thus reducing the tendency to the above-mentioned arterial tensions.

If in truth the Calabar bean has any influence on epilepsy, I cannot but think that its power must lie in the fact conclusively shown by Dr. Fraser, and which my own observations abundantly bear out—that even in moderate doses it causes a decrease in the volume and frequency of the pulse, acting of course through the heart.

In this property it resembles the bromide of potassium.—*Practitioner, Feb.* 1872, *p.* 75.

32.—HYPODERMIC INJECTION OF NICOTINE IN CASES OF TETANUS.

By Dr. E. M. Wuth, Surgeon to the Springsure Hospital, Queensland.

John M., æt. 21, had never been ill before, save some three months previously, when he received a scalp-wound, by a fall from his horse against some sharp stones. There was loss of memory after the fall, but in a fortnight the patient had quite recovered and was able to follow his usual occupation as a shearer. Of late he had been employed at the Washpool, whereby he was obliged to stand for hours up to his hips in water. On my arrival I found him in the following condition: he was lying on a stretcher and resting on his back, with his knees drawn towards the face. The jaw was fixed and the angle of the mouth drawn backwards. The pupils were slightly dilated, the eyeballs fixed, the neck stiff, and all the muscles, especially those of the abdomen, rigid. When the spasms were at their height, the abdominal muscles pressed with such severity on the bowels that flatus was audibly discharged by the rectum, and a foam appeared between the teeth. This stage lasted about a quarter of an hour, after which a partial relaxation took place, during which time the patient could separate his jaws to the extent of about half an inch. The bowels being confined, a draught was administered, when it was found that the muscles of deglutition were affected with tetanus. Castor oil with soap water was therefore given per anum, and a copious motion soon followed. The bladder being distended with urine, this was drawn off by the catheter. The patient now with difficulty moved his arms, and pointed to the lower part of the sternum and lower ribs as the seat of pain. Though sensible, he could not speak. I was told by his friends that he had been in this condition for the previous 48 hours. There was no sleep and no ability to take food, and even drinks could not pass beyond the mouth; nourishing enemata were therefore employed. As to treatment the maxim, "Contraria contrariis," has never been worthier in my eyes than as to the choice of remedies for tetanus. I mean, that for its complete cure, nothing can be more endowed with success than the production of a state in the system exactly opposite to that by which tetanus is caused. From all the remedies tried, I thought chloroform deserved here the first rank, and it was administered therefore by me for three consecutive days, and with temporary benefit. However, on the fourth day of my treatment, terrible opisthotonos recurred, and the patient was so weakened and the tetanic symptoms had so gained command over him, that it led me to bring H. Hobart's hopes into realization, by injecting half a drachm of a solution of one drop of pure nicotine in an ounce of pure water, into the cellular tissue of the thigh. Though immediately the pulse sank to 60, the temperature showed little deviation. All the muscles, except those of the neck, suddenly relaxed, and presently the patient began to perspire. He fell into a sound sleep for four hours, after which he had fully recovered the use of the organs of speech. His first question was: "What has the doctor done with my thigh? I feel all benumbed!" He then partook of some wine and water, and an egg beaten up in milk. The injection was used at twelve o'clock at night. The patient awoke about four o'clock next morning. One hour later, a pill of one grain of extract. nicotianæ aquos. (Ph. Hess.) was administered to him. He again awoke drowsy at nine o'clock a.m. Although his body felt very sore, the tetanic symptoms had ceased, and from this time his bowels and bladder acted regularly. A pill of the same extract was given for three nights following, after which the patient felt, and has been since, as well as ever.

Haughton and O'Reilly (Medical Times and Gazette, June 12th, 1858) highly extolled the antidotic qualities of nicotine and infusion of tobacco in cases of poisoning by strychnine, and considering the physiological analogy of the effects of the latter poison with the symptoms of tetanus, I may urge upon my professional brethren the rationale of the treatment.—*Australian Medical Journal*, Sept. 1871, *p.* 264.

33.—USE OF NUX VOMICA IN CERTAIN NEUROSES OF ORGANIC LIFE.

M. Brugnoli has employed nux vomica successfully in the nervous movements of pregnancy, gastralgia, dyspepsia, hypochondriasis, nervous palpitations of the heart, nervous and periodic cough, asthma, and finally in albuminuria. This remedy acts either on the pneumogastric, or on the great sympathetic, or on the spinal cord. He records a case of a lady affected with severe cough recurring every evening and lasting throughout the night, who was cured in two days by the use of nux vomica. Another patient was affected every evening *with violent cough* accompanied by catarrhal expectoration, and was also cured in two days by the use of the alcoholic extract of nux vomica mixed with the extract of gentian. Cough may always be allayed by this means, whether it be caused by bronchitis, by pneumonia, by pulmonary phthisis, or by emphysema. It proves a useful remedy also in cases of cardiac pulsations, and in irregular or too frequent action of the heart. In albuminuria, M. Brugnoli thinks the administration of nux vomica has retarded its progress to some extent, especially in cases of scarlatinal albuminuria.—*Practitioner, Feb.* 1872, *p.* 118.

DISEASES OF THE ORGANS OF CIRCULATION.

34.—THE TREATMENT OF HEART DISEASE.

By Dr. J. Milner Fothergill.

[In the treatment of heart affections the first thing to be considered is the removal of the cause. This is of course chiefly of moment in cases which have not advanced far, and are consequently capable of repair. The *causes* of cardiac disease are]

Excessive exertion, Bright's disease, imperfect nutrition, nervous exhaustion, pericarditis, and last, not least, endocarditis, with its consequent valvular lesions, for which there is no repair, and only more or less imperfect compensation, of varying permanency, to be hoped for. To this list must be added syphilis, which is not only a cause of congenital malformation, but affects the muscular structure of the heart itself, as one of its ulterior actions.

Taking them in the above order, we come now to consider the effect of strain on the heart, as the result of violent muscular exertion, *pur et simple*, without any want of nutrition, or other failure, in the heart itself. This is not associated with an active or industrious life, with exertion prolonged as a daily task, a life-long duty, but with violent temporary exertion, the consequence of labour in some forms of industry, or in voluntary effort, as in the gymnast. This cause is seen largely in action in hammermen, who have to put forth all their strength and endurance during the brief period of time that heated iron remains sufficiently hot to be worked. During the process of heating the man is resting, true, but the vascular excitement and heaving respiration tell how severe is the strain to which the viscera have been subjected. Let us consider what the *modus operandi* is here. Firstly, there is violent action of the muscles of the upper limbs, which compress the arteries, and even act more or less across them (the musculo-cardiac function of Wardrop), and form a decided obstruction to the flow of blood from the left ventricle. The same action of the muscles, on the other hand, tends to drive the venous blood towards the heart. The blood is thus collected in the venous centres—viz., the venæ cavæ, right heart, and pulmonic circulation. The chest is also more or less fixed, as having points from which the muscles work. When each "heat" is over, on examination, the "striker" will be found panting, with violent action of his heart, and bounding pulse, gradually subsiding into a balance of the arterial and venous circulations, until the next effort, which is not far distant.

It is obvious that here there is, of necessity, a strain put on all parts, from the obstructed lesser arteries backwards to the aorta, left ventricle, and the truly venous parts, the pulmonary vessels and right heart. The morbid results manifest themselves in various parts. For instance, in dilatation of the aorta, with atheroma in the distended endarterium; in thickening and distorting the aortic valves, driven together with excessive force; in yielding of the left ventricle; and inevitably in dilatation of the pulmonary artery, with dilatation and hypertrophy of the right ventricle. The last is almost unavoidable, and tends to perpetuate the mischief inaugurated in the left ventricle, thus—the enlarged and altogether bigger right ventricle sends a greater mass of blood through into the left side, and thus the left side is filled under greater pressure, a well-known cause of dilatation of the ventricle. Its contraction, in turn, throws a larger volume of blood into the aorta, and keeps up the condition of over-distension. This excessive aortic distension leads to a fuller supply of blood to the coronary arteries, and full nutrition of the heart. For a time this condition is maintained, but sooner or later the action changes into some distinctly morbid condition. The distended aorta may be ruptured by a blow, and a true aneurism formed by cracking the brittle inner coat; this distension may go on to atheroma and its consequences; the aortic valves may become diseased and imperfect, or the muscular walls may yield. The disease of the aortic valves has a curious effect on the muscular walls. Hypertrophy, and that to an unusual extent, supervenes, but it is not permanent. Mauriac has most ingeniously shown, that when the aortic valves become imperfect, the blood column in the aorta, no longer perfectly arrested in its backward course by the semilunar valves, does not, on the arterial recoil, perfectly fill the coronary arteries, and sooner or later the hypertrophied walls become degenerated from imperfect nutrition. The changes in the arterial system act on the parts behind them; but the venous system not unfrequently is itself the first to yield. Thus, from the pulmonic congestion comes tuberculosis, emphysema, &c.; from yielding of the right ventricle we have dyspnœa on slight exertion, palpitation and true cardiac asthma. This condition is common among hammermen, colliers—especially when working in constrained positions, which can only be maintained for limited periods, which, however, test their endurance—among gymnasts, &c. When, then, any of the earlier symptoms of heart failure, or failure in the other organs, manifest themselves, it is only too obvious that the form of labour which evokes the failure must be abandoned; for the causes which could induce disease in the healthy heart must have a sadly deleterious action when that organ is commencing to yield, and the various forms of compensation to become imperfect. No measure, however successful in affording relief to symptoms, can be expected to endow these organs with the virtue to resist the stress with impunity. By rest, where practicable, and a change of labour—some new occupation not necessitating violent effort, where labour in some form is unavoidable—the changes in the muscular walls may be greatly modified (in proof of this can be adduced the passing away of the hypertrophy of the left ventricle of pregnancy on parturition); and a return to the normal state more or less completely attained. But if the muscular walls have grown equally with the demand on them, and the changes have commenced in the arteries, nothing known in the present state of medicine can in any way restore them. Here the removal of the cause can alone promise anything, or hold out the hope of a prolonged existence.

[Considerable difference of opinion has existed as to the relation of cardiac to renal disease. The opinions of Dr. George Johnson, however, appear the most correct and satisfactory. He shows that in kidney diseases there is]

An hypertrophy of the muscular coat of the arterioles generally, and evolved this theory. The walls of the arterioles are largely muscular, and have a "stop-cock" action in altering the calibre of the arterioles, and thus permitting a larger or smaller bulk of blood to pass to the various tissues on the arterial recoil. The retained urine salts in the blood poison it, and the arterioles contract to prevent the entrance of poisoned blood into the tissues. This constitutes an obstruction to the blood current, and changes in the left ventricle are inaugurated.

The *treatment*, by removal of causation, of the cardiac troubles consequent on kidney mischief, which will be found most effectual, will be the removal of the accumulation of the products of histolysis, since treatment of the kidney itself is impossible.

For the achievement of this we possess two totally different means—either action on the kidney, such of it as remains useful, or the bringing of other organs into increased action; usually both combined will be found most effectual. Thus, an active purgative, by exciting the bowels, will aid in the elimination of the offending material; diaphoresis is an excellent form of wielding this power of other organs to aid in the elimination of material somewhat special to another organ. The most persistent form which the products of retrograde tissue metamorphosis assume is that of uric acid; and experience has told me that potash and buchu, with colchicum, better relieve palpitation and the other evidences of heart disturbance, in Bright's disease, than did digitalis, belladonna, &c., without this truly specific treatment. The retained uric acid, which can only exist in the alkaline blood, in union with some base, unites with the potash, and in this highly soluble condition passes readily out through the kidneys by dialysis. The action of colchicum and buchu is to increase the elimination of the urine salts by acting on the renal secreting cells, and the consequence of this treatment is a decided increase in the specific gravity of the urine, not attained by diminution of its bulk. This treatment of the heart trouble, consequent on renal impairment, by removing the excess of waste material in the blood, is a most important point in the checking, and, to some degree, arresting the morbid process already instituted. There is no doubt that the constant presence of uric acid in the blood still further aggravates the kidney mischief already existing. The question of the retention of urea in the blood in acute congestion of the kidney and uræmia, does not bear on the subject under discussion here.

No treatment, however should be persisted in until anæmia be produced. Defective nutrition is one of thegreat primary sources of heart failure, and as a secondary factor it very commonly hastens the setting in of loss of that compensatory hypertrophy, which has hitherto ameliorated the mischief done. The tendency of Bright's disease is towards imperfect nutrition, and when this is threatening the treatment must be accompanied, or followed, by the administration of iron. For this purpose the potassio-tartrate is eminently suited, and goes well with the bicarbonate of potash. The effect of blood-poisons of all kinds is decidedly to check blood-formation, as for instance, Bright's disease, syphilis, malarial poisons, lead, &c., and their elimination by their respective specifics, potash, mercury, quinine, iodide of potassium, &c., is the first step to be attended to in the treatment of the affections to which they give rise. In all these conditions the addition of iron, to aid in the formation of new blood, is of great service. In case of heart failure all conditions of anæmia should be carefully examined and submitted to the treatment adapted to each. Thus, leucorrhœa and menorrhagia frequently cause the anæmia which leads to imperfect nutrition and consequent heart failure.

Pretty much the same effect upon the heart as is produced by anæmia, is produced by nervous exhaustion, only here the action is more special upon the heart. The effect of nervous exhaustion is largely located in the organic nervous system, and the viscera supplied from it. The effect of this nervous exhaustion is to impair the sensitiveness of the cardiac ganglia, upon which depends cardiac contraction. The sensation of distension of the muscular fibres is recorded by the cardiac ganglia, and contraction follows, as in all hollow or spherical masses of muscular fibre. When, then, from impairment of sensibility a condition of over-distension results, there follows imperfect power of contraction, and thus dilatation is inaugurated. This imperfect contraction is thus productive of dilatation and other consequent troubles. The ventricle not thoroughly emptied is already, on diastole, partially full, it is soon full to over-distension from the pouring in of blood from the gorged auricle and veins behind: imperfectly emptied, it is soon over-full, and frequent imperfect contractions take the place of normal contractions. This condition not only tends to perpetuate itself, but sooner or later to lead to degeneration of tissue. A small bulk of blood only is thrown at each ventricular systole into the aorta; it is imperfectly distended, and consequently its recoil is diminished; this aortic recoil is the propelling power for the coronary arteries; the lessened flow diminishes the nutrition of the heart-walls, and consequently degeneration results. At the same time, too, every additional contraction in the minute diminishes the heart's period of rest. All causes, then, which increase the rapidity of the pulse, and especially when this increase in frequency is accompanied by loss of power in each pulsation, should be avoided, as excitement, anxiety in *speculation*, excessive drinking, the denial of sleep, especially in the *horizontal posture*, tobacco-smoking, and, certainly not to be omitted, the excessive *use of tea*. The effect of agents which lead to nervous exhaustion and increas-frequency of heart contraction, with loss of quality, is strictly analogous in ct to imperfect nutrition or anæmia. The complete or partial removal, ere complete removal is not feasible, of these causes is imperatively demand- in the treatment of the heart failure which results from them.

[Dr. Fothergill then passes on to consider our means of acting on the heart elf.]

Can we increase and diminish the completeness and power of the ventricular ntractions? is a question which can now be answered in the affirmative. ere do exist agents which positively stimulate the heart-walls into increased ntraction. Thus the condition of the heart-walls as regards distension is ought within the reach of remedial agents administered internally. The ef- ct of such an agent in the condition described immediately above, is to cause e ventricle to contract more perfectly; and thus it is not only relieved from e danger of paralysis from over-distension, but aided to recover itself. No dative to the heart is needed here betwixt its distension and the risk of para- sis from the carbonic acid of the highly venous blood within it: it is only ready too powerless; but some agent which will enable it to contract more ficiently. That we are in possession of such agents is certain. The chief e in use is digitalis.

Physiological experiment demonstrates what clinical observation had long spected, that the effect of digitalis is to produce more perfect action of the entricles. The condition of complete paralysis by aconite, identical with that f the over-distended heart, was quickly relieved by the administration of digi- lis; and not only so, but that previously paralysed heart, in extreme disten- on, can be brought to a standstill in the opposite condition of complete con- action, if enough of digitalis be administered. In practice, then, digitalis can e administered with excellent effect in this condition of over-distension of e right ventricle, and has been so used. In all conditions where the muscular alls of the heart are unequal to the demand on them, whether from valvular isease or other cause, digitalis can be given, not only for a time, but continu- sly and uninterruptedly for years, with excellent effect. The continuous d persistent administration of digitalis in cardiac inability not only secures e immediate effects of improved ventricular contraction, but has an ulterior fect upon the heart itself; and oftentimes the heart, getting further and fur- er distended and enfeebled, sending less and less blood into the aorta at every stole, and thus eliciting less aortic distension and consequent recoil, and with at diminished recoil an impaired coronary circulation and supply of blood to self, thus ensuring more complete muscular failure, is, by digitalis, enabled contract more perfectly, to elicit better aortic distension and recoil, better pply of blood to itself, and improved nutrition; while every contraction few- in the minute is adding to its period of rest. Thus, not only can a down- ard progress be arrested, but even a career of improvement inaugurated, hich in simple dilatation or distension may result in perfect restoration to alth. Where this cannot be accomplished, not only may life be made more arable, but even be prolonged; and the question of the importance of pro- nged life in individuals is one the importance of which has been pointed out Quetelet (Sur l'Homme); while that importance itself is materially en- nced by combining with it a power for more or less labour, corporeal or ental; and that that is within the clear limits of possibilities is no therapeutic tion, whatever doubts may be cast by some on the possibility of perfect storation to health. Even in the worst cases patients cling to existence, and mething may be done to relieve the suffering which results from heart fail- e by thus acting on the muscular walls by digitalis.

Along with this direct treatment of the heart itself may, and often must, be mbined diminished call for exertion on its part; rest is imperative for a ne, or more or less permanently, and the improvement in the coronary circu- ion must be supplemented by improvement in the blood itself. Iron, cod- er oil, arsenic, and the other means of improving blood-formation, and from again improved nutrition, are aided by the vegetable tonics strychnine, qui- ne, &c., which improve the digestive powers of the stomach. As to iron, ller says that the continuous administration of iron for three or four years s resulted in the removal of every physical sign and rational symptom of dilatation. Thus the heart itself has a power of recovery when furnished merely with an improved blood-supply without the aid of means acting directly upon itself. In practice, the combination of different measures may be indicated or demanded, and some of the formulæ most used by the writer were these—

1. ℞. Pot. bicarb. gr. v. or x.; ferri pot. tart. gr. v.; tinct. digital. ♏ x.; inf. calumb. ℥ 1, ter in die.
2. ℞. Tinct. digital. ♏ x.; sp. chloroformi, ♏ xxv.; inf. buchu, ℥ 1, ter in die.
3. ℞. Tinct. digital. ♏ x.; liq. strychnia, ♏ v.; tinct. fer. mur. ♏ v. or x.; aquæ or inf. quass. ℥ 1, ter in die.
4. ℞. Tinct. digital. ♏ x.; sp. æth. nit. ♏ xxv.; tinct. sem. colch. ♏ x.; inf. buchu, ℥ 1, ter in die.
5. ℞. Pulv. digital. gr. ½.; pulv. ferri sulph. exsic. gr. ¼; pulv. pip. nig. gr. 1; ext. gent. or pil. al. et myrrh, q. s. 1, bis in die.
6. ℞. Tinct. belladonnæ, ♏ xx.; tinct. nucis vomicæ, ♏ x.; mist. camphoræ, ℥ 1, ter in die.

These different formulæ may be useful, and the second is especially agreeable in simple cardiac debility; the first is better when washed down with a tumblerful of water, and with the pills a patient may go away on a visit very comfortably. Thus by digitalis we can, more or less, according to the nature of the case, and the integrity of the heart-walls, act upon a heart whose contractions are becoming incomplete. This is the centre point of action upon the heart directly; for we must all be only too painfully aware that any action on diseased valves is beyond the reach of remedies, in this age as yet. But whether the origin be valvular failure or other cause, as long as the heart can contract completely a better state of matters is maintained than when the heart-walls fail. It is this failure of power, this inability to contract completely on the part of the ventricles, which is the thing we have to dread as being the end, or at any rate the beginning of the end, of the case. It may not be out of place here to add that in electricity we possess a power that may be of great service in the treatment of heart failure. The effect of a feeble electric current is to increase the force and lessen the frequency of the heart's action: as the writer has observed with a small Stohrer's battery, one pole placed over the heart and the other to the nape of the neck.

Rational treatment of Sequelæ.—The third and last division of the treatment of heart affections is only of less importance than the preceding divisions, because it does not so much prevent as alleviate evils. Still, when from inability in the centre of the circulation we have decidedly venous congestion, and from it, again, a sort of re-balancing of the system arterial and venous, we have the different viscera in a state of asthenic congestion, a condition unfavourable to the performances of function. Thus, we have gastric catarrh, congestion of the liver, diminished flow of urine, and tendency to disease of the kidney, as the consequence and not the cause of heart disease, in females uterine discharges menorrhagic or leucorrhœal; so we have effusion through the lining membrane of the air-passages, and a flow of fluid, chronic bronchitis or bronchorrhœa, œdema of the lungs, &c., &c., and finally general anasarca. Now, it is obvious that where we hope not only to relieve matters, but to inaugurate improvement, we must relieve the viscera from a condition unfavourable to improved nutrition. For this purpose we can use purgatives, diuretics, diaphoretics, and even expectorants; all agents affecting the secretory and excretory power of the different organs.

Purgatives, and more especially drastic cathartics, have been long resorted to with the intention of calling out the action of the bowel; and the different effect of two drachms of pulv. jalap. co., with a grain of gamboge in it, on a patient with advanced heart disease, to its effect on a healthy person, speaks volumes. The latter would feel decidedly depressed, but the sufferer from heart disease feels enlivened, sprightly, and altogether improved. Four or five copious watery evacuations have relieved his venous congestion, relieved his distended right ventricle, and improved his pulse, while the relief to the cerebral congestion manifests itself in his improved spirits. This method of acting on the viscera in the congestion of heart failure is deservedly a favourite one; and for the purpose of exciting free catharsis the usual means are pulv. jalap.

co., pulv. scam. co., elaterium, and gamboge. Gamboge rubbed up with bitartrate of potash and black pepper, or other carminative, is a capital cathartic. All may have to be used in turn, as one form soon loses its efficacy. Chronic diarrhœa is not in this condition to be arrested, except by action on the skin, unless it is clearly threatening the patient's existence; it is a natural drain, as much as eczema in hydrops. Purgatives are often thought to have a diuretic effect in heart affections; this is the impression of experience as a fact. Really, the improvement in the circulation is followed by freer circulation through the kidneys and better arterial tension, and consequently there is better diuresis of water.

Diuretics have not only always been resorted to in the treatment of dropsy, which was, until recently, regarded as a disease *per se* and not a sequel, but the improved or unaltered flow of urine has been adopted as a sort of test of the efficacy of the remedial measures resorted to. Long before the experiments of Paff, or Winogradoff, Handfield Jones, and Brunton, on arterial tension, popular observation had learnt that an increased flow of urine indicated an improvement in the patient's immediate condition in dropsy; and it is therefore, in the agents classed as diuretics that we find digitalis, squill, belladonna, &c., which we now know exercise their diuretic action by virtue of the increased arterial tension, the result of more perfect ventricular contraction. The water excretion by the kidneys is a question of hydraulics—a purely mechanical result of the pressure of the blood on the thin-walled glomeruli of the Malpighian bodies. When the arterial blood is driven in but feebly, and the venous is almost stagnant, we find the bulk of urine very low indeed: but its sp. gr. may be very high, for the bulk of urine and the elimination of solid excreta by the renal secreting cells bear no necessary relation to each other. No doubt, too, certain other agents act as diuretics, as juniper, turpentine, potash nitrate, nitric æther, &c., by their being direct stimulants to the kidneys, and thus inducing an increased flow of blood to these organs. But it is in the first division we find our agents which are the best diuretics in heart failure, and what position scoparium should hold it is difficult to say from want of observation and experiments, but it will probably be found among the first named. The other class of diuretics, as colchicum, cantharadine, and buchu, &c., which increase the amount of solids without effecting the bulk of urine, are not to be regarded as diuretics in the treatment of heart failure. Stimulants are diuretics insomuch as they increase the completeness of the ventricular contractions and improve arterial tension.

Diaphoretics are not yet sufficiently adopted as a means of affording relief in dropsy and the sequelæ of heart failure; but it is not only a very effectual, but, in my experience so far, a perfectly safe way of increasing water elimination, especially when the heart failure is accompanied by renal disease. The sitz-bath is a good form, or any bath which permits the patient to breathe freely; Turkish baths are regarded as not very safe. Another plan, employed by the late Sir James Simpson, has been so useful that it cannot be omitted, and that is, to fill six or eight lemonade bottles with boiling water, cork them well, nd draw over each a woollen stocking wrung out of hot water, then pack hem round the patient in bed, who in fifteen minutes or so commences to weat profusely: they can be continued for an hour, or repeated when necesry. This plan can be resorted to with the poorest patients, and when a paent of any class is confined to bed. In obstinate cases, where the skin will t be induced to relax by hot baths, cold packing has been resorted to in der to paralyze the minute vessels of the skin, and thus produce dilatation of em in cases of chronic kidney disease; but I am not acquainted with the tails as to whether cases with distinct heart failure were so treated or ot.

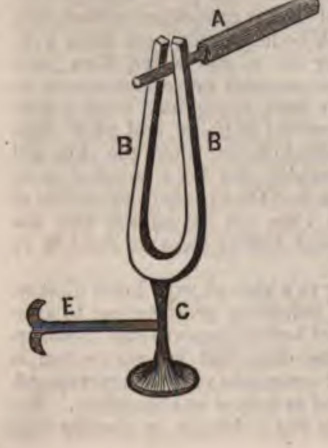
Dropsy is not only a great cause of suffering to a patient with heart disease, at is regarded by non-professional people as indicating great danger to exisnce. It is, no doubt, a sign of failing power of serious import; still its occurence often gives temporary relief. When the distended venous system is elieved by a sudden general œdema, great and immediate relief is experienced, nd that may be utilized to improve matters and to induce re-absorption. But usually *dropsy commences* insidiously, retreats for a longer or shorter time under proper treatment, but usually it is not long in returning, and is more persistent on each reappearance. If, during the interval, the patient has been under good treatment, and yet in spite of it it returns, it is a prognostic sign of the very worst import: no worse sign can exist, and its reappearance calls for the most energetic treatment. Sometimes œdema is very capricious in its seat, and flits about in a curious to-and-fro manner; in one well-remembered case, where the lesion was tricuspid, the oscillations from legs to lungs and back again were very marked. If the patient was found with a less agonized countenance, his legs were tremendously swollen; when he was gasping for breath, his legs were fallen. Dropsy, as a sequel to heart disease, is amenable, more or less, to the measures cited above, in addition to which is the question of puncture. Nature oftentimes establishes a drain of her own; not always with good effect. Puncture, or incision, is, in my personal experience, in most cases worse then useless, only adds a wet bed to the patient's sufferings. One case only can I recall where the benefit was decided and unequivocal. Others have had a less unpleasant experience, but it is in dropsy from heart and kidney disease combined that the benefit of puncture or incision is chiefly seen. In simple heart failure it is a hazardous proceeding, and sometimes the carbonic acid poisoning, the irregularity of pulse, dyspnœa, &c., commence to increase on the establishment of the drain. In one case this occurred when the serum spirted out of the needle-holes in an arterial jet, and the bed was soon in a perfect puddle, and could not be kept otherwise during the six-and-thirty hours the patient survived. Still the puncture, or incision, is among our armamentaria, and does frequently afford more or less relief; but the incisions may become troublesome sores, or even slough.—*Edin. Med. Journal, March* 1872, *p.* 776.

DISEASES OF THE ORGANS OF RESPIRATION.

35.—ON THE APPLICATION OF THE DIAPASON TO CLINICAL AUSCULTATION.

By Dr. W. Handsel Griffiths, Assistant Librarian to the Royal College of Surgeons, Ireland.

An extensive study of the science of acoustics with reference to clinical auscultation leads me to suggest, as a valuable adjunct to physical diagnosis, a modified form of the ordinary diapason, or "tuning-fork."



It is a well-known fact that the intensity of sound evolved, or the degree of resonance elicited, by a vibrating diapason, varies with the nature of the substance or surface on which it rests. I have been lately engaged in an experimental inquiry with a view to define the value of this principle as applied to clinical auscultation, and have been much gratified by the result. The form of diapason which I have hitherto used is intelligibly figured in the annexed cut. B B are the "prongs," which, on being suddenly separated by the "sounder," A, vibrate, and so evolve sound; C is the "connector" by which the sound is communicated to the small bell-like "dome" (similar in general size and shape to the small end of the stethoscope); and E is the handle by which the instrument is held in apposition with the surface of the organ or part under examination.

Now, although the variable resonance afforded by this instrument on applica-
ı to different regions of the body is most remarkable, and beautifully indi-
ive of abnormal condition or altered structure, I conceive that it is possible
nvent a more delicate and definite indicator; but it would probably be in ac-
dance with the same principle.

t is not in pulmonary affections only that such an instrument is likely to
ve valuable; in almost every abnormal condition of almost every organ the
ications afforded by it are likely to prove of inestimable service in forming
agnosis.

ow I am not promulgating these suggestions as the result of mature or
fected research, but I have thought it expedient to publish them, crude as
y are, in the hope that the attention of abler minds and more acute intellects
y be attracted to the subject. The field of observation thus opened is, I
eve, a wide one, and likely to repay a careful and protracted investigation.
ust that the co-operation of contemporary physicists and physicians will
ble me, ere long, to offer to the profession a standard instrument, constructed
strict acoustic principles, which will afford definite and delicate indications
variable resonance, and which will be reliable as an index of altered struc-
e and organisation.

am not aware that the idea of applying the diapason, or any similar instru-
nt, to clinical auscultation has hitherto been suggested; but if so, I trust that
ay be corrected, and that the credit of priority will be claimed by the origi-
or. At all events I am convinced that the subject, if not novel, is well
thy of revival.—*Lancet, Dec.* 16, 1871, *p.* 848.

—ON THE PATHOLOGICAL AND THERAPEUTICAL RELATIONS OF ASTHMA, ANGINA PECTORIS, AND GASTRALGIA.

By Dr. F. E. Anstie, Senior Assistant-Physician to Westminster Hospital, &c.

e following is the proposition submitted in this paper:—that asthma, angina
toris, and gastralgia (or neuralgia of the gastric nerves) are essentially
endent on neurosis of the vagus nerve, which is of central origin, and in a
e majority of cases is mainly or entirely to inherited peculiarities of the
tral nervous system.]

he evidence arranges itself under five sections:

. Inferences from the known physiological functions of the vagus;

. Evidence of the interchangeability of asthma, angina, and gastralgia, in
same individual;

. Evidence of the pathological connection of these neuroses with neuralgia
he fifth nerve;

Evidence of the common dependence of asthma, angina, gastralgia, and
ralgia of the fifth, on peculiar inherited neurotic tendencies;

Evidence from the similarity of effects produced by certain remedies on all
e maladies.

As regards the physiology of the vagus, there is no need to deliver a
s-room lecture to the present audience. It will be enough to remind you
, besides its other offices, the pneumogastric acts as the great afferent of
ory impressions from the stomach and from the mucous surfaces of the
s and trachea, and as the main, if not the sole, organ of cardiac sensibility;
its ramifications form an integral part of each of the various nervous
uses in the cardiac, gastric, and respiratory territories; and that its deep
in is placed within the medulla oblongata, and united by the most intimate
ections with the sensory nucleus of the fifth nerve. There are thus *prima
e* reasons for presuming that the three neuroses, asthma, angina, and
ralgia, would all depend upon irritation of the vagus. As regards asthma,
e is I suppose a pretty general adhesion among the best recent authorities
he theory of irritation of the pneumogastric, which is well expressed by M.
of Paris in an article on the Treatment of Asthma in the Practitioner for July,
). As regards the pain of angina, and indeed as regards the sources of

cardiac sensibility in general, there has been much dispute; but the facts see to me to point very clearly to the belief that branches of the vagus, reinforced the cardiac plexus by other spinal branches which come from the cervical the uppermost dorsal cord-centres, through the medium of the sympath ganglia of the neck, fully account not only for heart-sensibility, but also the curious transmission of pain in angina to the arm, the chest-wall, and neck. The chief objection to this view has rested upon the belief that sympathetic branches must also have a share in the production of the pain; the idea that the sympathetic is capable of expressing sensations of pain seems to me to rest on an exceedingly doubtful basis, and to be supported chiefly by the apparent analogies of the pain of colic, gall-stone, &c., for which pains there is really a much more probable source in branches of spinal nerves. Everything in recent physiological observation seems to suggests that all the apparent cases of sensation in the sympathetic fibres are really due to branches of spinal nerves that are bound up with them. As regards angina, the only question as to which I feel much doubt is, whether the vagus centre or the cervical and upper dorsal cord-centres just referred to are most prominently concerned in the origination of the disease; but, at any rate, it seems clear that, from the moment that serious symptoms of angina are set up, the vagus centre must be involved, since this must be the intermediate instrument of the reflex disturbances of respiration which occur during the paroxysms. On the whole, I lean strongly to the idea, which will be found expressed in the pathology chapter of my work on Neuralgia, that the irritative process in angina commences in the spinal centres which send branches to the cardiac plexus, and then involves the vagus secondarily, but at a very early period.

2. The second branch of the evidence that connects asthma, angina, and gastralgia, all with the same great nerve, is derived from the singular interchangeability of the three maladies. Kneeland long ago remarked that spasmodic asthma and angina were thus apt to pass the one into the other. For my own part, I can refer to several instances of the connection between all three diseases, and would sum up the general results of what I have seen in the following terms. 1. Severe and protracted asthma nearly always becomes complicated, after a time, with symptoms which may not amount to what would commonly be called angina, but assuredly belong to the same category; and in not a few instances (I have myself seen three) asthma leads directly into declared and unmistakable angina. 2. Asthma is nearly always associated with gastralgia. It is true that the neuralgic character of the pain is very often overlooked, and it is attributed incorrectly to dyspepsia; but I have frequently had occasion to prove the fallacy of this idea, the only ground for which is, that asthmatic patients will not bear putting a large amount of food in the stomach at once. Asthmatic patients are not commonly dyspeptic in the proper sense of the word; and the epigastric pain, from which they almost all suffer more or less, requires to be dealt with as a part of the general nervous affection. 3. The chronic form of angina is, so far as I am aware, almost invariably associated with more or less gastralgia, which, as in the case of asthma, is continually being mistaken for dyspeptic pain, and treated, with very unfortunate consequences, from a gastric instead of a nervous point of view. Like asthmatics, these patients require to be fed in small *meals*; but nothing save harm can come of keeping them on a small total *supply* of nutriment.

3. The next branch of evidence for the essential interconnexion of the three neuroses of which we are speaking is derived from the way in which they are mixed up and complicated with neuralgia of the fifth nerve—a complication which, if it can be shown to occur with a certain special frequency in these diseases, would go far to trace home the *fons et origo mali* to that portion of the medulla oblongata in which the nuclei of the vagus and the fifth are found in intimate connexion with each other. Upon this point the evidence is, in my experience, exceedingly strong, and may be shortly stated thus: that, whereas neuralgia of the trigeminus is a more or less occasional complication or attendant of other neuralgias generally, it is an almost invariable complication of nervous asthma, nervous angina, and gastralgia. Of course it varies greatly in severity. It may amount, and indeed generally does, to nothing but an attack of migraine every month or six weeks; but there it is, nearly always. And, as

ards gastralgia, I have seen numerous instances of its interchangeability th facial neuralgia, of which the following is perhaps the most striking. A tchmaker's assistant, aged 42, applied to me at Westminster Hospital, suf- ing from intermittent attacks of epigastric pain, which came on specially at nes when he was exhausted from want of food, and were uncommonly severe d prostrating. After he had been under my care for ten or twelve days, ese pains were abruptly superseded by violent neuralgia in the globe of the ht eye and in the branches of the ophthalmic division of the right fifth nerve nerally. This neuralgia was not merely severe in itself, but it ran on into condary iritis, with destruction of the eye for visual purposes. This is the ost severe case of trigeminal neuralgia complicating gastralgia that I have en, but I have witnessed plenty of slighter ones; and on the whole, I believe at true gastralgia, as distinguished from mere dyspeptic stomach-pain, is arly always attended with a tendency to facial neuralgia in some form and egree.

4. In the next place, the close interconnexion of asthma, angina, and gas- algia, and their common dependence on an affection of the central nervous ystem, is rendered more probable by the remarkable history of inherited neu- osis which the sufferers from them will always be found, upon careful inquiry, o present. This is a branch of investigation which has greatly engaged my ttention; and in my work on Neuralgia, the results of a good many such nquiries as to the pedigree of neuralgic patients will be found. But I have not ad space for everything in that work, and must probably reserve for a separate ublication the remarkable facts which come out respecting the hereditary con- exion of asthma and angina pectoris. Respecting gastralgia, it has been, for bvious reasons, impossible to procure evidence of a precise and reliable haracter; but as respects asthma and angina pectoris, I will go so far as to ay that I believe there hardly ever existed a sufferer from one of these diseases n whose blood-relations, either of the same or previous generations, one or nore examples of the other could not be found by proper inquiry; and occa- ionally one comes across families which seem to have been quite plague- tricken, as it were, alternately with these two maladies. And it is a singular act that I have twice known these diseases to be the only nervous maladies hat could be traced in a large family of two or three generations.

I may here properly introduce the reply to an expression of surprise which ome of my audience may have on their lips, at the frequency which I ascribe o angina pectoris, which, on the contrary, has usually been considered a very are disease. But I have already had the honour to tell this Association (at he meeting in 1868) that observation had convinced me of the error of that pinion, which arose from neglect or misunderstanding of all but the everest and suddenly fatal cases of angina; whereas there are, in fact, a mul- itude of cases in which the nervous affection—the cardiac neuralgia—which is he only essential portion of the disease, exists in all shades of severity, down an exceedingly mild and trivial complaint. And my subsequent experience s strongly confirmed me in this belief, which is also expressed by some of the ost careful recent writers.

5. The last, and to me personally the most interesting, link in the chain of dence which connects together the three neuroses asthma, angina, and gas- lgia, is formed by the results of therapeutical experiment. There is one edy which is supremely effective, where it can be tolerated, in all these ee maladies; namely, arsenic. As regards angina, I may say that, since my ntion was drawn to arsenic by a remarkable case published by Philipp in 5, I have had a rather large number of severe cases to treat; but it is within last twelve months, that I have received the most conclusive proof of the ver of this drug over the nervous symptoms of angina. Two examples ecially illustrate this, the subjects being respectively aged 65 and 75. Each these gentlemen was free from recognisable organic heart disease; but there s much probability in both cases that there may have been a certain amount ossification of the coronary arteries. Be that as it may, in both these cases, enic, in five-minim doses of Fowlers's solution three times a day, relieved the tacks very rapidly, and completely removed them in the course of about a rtnight. There *had previously* been daily paroxysms in each of these cases;

and the elder of the two gentlemen was the more struck with the rapidity and completeness of the relief obtained, inasmuch as he was a medical man, and was, of course, thoroughly well aware of the intractability and the formidable nature of the disease. As regards gastralgia, the efficacy of arsenic was some years ago pointed out by Dr. Leared; and I have had several opportunities of proving the correctness of his statement. But more especially is it an effective remedy in that form of gastralgia which accompanies asthma. I have for many years been accustomed to the use of arsenic in asthma, and had seen great benefits produced by its tonic effect. I had also noticed, without thinking much about it, that the tendency to gastralgia, and also to facial neuralgia, was at the same time remarkably diminished; but at present I cannot regard the coincidence as accidental. In regard to the effects of arsenic upon asthma and angina, it seems proper to refer to the facts, long disputed, but now established as true, respecting arsenic-eating in Styria. It has been ascertained beyond doubt that many natives of that country eat arsenic, with very remarkable benefit to their power of ascending steep mountains. It enables them to do this comparatively without distress to breathing and circulation. There are, however, a considerable number of persons who cannot take arsenic in the doses and for the prolonged period which are necessary to make any permanent impression upon either of the three neuroses of which we have been speaking. These patients suffer such irritation of the stomach or bowels from it, that they cannot continue it. For the relief of such persons, I beg leave again to suggest a remedy that I mentioned in my paper on Visceral Neuralgias in 1868, and which I have since found very useful in spasmodic asthma, as well as in gastralgia and angina; I mean the subcutaneous injection of strychnia, in very small doses, from the 1-120th to the 1-80th or 1-60th of a grain. I would always try the subcutaneous administration before giving the remedy by the stomach, wherever it is possible to do so; but, if this cannot be done, strychnia should be given by the mouth in doses of 1-40th to 1-24th of a grain three times a day. I think there is little doubt that strychnia also has a physiological affinity for the tract of the vagus and the trigeminal nerves; also this is much feebler than that of the arsenic; hence I should always recommend that the latter be tried first.—*British Medical Journal, Nov.* 11, 1871, *p.* 550.

37.—A FEW REMARKS ON THE TREATMENT OF ASTHMA.

By Dr. John C. Thorowgood, Assistant-Physician to the Hospital for Diseases of the Chest, Victoria Park.

Experiments made by Paul Bert, Traube, and others, have shown that one result of irritation of the pneumogastric nerve is to cause contraction of the lungs and arrest of respiration; this arrest being more easily produced during expiration than during inspiration.

The lungs themselves contract just as does a sponge when squeezed in the hand, and so long as this contraction continues so long does the paroxysm of asthma and difficult breathing exist.

Prolonged expiratory efforts, as by a violent burst of coughing or a fit of laughter, are very apt to determine a fit of spasmodic asthma in one predisposed thereto, illustrating the fact shown by the experiments above alluded to, that arrest of respiration from closure of the lungs is more easily produced during expiration than during inspiration.

This will be enough of the pathology of asthma for my present purpose, since I have shown what a fit of asthma is in itself, and that it may be caused by irritation of the pneumogastric nerve.

Now, this pneumogastric nerve, as its name implies, supplies nervous filaments not only to the lungs but also to the upper part of the alimentary canal—viz., the pharynx, œsophagus, and stomach, and hence any irritation of these parts may show its effects in the form of contracted lungs and asthmatic paroxysms.

These are matters tolerably well known and understood, and yet in the practical treatment of such a complaint as spasmodic asthma we seem hardly to get that help from this physiological and anatomical knowledge to which we are fairly entitled.

Efficiently to treat spasmodic asthma in its various forms we must recollect that we are dealing with a pulmonary neurosis, and endeavour to allay any irritation of the pneumogastric nerve as much as lies in our power.

Commonly we find in a paroxysm of pure spasmodic asthma that medication of those filaments of the pneumogastric that go direct to the air tubes and lungs by means of certain fumes and vapours, as, for example, burning nitre paper, or the smoke of stramonium or tobacco is sufficient to relieve the spasm and cause relaxation of the air tubes, but these are at best but temporary expedients, and naturally patients and physicians seek after some means of cure likely to be more radical and permanent in its effects.

So far as the experience of the present writer goes this may be found often in treatment carefully directed to the stomach and liver in the first instance; and, secondly, in the use of certain tonics that have in some way or other a permanently curative power over neuroses of the lungs and heart.

In observing patients with various chronic maladies of the lungs, we often have brought before us the sympathy that exists between the pulmonary and gastric organs. For example, E. J., a man among my out-patients at Victoria Park Hospital, has been almost from childhood liable to cough and difficult breathing, and he remarks that he has always noticed any overloading of his stomach with food to be invariably and at once followed by increase of frothy expectoration.

Again, Mr. B. has had asthma for twenty years, and has taken all the most approved remedies for this complaint, under most able advice, with very little real benefit. He has, however, no trace of structural disease about the heart or lungs, while symptoms point very decidedly to irregular action of the liver and stomach. The occasional use of the *Cigarettes de Joy* and a mild dosing with Carlsbad salt, brought this gentleman to a degree of freedom from asthma that he had not enjoyed for full twenty years.

Much stress is very properly laid in books on asthma on the patient's diet. He must not dine late, and certainly must never indulge in supper. Sound and sensible as this rule is, yet I believe the practice of eating nothing after midday s carrying things a little too far in most cases. A state of exhaustion may be duced which always increases any kind of nervous irritability, and further an this, the long-continued innutrition may later in life lead to actual structural change of a degenerative kind in the lung tissue.

I find in practice that a small sandwich of bread and meat, or sometimes a up of bread and milk, is very grateful to a hungry asthmatic in the evening, nd in no way injurious.

The medicines that have more or less credit in the cure of asthma are almost legion," the reason being that some fit, as it were, one end of the pneumogastric, some another. Thus bismuth and hydrocyanic acid are of great value hen the neurosis is of gastric origin. Carlsbad salt, nitric acid, and at times mall doses of mercury, are all unmistakeably curative when the hepatic system requires relief.

Other remedies, such as ipecac., belladonna, nux vomica, will cover a wider nge of symptoms dependent on pneumogastric disturbance, while if we have ason to suspect a gouty or rheumatic diathesis as the root of the trouble, alkalis with iodide of potassium, sulphur, and arsenic will be the remedies indicated. While, then, we certainly have no "specific" for the cure of asthma, e may, by considering the complaint in its wide range, as a neurosis of the eumogastric, get some very satisfactory results from treatment.—*Medical ress and Circular, Jan.* 10, 1872, *p.* 27.

38.—TREATMENT OF WHOOPING COUGH BY MORPHIA.

By Dr. John Kent Spender, Surgeon to the Mineral Water Hospital, Bath.

[The following two cases illustrate the advantage which may be obtained by the administration of morphia in extremely small doses, so as to get the sedative without the narcotic effect.]

May, 1863.—A young gentleman, æt. 10, suffering from the troublesome sequel of whooping cough; cough incessant, but the peculiar spasm of the specific disease had passed away. One twenty-fourth of a grain of acetate of morphia in solution was prescribed every two hours for the first two days; afterwards at longer intervals of time. The cough was notably alleviated almost immediately, and the patient enjoyed refreshing sleep during the first night. Scarcely any drowsiness was produced by the morphia, nor was any other inconvenience felt; not even constipation. Within five days the cough had nearly gone.

May, 1869.—A female child, æt. 5; parents not very poor; house clean and healthy. Whooping cough, rather severe; no complications. One thirty-second part of a grain of morphia with three grains of bromide of potassium, in solution, every two hours; mother was instructed to suspend the medicine for four hours at any time if unusual drowsiness came on. No medicine was given during the night. The usual auxiliary treatment as regards diet and clothing. The paroxysms of whooping cough soon became less severe, and the duration of the disease seemed shortened.—*Medico Chirurgical Review, Jan.* 1872, *p.* 217.

39.—ON THE THERAPEUTICAL VALUE OF THE HYPODERMIC INJECTION OF ERGOTIN IN HÆMOPTYSIS.

By Dr. C. Currie Ritchie, Physician to the Hulme Dispensary, Manchester.

In a suggestive paper published in the Practitioner, for Sept. 1868, Dr. Alfred Meadows advocates the extended use of this drug, which he had found extremely useful in various uterine and vesical affections; and in the same spirit I venture to submit the following observations on a therapeutical application of ergot, hitherto but little known, with the hope that the experience of others may be elicited, and so an accurate knowledge of its real value be arrived at.

It seems to me that the restricted use of ergot is the direct result of the want of a full appreciation of its great physiological action; that is, of the power which it possesses of inducing contraction in muscular fibre of the involuntary or non-striped variety generally, and not merely in that of the uterus. Although having an important bearing on the general action of ergot, the following remarks apply especially to its action on the blood-vessels, of which unstriped muscular fibre is so important a histological element.

Experimental research has conclusively shown that ergot contracts the minute arteries, and by so doing increases the blood pressure. A simple experiment will, as Dr. Meadows has shown, be sufficient to establish the former of these propositions: thus, if a single grain of ergotin in solution be injected subcutaneously into the web of a frog's foot previously extended under a microscope, in a few minutes the circulation will be seen to become much quicker, then it will stop for a few seconds and oscillate in a spasmodic, jerking manner; after about half an hour, the blood-current will gradually return to its normal even, steady flow. The well-known experiments of Brown-Séquard afford additional evidence on this point. He found that in the dog the vessel of the pia mater contracted during its administration, just as with belladonna, but that ergot acted more powerfully on the cord than belladonna: both of these agents diminish the reflex power of the cord. The second proposition, that the blood pressure is increased, receives interesting corroboration from the recent researches, chiefly with the hæmadynamometer, of Dr. Ch. L. Holmes, published in the *Archives de Physiologie*, for 1870. He finds that "ergot conracts the minute vessels by an action on their muscular walls," and "that this ontraction augments the blood-pressure in the large vessels;" it is, moreover, ound that previous division of the vaso-motor nerves does not prevent the iarrowing of the minute vessels, and that this diminution of calibre is also produced in the minute vessels of the lungs, causing a temporary diminution in the pressure of the systemic arteries. This last result has a direct bearing on the whole subject of pulmonary hemorrhages: and as it seemed to Dr. Holmes to be incapable of a scientific explanation, he performed numerous experiments in order to test its accuracy. Thus, he surmised that the effect might be due simply to the sudden introduction into the interior of the heart of a foreign body, and accordingly he substituted other substances for the ergot which he had injected into the jugular vein of a dog; but whereas the injection of ergot was with great rapidity followed by sudden diminution of the blood-pressure, which remained depressed for several seconds and then reached a higher level than before the injection, no such result followed the injection of the other substances. This sudden primary descent of the pressure-curve occurred after ergot-injection, even when it was introduced at a distance from the heart, and when the cardiac nerves had been divided previous to the injection; so that Dr. Holmes is led to the conclusion that the occurrence is due to contraction of the pulmonary vessels, "impeding or even preventing the passage of blood into the eft heart, and so diminishing the flow into the arteries and lowering their ension."

Such considerations as these lead us to hope that renewed and extended in-restigation by those who have the opportunity of inquiring into the therapeu-ical actions of the ergot of rye may assign this powerful agent its true place n our Materia Medica.

Ergot has long had the reputation of being a styptic; and as such has, in ommon with a multitude of others, been recommended in the treatment of æmoptysis. Dr. Dobell (*Retrospect*, vol. lviii., p. 119) quotes a case in Guy's lospital under the care of the late Dr. Addison, which proved very intractable ntil ergot was administered. Dr. Dobell himself recommends it, but admin-ters at the same time ergot, digitalis, gallic acid, and other preparations, hich leaves us in doubt as to which of the drugs was the means of arresting ie hemorrhage. In a paper by Dr. Herman Weber (Clin. Soc. Trans. ii, 155), remarks, "Another substance from which I have seen good effect in arrest-g severe hæmoptysis is ergot of rye in large doses, viz., from three to six achms of the watery extract of our Pharmacopœia in twenty-four hours." I ive not found the internal administration of ergot followed by the satisfactory sults so frequently obtained from the use of gallic or sulphuric acids, acetate lead, or sedatives; and in several cases it has produced such a disagreeable nsation of nausea that its use has had to be abandoned. In one case there as præcordial pain for half an hour after each dose.

An interesting paper published by Von Langenbeck in the *Berliner klinische ochenschrift*, 1869, "On the Hypodermic Injection of Ergot in Aneurism," ntains the first account I can find of its administration by this method. He gued that, as ergot induced contraction of the smooth muscular fibres, it ould be a powerful agent in producing hæmostasis. Two cases of aneurism, ie in the right supraclavicular fossa and the other of the right radial artery, ere successfully treated by this means. Dr. Dutoit of Bern (Langenbeck's *rch.* Bd. xii. No. 3) and others have successfully repeated this method of deal-g with aneurisms. In some editorial remarks appended to the medical peri-ope of the Edinburgh Medical Journal for July, 1870, a case is quoted where patient who had bled to thirty-six ounces from the lungs, within a few min-es, had his hemorrhage completely checked in three minutes by the subcu-neous injection of five grains of ergotin; and Dr. Allen Jamieson records a se (*Retrospect*, vol. lxiv, p. 108) in which the same treatment was successful-adopted in a slighter case. The last case attracted my attention to this ode of treatment, and I have since adopted it in eight cases. The ergotine ed was obtained from Messrs. T. and H. Smith, of Edinburgh, and a solution five grains in ten minims of distilled water was employed, except in Case 8, which unusual irritation was produced by it; I therefore substitute a solu-on (*prepared according to* Langenbeck's formula) of three grains of ergotin, in equal parts of glycerine and rectified spirit, which produced very little irritation.

[These eight cases are then related. In the first, five grains of ergotine were injected, with the effect of at once arresting severe hæmoptysis which had continued nine hours. The third was a case of phthisis, in which "pints" of blood had been expectorated, according to the patient's statement. There was a dry cavity at the apex of one lung, and the hemorrhage had been excited by the exertion of pulling on the boots. It had continued two days. The same dose of ergotine was injected as in the first case, with the effect of arresting the hemorrhage for two days, when a repetition of the injection permanently stopped it.]—*Practitioner*, *Dec.* 1871, *p.* 321.

DISEASES OF THE ORGANS OF DIGESTION.

40.—ON DYSPEPSIA OF LIQUIDS.

By Dr. John C. Thorowgood, Assistant Physican to Victoria Park Hospital for Diseases of the Chest, and Lecturer on Materia Medica at Middlesex Hospital.

The cases here briefly recorded have seemed to me good illustrations of that indigestion of liquids which has been carefully described by M. Chomel, in his work on Dyspepsia, published in 1857.

The affection, in its fully developed form, does not seem very common in this country, but during the last few years I have met with a few cases exactly corresponding to the description of the malady given by Chomel.

Case 1.—James McC., aged thirty, a pale, dark, intelligent man, came under my care September 14th, 1861, complaining generally of dyspeptic symptoms, and specially of the great uneasiness caused by the presence of any amount of liquid in his stomach. Liquids in the slightest degree acid were most distressing to him, and at intervals he had attacks of sour pyrosis. He complained much of dryness of mouth, with dry skin and costive bowels; urine loaded with lithates, but free from traces alike of sugar or albumen. No loss of flesh; pulse slow and soft; nothing irregular to be found in heart or lungs; no sort of tumour or thickening about pylorus of stomach, but on gently vibrating this organ, fluid was heard splashing about within it, and this sound could be always produced irrespective of any liquid recently drunk. The stomach was much distended. The early treatment of this case consisted in the use of alkalies, with bismuth and various bitters, but no improvement resulted; the only noteworthy feature was the effect of a pill of extract of opium at night, which regulated the action of the bowels so completely that the patient asked for the pills as *aperient* pills.

Often at his visits this man drew my attention to the quantity of liquid always present in his stomach, and at last we agreed that he should drink as little as possible, and take no other medicine than a powder of rhubarb and magnesia every morning. From this time he steadily improved, and after about two months' treatment he was let go cured, and six months afterwards was still in good health.

Case 2.—A man aged fifty-three years, dark, sallow, and very hypochondriacal, had been dyspeptic for eighteen months, and had taken quantities of physic. He often has pyrosis, has large sodden tongue, costive bowels, and dry skin; urine dark in colour, specific gravity 1030, acid, and nothing abnormal chemically or microscopically. The indigestion of liquids was not so marked in this case as in the one preceding, but no medicine gave relief till the dry *plan of diet* was enforced, then he amended considerably, and I lost sight of him.

Case 3.—A young healthy-looking man, aged twenty years, by trade a bricklayer, came to me complaining of faintness and palpitation of the heart, and said he had got water in his chest, for he could feel it. Examination of the chest showed the lungs and heart to be in every respect quite healthy; the stomach is large and dilated, and when he shakes the splashing noise of liquid therein is plain enough. He says he can produce this noise at any time. This patient has a dry skin; large sodden tongue; pulse 80; urine clear. No water-brash.

So far as treatment has gone during one month with alkalies, bismuth, and various tonics, little good has been done. The dry-diet plan is the only means of effecting a regular cure; but as this involves a pretty complete abstinence from beer, the patient can hardly be made to give it a fair trial. That the less he drinks the better he is however, is a fact already well proved.

At first sight this sort of case may appear of a pretty ordinary kind; but what I claim as their characteristic features are these:—They are cases of obstinate dyspepsia, marked specially by the discomfort of feeling the stomach always distended as with fluid, while the mouth is usually dry, thirst troublesome, and yet drinking always aggravates the discomfort. There is no acute, pain, and the patient rarely gets thin. Movement of the patient produces that splashing noise or "*clapotement*," as Chomel calls it, significant of the presence of wind and water in the stomach; and this can be produced at any time by the patient. Lastly, there is the almost certain amelioration of the symptoms under the influence of the dry plan of diet; and till that is followed no medicine does any good whatever.

It is necessary to seek carefully for any organic disease, for if there be stricture or thickening about the pylorus it will not add much to the doctor's credit to promise acertain cure if the dry plan of diet is followed; relief there may be, but not cure. Chomel says of one of his cases; "J'ai vu un autre individu, atteint de dyspepsie des liquides, qui, en appuyant avec quelque force la main sur l'epigastre, faisait remonter dans sa bouche plusieurs onces d'un liquide aqueux; mais ce fait a été unique pour moi." I have sought to elicit this symptom in the cases recorded, and in others, but have not been able to produce it.

At times fits of fainting, with irregular action of the heart, are prominent symptoms in these cases of dyspepsia of liquids, the cause being due to the state of the stomach. Marked relief is obtained to these symptoms on the dry-diet plan, and this confirms the soundness of the practice of not allowing patients with heart disease to live much on liquids and slops, but rather to feed them on easily digested solids, such as mutton, chicken, boiled fish and game. These are all forms of solid food, suitable in cases of dyspepsia of liquids. Milk does not seem otherwise than beneficial, but much of farinaceous or vegetable ood should be avoided.

The patient must bear a certain amount of thirst as well as he can, and when he does drink must take but very small quantities at a time, and not drink for n hour or more after he has taken his meal of solid food. Weak whisky-and-water, sherry wine, and toast-and-water, are among the least objectionable drinks; and sometimes a small cup of pure beef-tea, free from any farinaceous dmixture will suit well. One of my patients found a wineglass of good stout o agree well, and relieve his thirst.

Compared with diet, medicine is but a secondary matter; but a dose of regory's powder in the morning and a tonic mixture of rhubarb and tincture f nux vomica twice a day, I have found serviceable. Mild saline purgation I ave tried, but its effects are very uncertain, relieving at times slightly, at other mes adding decidedly to the discomfort, and it is not a method of treatment to e recommended. A pill of opium at night, when there is much nervousness nd hypochondriasis, is certainly beneficial, and a pill of bitter extract or of albanum and assafœtida twice a day may be used with advantage.

It is rare for the patients to emaciate, provided the case be uncomplicated. n one of Chomel's cases, however, he noticed a degree of emaciation that aused great anxiety, being an unusual symptom, and likely to indicate organic isceral disease. The dry regimen was enforced, and the lady got quite well, hough any over-indulgence, even in such a fluid as plain water, would be followed by *return, in some* degree, of the unpleasant symptoms.—Lancet, Feb. *7, 1872, p. 215.*

DISEASES OF THE URINARY ORGANS.

41.—TABLE FOR THE EXAMINATION OF URINE.

By Dr. J. Campbell Brown, Lecturer on Chemistry and Toxicology at the Liverpool Royal Infirmary School of Medicine.

I.—Observe the colour and appearance of the urine, whether it is clear or turbid, and whether it contains much mucus.

A high colour may be due to Bile, Blood or Purpurine; a pale colour may indicate excess of Water, and frequently also Glucose.

II.—Observe the reaction to red and blue litmus papers.

Normal urine is slightly acid; if the reaction is alkaline, and the red colour of the paper is restored on drying it, the alkalinity is probably due to ammonium carbonate from the decomposition of urea; confirm by observing whether effervescence occurs on the addition of an acid to the urine.

III.—Observe the specific gravity.

a. If the specific gravity is above 1025, test for glucose by (1.) potash solution and heat; GLUCOSE gives a dark solution. (2.) Add potash and filter, if necessary, then add copper tartrate and more potash until a blue solution is obtained; on heating to the boiling point glucose reduces a red or orange precipitate of Cu_2O.

b. If the specific gravity is high and sugar is not present, add to a portion of the clear urine in a deep watch-glass about one-half its volume of cold concentrated nitric acid; a deposit of hexagonal plates of urea nitrate indicates excess of UREA. (Probably excess of phosphates and other salts will be found accompanying excess of urea.)

c. If the specific gravity is below 1012, this may be due to great dilution of the secretion with WATER, which will be further indicated by the large quantity passed in twenty-four hours; but it is more generally due to disease of the secreting organs, and is accompanied by albumen, the urine being then frequently alkaline, but sometimes acid.

IV.—Heat a portion to the boiling point in a test tube, albumen may be at once coagulated; add nitric acid drop by drop; a flocculent precipitate indicates Albumen; confirm by adding to another portion of the urine acetic acid, filtering to remove mucus, if necessary, and then adding potassium ferrocyanide; a white precipitate indicates Albumen. The deposit from an albuminous urine should be examined microscopically for CASTS, PUS and BLOOD GLOBULES.

Boiling alone may first cause a precipitate of Calcium Phosphate, which will be re-dissolved on the addition of nitric acid. If a turbid urine is rendered clear by boiling, the turbidity is due to urates.

V.—Add to a portion of the urine, ammonia in excess; the white precipitate consists of ALKALINE-EARTHY PHOSPHATES; filter and add ammonium chloride and magnesium sulphate; the white crystalline precipitate indicates the amount of phosphate which was originally present as ALKALINE PHOSPHATES.

VI.—To another portion add ammonia and filter; then add ammonium oxalate; the white precipitate contains the CALCIUM as oxalate.

VII.—To another portion add nitric acid; divide into two parts; to the first add barium chloride; the precipitate contains SULPHURIC ACID as barium sulphate. To the second add silver nitrate; the curdy precipitate contains the CHLORINE as silver chloride.

VIII.—A dark brown or blue colour may be due to Indican which is destroyed by nitric acid.

Any colour from that of Gregory's powder to an olive green tint may be due in part to bile.

(1.) Pour a layer of the urine (concentrated, if necessary,) on to a white dish, and add concentrated nitric acid. A play of colours, green, blue, purple, and red, indicates BILE PIGMENT.

(2.) Boil a portion of the urine with acetic acid, and filter to remove albumen, then add a few crystals of cane sugar, and a few drops of concentrated sulphuric acid; a purple tint indicates the ACIDS OF BILE.

A red colour may be due to blood; in this case heat will have destroyed the colour, and coagulated the albumen of BLOOD. Examine

(1) by the microscope for BLOOD GLOBULES, and

(2) by the spectroscope for HÆMATINE.

A high colour may also be due to purpurine. In this case it is unaltered by heat and by nitric acid. Boil a portion with hydrochloric acid. A dark red or purple colour indicates excess of PURPURINE, of which a small quantity is present in normal urine. Allow to stand for a day; the crystals which slowly form are URIC ACID, an excess of which frequently accompanies purpurine.—*Liverpool Medical and Surgical Reports, Oct.* 1871, *p.* 42.

42.—ON THE TREATMENT OF DIABETES BY LACTIC ACID. (CANTANI'S METHOD).

By Dr. George William Balfour, Physician to the Royal Infirmary, Edinburgh.

[Dr. Balfour at the commencement of his paper gives a *resumé* of the theories of diabetes, and the modes of treatment founded thereon.]

Now, without entering more at large into the pathology of diabetes, it is obvious that these various modes of treating it are all underlain by one common fact—viz., that in diabetes the natural glycogenic function is discharged in a morbid manner, and that this morbid discharge of function may be importantly modified in two modes; 1st, by depriving the organism of that pabulum from which the glucose is mainly derived, and 2nd, by employing such remedies as are capable of altering the nervous energy of the organs at fault; practically it has been hitherto found of importance to conjoin these two methods.

The chemical treatment of diabetes by the attempted burning the sugar by artificially induced hyperoxygenation of the blood, has not been found more successful than the simple dietetic plan without this addition, and for the chemical reason stated this could not be otherwise. While the basis of the other chemical treatment, that by alkalies, has been shown to be erroneous, the blood of diabetic patients being not less alkaline than that of others on the one hand, while on the other, a fluid even more akaline than blood cannot decompose glucose, so that if the alkaline treatment have really proved more successful than the simple dietetic plan, that must have depended on its exercising some modifying power over the function at fault—a modifying power, however, of so doubtful a character that it has not been able to prevent this method of treatment from falling, in this country at least, into comparative desuetude.

It is a matter of daily observation, that morbid alterations of function are frequently only to be permanently remedied by a restoration of the body to its normal standard, the great difficulty of promoting a cure in such cases being the impossiblity of giving perfect rest to one organ while endeavouring to build up the frame, the skill of the physician being shown by the manner in which he solves this complicated problem, giving as much rest as possible to the organ at fault, while at the same time improving the general nutrition, and thus restoring the healthy tone to the constitution. It is in this respect that all the treatments hitherto propounded for diabetes have failed; they may have given rest to the organ at fault, but they have too often depressed the general health, and, while giving temporary relief, have possibly in many cases hastened the end; and even when a tonic treatment has been mainly relied on it has failed for want of physiological adaptation. It is in this respect that Cantani's treatment is pre-eminently useful; it relieves without depressing, it gives as nearly as possible perfect rest to the organ at fault, and at the same time builds up the body by an artificial adaptation of physiological means, and restores the healthy tone to the constitution, thus enabling it to return to healthy action. It is by no means put forth as a perfect cure for all cases of diabetes; it is a means of relief to all, but only a cure for some; yet it seems to act curatively in a larger proportion of cases than any other mode of treatment hitherto devised, while the relief to all is more perfect.

Professor Alnaldo Cantani, of Naples, agrees so far with Reynoso in believing that in diabetes the question is not so much one of increased production of sugar as of defective combustion; this he conceives to be proved by the small number of respirations made by diabetic patients and by their average low temperature, ranging, according to him, from 95° to 96°8 F. in uncomplicated cases; the temperature, of course, rising with any complication—such as tuberculosis—giving rise to pyrexia, but always remaining below the normal of such pyrexia. This defective combustion he believes to depend upon the production of a morbid form of glucose, which he has termed *paraglucose*; this is incapable of being transformed into lactic acid, and therefore cannot be burned; it is consequently passed unchanged in the urine. The result of this is, that the sugar and starch of the food being transformed into this incombustible paraglucose, the heat of the body is in so far maintained at the expense of the albuminates and fats, and from the combustion of the former we have that excess of urea which adds so greatly to the density in many cases of diabetes, while, as the albuminates and fats which the patient receives as food are insufficient for his requirements, those of his own tissues are also employed, and hence his rapid emaciation.

In the early stages of diabetes the quantity of sugar passed in the urine oscillates with the diet, and with an exclusively animal diet is much lessened in twenty-four hours, and may entirely disappear in a few days; in the later stages, when sugar persists in the urine even after the withdrawal of all amylaceous food, Cantani believes that not only the inosite of the muscles may be transformed into paraglucose, but also that the gelatinous tissues, which Baedeker succeeded in transforming into sugar apart from the system, may in this anormal pathological condition become transformed into paraglucose, and thus account for the persistence of sugar in the urine of those fed exclusively on an animal diet apart altogether from the question of the albuminates themselves being transformed into diabetic sugar. Thus, though all diabetics are to a certain extent autophagic, we may yet have this abnormality developed to excess, and may thus have three steps in this disease: first, that in which the sugar oscillates with the quantity of amylaceous food supplied; second, that in which the inosite and gelatinous matter of the animal food are transformed into diabetic sugar; and, third, when not only these but also the inosite and gelatinous matters of the body of the patient himself are so transformed—a most severe and hopeless form of autophagia. Cantani, with most modern pathologists, recognises the liver as the organ mainly at fault in these cases; but whatever the organ may be, he proposes to give it as complete a rest as possible, by depriving it of its pabulum in subjecting the patient to a rigorous meat diet, thus reducing to a minimum the introduction into the system of the sugar-producing substances. But as that is only a temporary expedient, having reference to only one element of this disease, and incapable of arresting the waste and ultimate complete degradation of the body, he further proposes to prevent this waste by supplying a combustible agent in a quantity sufficient for the wants of the body, so that the fats and albuminates may continue to be stored, and the body thus gradually restored to its normal standard; and he hopes that this restoration of the healthy standard of the constitution, coupled with the prolonged functional rest to the organ affected, may suffice to prevent any relapse into its morbid condition even after a return to the ordinary dietetic conditions of modern civilized life. The result of Cantani's experiments have to a large extent proved the correctness of his views. The combustible agent which Cantani has selected is lactic acid, and it has been wisely chosen, inasmuch as it is in all probability that agent employed in the normal conditions of nutrition, representing as it does the intermediate stage between glucose and carbonic acid, so that not only is a complete rest given to the organ at fault, but that very agent is supplied to the system which would have been normally present had the organ at fault been doing its duty after a normal fashion, so that while as complete a relief as possible is afforded to the organ at fault, there is in no respect any anormal strain put upon the system. The organ at fault is for the time being merely thrown out of gear, but all the other functions proceed in a natural manner, so that, when the normal tone of the organ has been re-acquired, it may again be replaced in the natural cycle without the harmony of the natural sequence of the functions being in any way interrupted either by the cessation or the restoration of its function. The quantity of lactic acid which Cantani administers is from 77 to 154 grains daily, diluted in from 8 to 10 fluid ounces of water, and taken during the day. His exclusively meat diet means rigorously one of plain meat, roast or boiled, without any sauces of milk or eggs, and certainly without any bread, flour, or any vegetable matter whatever, the only seasoning permitted being salt, oil, and a little vinegar. For drink he allows water, either plain or with a little of the purest alcohol; coffee, tea, and wine being prohibited. His results have been somewhat surprising. In recent cases the cure is stated to be almost certain and speedy; and even where an exclusively meat diet is not persisted in, life is apparently prolonged, and many of the unfavourable results of diabetes are prevented, though the mellituria is not arrested. Dr. Sammut of Naples, from whose report I quote, states that he has seen an army lieutenant enter the hospital emaciated, weak, and impotent, with polyuria, thirst, and extraordinary hunger. In twelve days the last trace of sugar had disappeared from the urine, and in six weeks the patient had gained nine pounds. He left the hospital in excellent condition—florid, strong, without thirst or hunger; he continued the treatment for two months more at home, and then resumed mixed diet, and after the lapse of eight months he was in excellent health, and weighed twenty-one pounds more than when he entered hospital, being also without a trace of sugar in the urine, though eating indiscriminately of all that came before him. A tailor from Naples was admitted in a desperate condition. He left florid and robust. Since then, for more than seven months, he has been eating promiscuously, and is more fat, rubicund, vigorous, and energetic than before his attack. Dr. Sammut also states that he saw in Cantani's clinic several other diabetics—he mentions five—all much improved, and several believing themselves to be perfectly cured.

These results are certainly very remarkable, and being a continuous series following the employment of a treatment based on rational considerations, are sufficient to claim the attention of every medical man interested in the progress of therapeutics. And I may remark that the latest treatment of diabetes propounded in this country—that by skim-milk—bears out Cantani's views in so far that it is a strictly animal diet, free from amylaceous matter, and containing three to six per cent. of lactin, which, under the influence of the caseous matter, becomes transformed into lactic acid. This treatment is therefore an approach, but a very meagre one, to Cantani's apparently more perfect system; and I may add that the results of the two systems in my own practice have fully convinced me of the greater applicability and more perfect success, so far as time allows me to judge, of Cantani's method.

The first case in which I employed it was not a favourable one. The patient had been formerly much benefited by the conjunction of opium in large doses with the dietetic treatment. He returned two years afterwards with commencing phthisis, and though benefited somewhat by the lactic acid treatment, yet he died, as was naturally to be expected. His urine fell in five days from an average of about 250 ounces to 70 ounces in the day, the specific gravity remaining about the same—1.030; this high specific gravity being apparently due to the amount of urea and uric acid present, a condition of matters usual in this treatment. The deposit of red oxide of copper following the application of Trommer's, which had been copious, ceased entirely, and was replaced by a brownish discoloration due to the excess of uric acid present. No change took place on boiling with liquor potassæ (Moore's test). The poor man therefore lost the main symptom of his disease, but he died exhausted after about three months' treatment.

Case 2.—David Hume, an engineer, aged 53, was admitted into Ward V. on July 14, 1871, *complaining* of great thirst, debility, and polyuria. Three years ago he suffered from a stroke of paralysis which affected the right arm and le He was able to resume work three months subsequently, but still complains weakness in the right leg and dulness of hearing in the right ear. His inte ments are rather dry, his joints normal, but his limbs are much emaciated, he sleeps badly at night. Sensation and intelligence normal, but motion m impaired from his great exhaustion, having had to rest frequently in co upstairs to the ward. He measures 5 ft. 7 in., and weighs only 8 st. 8 lb His tongue is dry in the centre, moist and clammy at the tip and edges, red, and glazed. His mouth and fauces dry. He has a constant bad taste i his mouth. His teeth are decayed. His digestion good, but his bowels usually constipated. His arteries are atheromatous, but the circulatory and respiratory systems otherwise normal. He calculates his total drink at 100 ounces daily, but he passes between 300 and 400 ounces of a pale-coloured urine daily, spec. grav. 1·030, and showing a large amount of sugar by the ordinary tests. He states that he has been under medical treatment for upwards of a year, but has nevertheless been gradually getting worse, and he dates the commencement of this illness betwen three and four years ago. Up to the 15th of August he received only ordinary diet and no special treatment, but this day he was placed upon a strict meat diet, consisting of 2 lbs. of boiled meat and 2 lbs. of steak in the twenty-four hours, which was the quantity he found to be sufficient. He was also ordered one teaspoonful of lactic acid in a tumblerful of water, to be taken in quantities sufficient to assuage his thirst. On the 29th of August his urine still contained sugar, was reduced, however, to 86 oz. in the twenty-four hours, his total drink being now stated to be 78 oz. This day he was ordered a bottle of diabetic koumiss in the day. Of this he, however, only drank about half a bottle daily. On Sept. 1—his total drink being 48 oz.—he passed 48 oz. of urine rather deeper in colour, of spec. grav. 1·025, and containing no sugar. On Oct. 1, the patient, having continued to improve uninterruptedly, was permitted to add to his diet four so-called bran biscuits, which contain, however, a large amount of starch, and a cabbage. He takes his acid regularly. His total drink is now 36 oz., his total urine passed 40 oz., spec. grav. 1·025, and no trace of sugar. On Oct. 20, two additional bran biscuits were granted to him; and on Oct. 25 he was permitted to breakfast on porridge and skim-milk, the bran biscuits being stopped, and his meat reduced to 2 lbs. daily, which is found sufficient. His weight is now 10 st. 7 lbs., and his urine keeps free from sugar. He was now put under surgical treatment for a stricture of the urethra, the lactic acid being continued, and bran biscuits gradually added to his porridge and skim-milk and meat diet, till he took six biscuits in the day. His drink and urine both averaged about 40 oz. in the twenty-four hours, the urine keeping free from sugar. On Nov. 14, his weight was 11 st. in his drawers alone, and, feeling quite well, he was discharged. He was shown at the Medico Chirurgical Society on the evening of Nov. 15, and has orders to report himself occasionally, so that his further progress may be ascertained.

Case 3.—Mary A. Early, a mill-worker, aged 15, admitted into Ward XIII., July 18, 1871, complaining of weakness, general malaise, polyuria, and diarrhœa, states that her present illness dates only from four weeks ago, when she was seized with headache, which lasted all day, nausea, great thirst, and diarrhœa. Her appetite being unimpaired, her previous health was good, and her family history unimportant. On admission her skin was hot and dry, limbs much emaciated, height 4 ft. 8¾ in., weight 4 st. 11 lbs. Complains of head-ache and great thirst. Her tongue is covered with a slight brown fur, moderately moist. Her bowels loose, several stools being passed daily. Her uri is pale, spec. grav. 1·030, and contains a large amount of sugar; its quantity much increased, but cannot be measured on account of the diarrhœa prese Other symptoms normal. The diarrhœa was treated with chalk and opiu mixture, and ceased in a few days. Her drink was now found to vary fro 100 to 170 oz. daily, and the quantity of urine passed from 120 to 180 oz. C July 27, she weighed 4 st. 7 lbs., and was placed upon an *ad libitum* allowan of meat, of which she managed to consume about 2 lbs. daily, no extras. S had a bottle of soda-water daily, and two tea-spoonfuls of lactic acid in a tu blerful of water as often as desired. The very next day her total drink sa to 32 oz., and her total urine passed to 22 oz. daily; and on July 29 all trace of sugar ceased to be discoverable, spec. grav. of urine remaining at 1·030, but containing a large quantity of uric acid and crystals of oxalate of lime; total quantity passed 20 oz., total drink 40 oz., in the twenty-four hours. After this she continued to drink with almost unvarying regularity 40 oz. in the day, passing from 28 to 40 oz. of urine, averaging 33·5 daily for the three months, August, September, and October. On Aug. 7, she weighed 4 st. 6 lb., having lost 1 lb. since being put upon her restricted diet, and on the 12th her weight remained the same. She says she feels quite well, but is unable to walk much or even stand long from a feeling of weakness across her loins. Aug. 19. Patient weighs now 4 st. 7 lbs., spec. grav. of urine 1·030, no sugar. On Aug. 12, she was offered cabbage or green vegetables, which she declined, and was then allowed to get half a slice of toast in the day; but this was stopped on the 20th, on account of a slight trace of sugar which was found in the urine on that day. On Aug. 25, the patient weighed 4 st. 8½ lbs., and was permitted to have some diabetic koumiss for drink. She did not like it, however, and only managed to drink three bottles of it between this date and Oct. 3, when it was stopped. On Sept. 17, patient weighed 5 st., and sugar having remained absent from the urine from Aug. 12, she was allowed to have half a slice of brown bread daily. On Sept. 30, patient weighed 5 st. 5 lb., a marked quantity of sugar was found in her urine, but as it was distinctly traced to her having eaten a slice of white bread and jelly which she had got from another of the patients, she was permitted to continue the brown bread. Oct. 9. Weighed 5 st. 7 lbs. Brown bread stopped on account of slight discoloration of the urine when boiled with liquor potassæ. Oct. 14. Patient weighs 5 st. 9 lbs., and on 14th Nov. 5 st. 10 lbs., her height being 4 ft. 8¾ in. Oct. 17. The urine keeping free of sugar, the patient was allowed to have four bran biscuits in the day, which contain a notable quantity of starch; and on the 27th these were stopped, and porridge and skim-milk ordered for breakfast. About the end of a week, however, a trace of sugar reappearing in the urine, the porridge was stopped, and for another week she was relegated to her meat diet, of which she takes about two pounds daily. After doing penance for one week she was again permitted to have her four bran biscuits, which she has continued to this date. Nov. 16. Her urine keeping free from sugar, 36 oz. in quantity, and spec. grav. 1·030. Obviously this girl is far from being in the same satisfactory condition as Hume, but, considering her youth, and the well-known obstinacy of diabetes in youthful patients, I think there is every reason to be thankful for the improvement she has made, and also strong grounds for hope that a more prolonged treatment will ultimately result in her restoration to health.

The lactic acid I have employed has been that obtained from the druggists, fluid, not syrupy, of a spec. grav. of 1·027, and with the ordinary musty smell of sour milk. Three to four drachms in the day seem to be quite sufficient for all practical purposes; yet much more may be taken without detriment, cases 2 and 3 occasionally taking as much as eight or nine fluid drachms in the day. The remedy is, however, as yet rather too expensive to be employed in larger doses than are absolutely necessary, as it costs ten shillings a pound; a more extended application will, of course, speedily cheapen its production.

The great advantage of this treatment is the prospect it holds out of a comparatively speedy restoration to ordinary mixed diet, with persisting absence of sugar from the urine—of rapidity of cure in short; and this rapidity is in itself a great encouragement to such patients steadily to persevere, in spite of the irksomeness of a diet rigidly restricted to meat, which seems certainly to be required for two or three weeks at least, after which, fish, fowl, and green vegetables may be added, and other relaxations gradually and cautiously permitted, provided the case progresses favourably. I fancied that the koumiss was of considerable benefit in Hume's case, and if its composition be correctly stated, it ought to be a useful diet for diabetics, but a rigid analysis is still requisite. It is prepared by Chapman of Duke Street, London, and is said to consist of milk, fermented in a warm place by the addition of a small quantity of very sour milk; it effervesces slightly, and is pleasant to the taste, being apparently composed of the caseous matter in a state of extremely fine division, lactic acid, and a certain amount of alcohol, besides the fatty matter and salts ordinarily present in milk. The only apparent drawback to it is the statement that in weather it may act as a diuretic—an action which is certainly not requir diabetes.

Bonthron's diabetic biscuits are perfectly palatable, and I fancied they prove a welcome addition to the very limited dietary permissible under tani's treatment. They are said to be prepared from finely-ground bran, w as free from starch as possible, and mixed with albumen and butter. I pro some of these biscuits, and had them analyzed by Dr. Stevenson Macada Edinburgh; and the analysis shows them to be wholly inadmissible i early stages of this treatment, while, after a certain stage has been reached, are not required, the ordinary bran biscuits of the shops, which are much palatable, being perfectly sufficient, and forming an admirable introducti more mixed dietary.—*Edinburgh Medical Journal*, *Dec.* 1871, *p.* 533.

SURGERY.

UTATIONS, FRACTURES, DISLOCATIONS, AND DISEASES OF BONES, JOINTS, &c.

ON THE USE OF THE BAVARIAN APPARATUS IN THE TREATMENT OF FRACTURES.

By Dr. Anthony H. Corley, Surgeon to Jervis Street Hospital, Dublin.

method of treating fractures was described at the time of the Franco- n war. (See *Retrospect*, vol. lxii, p. 172, also vol. lxiv, p. 145.]

appliances are few and inexpensive. A yard of the cheapest flannel, a or so of plaster of Paris, a few large pins with their heads bent, and a f calico or common roller make the surgeon independent of the surgical- ment maker. The flannel is cut into two rectangular pieces the length of ctured bone, and broad enough to encircle the limb and to leave a margin. a little wider than the other. Placing the narrow one evenly over the they are sewn together by a longitudinal stitching down the mesial line. now resemble two sheets of note paper stitched together at the fold, the a little larger than the inner.

ing the fractured limb carefully the flannel is to be spread smoothly it, taking care that the line of sewing corresponds to the posterior mesial the limb. The two ends of the inner sheet are now brought evenly over b and fastened together by means of the bent pins, leaving the two outer eets spread on the surface of the bed. By extension, counter-extension, anipulation, exact coaptation is secured, and now the plaster, mixed to a consistence with water, is partly smeared and partly poured on. The ter sheets of flannel are rapidly brought over the surface of the plaster is now caught on both sides between the inner and outer layers) and are gether at their margins till the plaster sets, taking care that the extension unter-extension are kept steadily up during that period. The pins must taken out (and it is for this purpose that their heads are bent), the edges d, a few turns of the roller, or this kind of bandage (the use of which I arnt from my senior colleague, Dr. Stapleton) now being applied, and the operation, which does not occupy ten minutes, is finished. Muscular at once ceases, the patient acquires the power of shifting his position, steady equable lateral pressure is secured, adapted exactly to the shape qualities of the limb.

ld it be necessary to maintain any peculiar position, as in the treatment 's and Colles's fracture, the assistant holds the limb while the plaster is , and a firm rigid case is at once secured, exercising uniform pressure, ble of slipping, and maintaining the fractured ends in steady apposition.

e most important advantage connected with this apparatus is the facility hich it may be taken off. Most plaster and starch bandages are not only some to apply, but so difficult to remove, that there is a strong natural cy on the part of the surgeon to leave them on, and trust that the bones ting properly. I believe I am not the only surgeon who has seen, when dages are cut, certain peculiar angles and curves in the limb that Nature intended to be there. Again, there are certain fractures, as compound tc., in treating which we must keep a portion of the limb in such a way as to be readily examined. In the Bavarian apparatus, when the bandages are loosened, the two opposite sides can be separated like the bent covers of a book, the line of stitching (which prevents the running together of the plaster) acting like the back or hinge of the book, and allowing the limb to be exposed or to be taken out if necessary. If the swelling, usually present at first, diminish very much, the anterior edges can be trimmed so as to allow tightening, or the whole case may be taken off and lined with cotton. I have used it *immediately* after accidents, and the only cases where I should defer its application are those of fracture from direct violence where there is such contusion that even the most equable pressure would be contra-indicated. I have used it with success in fractured femurs, tibia, fibula, humerus, radius, ulna, as well as with Pott's and Colles's fractures, and have seen it used equally successfully by my colleagues, Drs. Stapleton and Walshe. When I say that Dr. Stapleton stamps it with his approval, I may at once claim attention for it. It is, however, with special reference to its efficiency in the treatment of Pott's fracture, Colles's fracture, and fracture of the shafts of the femur, that I now wish to speak. Everyone knows the difficulty of treating the first of these very common accidents. I have tried Dupuytren's splint applied with care by myself, and have had the pleasure in a day or two of finding the splint slipped round to the back of the limb, a circumstance highly calculated to allow the fracture (to use the time-honoured witticism) to turn out remarkably well. I have used the same splint, placing the limb after its application in a box, and I have used the box alone, trusting to padding to keep the proper position. I have secured *tolerably* favourable results, but only by means of constant trouble, frequent re-adjustment, and exemplary patience. When the Bavarian apparatus is applied there is no further interference necessary, the patient can at once lie on his side, and get up to sit in a chair in two or three days.

With Colles's fracture I have since my attention was directed to Gordon's splint by Mr. Porter's paper, used none other until recently, and am perfectly satisfied with its superiority over every method of treatment before adopted. I have, however, tried how the plaster apparatus would suit, and have every reason to be pleased with the result. In applying it in this fracture I place a pad of cotton over the dorsal prominence, and hold the hand strongly adducted till the plaster is set. In the two cases of fractured femur which I have treated in the last six months I have secured such admirable results, and with such comfort to the patient, that I shall dwell at some length on the accessories to its successful use. I have always been under the impression that if in this fracture the ends of the bone could be got fairly into apposition and *kept together* by equable lateral pressure, there would not be the great necessity for extension and counter-extension now presumed to exist. The long splint, which enforces a constant recumbent attitude, and the use of a perineal band, has been felt to be such an imperfect mode of treatment, that dozens of modifications of it have been proposed, of which a few trials have convinced surgeons that the proper apparatus is still a desideratum. Counter-extension by the perineal band is particularly objectionable from its pressure, from the irritation or even the ulceration so likely to be produced, especially in children, and from its tendency in fractures high up to increase the deformity.

With the plaster apparatus the necessary lateral pressure is produced, and the only requisite afterwards is some means by which slight extension and counter-extension can be kept up, less I believe for the purpose of producing any effect at the fracture than for preventing the motions of the patient's body (which I freely allow) from disturbing the position of the fractured ends. Having adjusted the apparatus I apply two long strips of soap plaster to the opposite sides of the leg, uniting them in a loop below the foot; to this loop I fasten a cord, which passes over the foot-rail of the bed, and has a weight dependent. This is the extension. I then raise the lower end of the mattress so that the feet are considerably higher than the pelvis, and the body forms the counter-extension. After the second day I found in both my cases that the patient could sit up to eat his dinner, &c., without pain increasing, or *disturbing* his limb in the slightest degree.

My second case is still under treatment, and through the courtesy of my *colleague*, Dr. Walshe, I am able to mention that there is at present in Jervis-

Street Hospital a child, æt. 4, both of whose thighs and right humerus were fractured by being caught in the wheel of a car. All the broken bones were at once put up with the plaster apparatus. Immediately afterwards the little patient seemed quite at ease, and free from suffering, and has since continued to progress. The efficiency of the plaster in this case, and the difficulty which the application of two long splints would present, speak volumes.

The cases which I have noted (some twenty in number) may seem too few to give me such confidence in the apparatus, but I trust, for the reasons I mentioned, the society will excuse my bringing this mode of treatment forward, and that my explanation and advocacy may secure for it a more complete and satisfactory trial.

[The following discussion took place when this paper was read before the Surgical Society of Ireland.]

The President (Mr. Wharton) said he had listened to the paper with great pleasure, and he was sure they all felt greatly obliged to Mr. Corley for bringing forward this mode of treating fractures. He had seen his colleague, Mr. White, use it. That morning he had had an opportunity of seeing such a case as Mr. Corley had exhibited the apparatus for. Nothing could be more expeditiously or efficiently done. It only took about five minutes to put up a fracture of the tibia. Mr. White had a stiff case for the patient's leg almost immediately, to the man's great comfort and relief. He had also seen Mr. White use a modified many-tailed bandage, which he called by Mr. Stapleton's name. All present who knew the great difficulty of getting a good bandage would appreciate it.

Mr. Stapleton said he had seen several cases treated with this apparatus by Mr. Corley, and he must certainly say that in Pott's fracture he considered it one of the greatest blessings to the patient, and one of the greatest improvements in surgery. He himself had for a long time discarded Dupuytren's splint. At the time it was brought before the society he combated the great advantages that were claimed for it. He (Mr. Stapleton) used a box splint with pads. If they put on Dupuytren's splint in the original way it would not do. It had been nicked and notched by surgeons to try to retain it in its place, but notwithstanding it still slipped. But he had never seen such advantage as was given by this apparatus in Pott's dislocation. As to the many-tailed bandage he was first led to use it in fractures of the thigh. In fractures of that bone, put up in Liston's long splint, with the bandage round the foot and up the thigh, if you wanted to see what was going on you had to unroll the whole bandage. Any one who had had a number of fractured thighs under his care at one time would know how laborious this was. He had had eleven fractured thighs under his care at one time, and he then took this plan of using the old many-tailed bandage. If one part was loose it could be tightened, or all could be loosened to look at the fracture. In treating fracture of the thigh he used three splints. He never disturbed the leg, but simply undid the bandage, and saw what was going on. It might be said, "Oh, put on a starch bandage," but he had seen some very ugly accidents from leaving starch bandages on too long. Sometimes when a surgeon thought that he was going to have a nice limb, and left it in the bandage for six weeks, he found a little more angular deformity than he wished.

Mr. Mapother said since Mr. Corley mentioned the use of this apparatus to him he had tried it in club-foot, and in two cases it had been fairly successful. In another, on account of the extremely small size of the foot, it was not easily applied, but in two cases where the children were three or four years old it suited admirably. After division of the tendo Achillis, he placed the foot in proper position, and then with this admirable apparatus he had completed extension, and the result was satisfactory. In another case he applied a roller covered with plaster-of-Paris, and covered with an ordinary bandage. Mr. Corley had called this the Bavarian apparatus. He was sure he had forgotten to state, as he did when he first mentioned the subject, that it was introduced to this kingdom by Asst.-Surgeon Moffett, a pupil of the College in 1855, and a licentiate. He had noticed it in use during the Franco-Prussian war, and had described it in his book on "Ambulance work."

Mr. Stapleton observed that this was not what was called the plaster bandage. What was used in Berlin, where he had seen it used, was a thin material, which was dusted very thickly with plaster-of-Paris. It was then soaked in water, and rolled round the limb. The material used was very open, like what they in this country called a lint bandage.

Mr. Fleming said the apparatus which had been just described by Mr. Stapleton was the one which had been used here for a long time. He used it himself in the Richmond hospital, and if he was not mistaken, it was a German who introduced it to this country. He believed he was a Professor in one of the principal schools, but he forgot his name. It consisted of a gauzy bandage of very open work, and was dusted over, he believed, with a preparation of plaster. It was then damped and applied to the limb, and plaster was rapidly applied with the hand to the outside. It quickly formed a tolerably solid cast, but it took a long time to dry satisfactorily. Some of his cases turned out well, but in some of them there was considerable deformity. He thought they were much indebted to Mr. Corley for bringing this new apparatus before the Society. In the first place it was capable of extremely easy and rapid application; in the next it was of great advantage to the surgeon, because it enabled him to watch carefully the progress of the case; and if, as it did happen in injuries such as those of the ankle-joint, there was effusion in twenty-four hours or so after, it was of great advantage that they should have the opportunity of ascertaining whether such a condition existed, and of applying the necessary remedies. He thought hospital surgeons should be very glad to have the opportunity of putting this apparatus into use as often as they had the opportunity.

Mr. Richardson observed that many years ago M. Seutin, owing to the accidents likely to occur under the use of the immovable starch apparatus such as excoriations and gangrene, substituted for it the starched apparatus having the properties of movability and immovability, and which he named the *Amovo-Inamovible* apparatus, the principle being identical with that just illustrated by Mr. Corley. In M. Seutin's starch apparatus the movability is imparted to it by dividing the apparatus, when dry, its whole length in front, and after some time should the limb shrink, he removes some of the edges of the apparatus to enable it to fit close to the limb and give it firm support. This apparatus has been compared by Goffre in his "*Précis Iconographique de Bandagès, Pansements et Appareils*," to a bi-valve shell. Indeed, in Goffre's book the bi-valve term is also applied to the plaster apparatus constructed on this principle.

Mr. Stapleton said that an apparatus had to be used to cut it.

Mr. Richardson said that was another matter. Seutin had established the bi-valve principle, and this name is applied to the *Amovo-Inamovible* starch, and even to the plaster apparatus in Goffre's work. He (Mr. Richardson) had often made Seutin's *Amovo-Inamovible* apparatus, and found that after it had been removed and re-applied two or three times, the starch cracked along the middle line posteriorly, and allowed of its easy removal and re-application.

The President wished to know how Mr. Corley put up Colles's fracture in this apparatus, because one of its advantages was its universal applicability.

Mr. Corley, in reply, said that with reference to Dr. Mapother's observation he had stated in his previous communication that he did not claim the slightest originality in the matter. He got a description of the apparatus in one of the Army Medical Reports. It struck him that it would be a most convenient apparatus, and having tried it he found that it exceeded his most sanguine expectations. The history given by Mr. Moffett, who was a resident pupil at Jervis-street Hospital at the same time as Mr. Walsh, was in a communication made by him while at Netley, and he mentioned that it was communicated to him by Dr. Streidlinger, as the apparatus used in the Bavarian ambulances, but it was intended rather as a temporary than, as he understood Mr. Moffett's paper, a permanent arrangement. He (Mr. Corley) found on trial that there were few fractures to which it was not applicable. As to Mr. Richardson's observations about the bi-valve, he had only to say that in this case the line of stitching prevented the plaster on each side from meeting, so that a hinge was left like the back of a book.

Mr. Richardson observed that the term bi-valve had been long ago applied in French books to this apparatus.

Mr. Corley continued to say that the starch bandage took two or three days to harden. He remembered, as a student, attending an hospital where the "succession duty" plan was carried on by the surgeons, and he saw a fracture put up on one of the last couple of days of the month. It was not held in position, and the in-coming surgeon, did not take the same interest in the case, and he remembered that the limb presented a very remarkable elbow on the external side. It impressed him very much with the importance of keeping fractures in position while the starch was hardening. That would take three days, while the apparatus, if the plaster was good, would be hard in three or four minutes. With regard to Colles's fracture, he held it securely in his hand in the ordinary way, and put a pad over the dorsal prominence, just where the splint recommended by Dr. Smith pressed. While so holding the fracture he grasped the outer layers, so as to press, and to secure the same object at the lower end, that was secured by the keel in Gordon's splint. The fracture in which he tried it was doing well. It had been put up in Gordon's splint. The other was put up in the ordinary pistol splint, and was giving the patient great pain and uneasiness. He put the fracture up in this apparatus, and the man was able to get up next day, and never had the least pain afterwards.—*Medical Press and Circular, Feb.* 14, 1872, *p.* 136.

44.—FRACTURE OF THIGH-BONE, AND ITS TREATMENT WITH THE AMERICAN APPARATUS.

By B. Wills Richardson, Esq., Surgeon to the Adelaide Hospital, Dublin.

Of the numerous appliances that have been invented for the treatment of fractured thigh-bone there are few more capable of producing good results, with little trouble to the surgeon, than the one known to European practitioners by the name of the "American" apparatus. When it is used the thigh need not be bandaged—a most important matter in oblique fracture with overlapping fragments, for many of the muscles of the thigh are so disposed that the constriction exercised by the bandage becomes itself a cause of shortening. Indeed, the muscles of the internal and posterior part of the thigh have been compared to cords extended between their osseous attachments. The roller necessarily presses these muscle cords towards the femur, and in this way, by acting antagonistic to the extending and counter-extending forces, tends to approximate their origins and insertions, and to draw the lower fragment of the femur towards the pelvis.

It is remarkable that Nélaton, the countryman of Desault and Boyer, whose splints have contributed to their fame, prefers the American apparatus to the splints of those celebrated men, as well as to every other apparatus known to him, for the treatment of oblique fracture of the shaft of the femur.

The mechanism for carrying out the American method of treating fracture of the shaft of the thigh bone varies a good deal, as will be seen in Hamilton's work on "Fractures and Dislocations." I have found that Dr. Hartshorne's apparatus, with some modifications of my own, thoroughly counteracts the tendency to overlapping of the fragments. Its cost, moreover, is very trifling.

Dr. Hartshorne uses two splints, and dispenses with the perineal band, which I retain.

In addition to the transverse bar which carries the extending screw, his apparatus has a second bar that is connected with a foot-piece to which the foot is secured. Dr. Chapin uses two separate extending screws; but one screw with a double hook acts better. His apparatus is then constituted as follows:—

1. An external splint (pine wood) of sufficient length to extend from the axilla to some inches below the foot. Its width should be such that the holes for the ends of the perineal band may not be too near one another. The holes should also be only a short distance from the upper end of the splint that the anterior portion of the perineal band may not press the groin, and favour swelling of the limb. The splint is secured to the trunk by means of a couple

of saddle girth bands, each provided with leather straps and buckles, and to the short splint by three leather straps with buckles, as well as by the transverse bar. The upper end of the splint should have a cushioned cap.

2. A short internal splint (pine wood) to extend from the perineum to the same distance below the foot as the external splint. Its upper end should likewise have a cushioned cap. Some of the American apparatuses have no inside splint, in which case the transverse bar should be immovably fixed to the outer splint. I use, however, the inner splint, because it prevents the long one from shifting its position, and thus assists in rendering the apparatus as firm and as steady as a box splint.

3. A birch-wood rectangular bar of the following dimensions:—Length, 12 inches; width from before backwards, 2 inches; vertical depth, 1½ inches. From four to six holes are drilled in it at a distance from one another a little more than the width of the substance of the splints. They are for birch-wood pegs, which secure the parallelism of the lateral splints, and maintain them at the requisite distance from one another. Although not represented of this proportion in the illustration the bar should have the width from before backwards I have mentioned, to enable the male extending screw to turn steadily in it. This screw, including its ring-handle should be at least 14 inches long.

4. A female screw (iron) which is securely fixed in the transverse bar.

5. In the apparatus I used first there was but one swivel hook attached to the extending screw. Two hooks, either soldered or riveted to a single plate, as seen in the figure, are preferable. Each of these hooks should be separated from one another by a space a little greater than the width of the ankle at the malleoli. The hooks form one piece with a narrow transverse iron plate, from the ends of which they rise, and which is secured to the extending screw by means of a swivel joint. It will be seen by the illustration that each side of the adhesive plaster loop diverges from the malleolus to its corresponding hook; therefore, when the extending screw is acting, the sides of the loop are prevented from unduly pressing the skin over the malleoli. On the other hand, should but one hook be used, the sides of the loop must converge to it, so that the greater the extension made with the screw, the greater the compression of the soft parts covering those processes, and the greater the risk of sloughing. To prevent this, wadding must be freely used under the strapping and a piece of wood broader than the sole of the foot secured between the sides of the loop. The double hook obviates the necessity of these precautions, with the exception of the use of wadding. This, however, is not to be used to an amount that would render the area of the adhesion of the strapping to the leg not sufficiently extensive to enable the strapping to resist the strain to which it is submitted by the action of the extending screw.

6. It is scarcely necessary to observe that a properly covered and stuffed perineal band, suitable pads, wadding, and scored splint form part of the apparatus.

Mode of applying the Apparatus.—Although the method of applying the adhesive plaster strapping and loop to the leg is described in Hamilton's, and in many other works which treat of fractures, I will, nevertheless, explain it here for the convenience of the junior reader.

The limb having being sponged with vinegar diluted with water and dried, the hollows of the leg are filled with wadding. Five superimposed straps of soap plaster, having a width of two inches, and sufficient length to form an eight-inch loop below the sole of the foot, are then applied to its sides. The ends of these straps should be placed at corresponding points a little above the knee at each side. The straps are secured to the leg with straps of soap plaster, according to the ordinary way of strapping.

The leg thus prepared, and the perineal band placed *in situ*, the saddle-girth straps are laid under the body, and the leather ones under the fractured limb, with the intervention of a pad long enough to extend from the buttock to the heel. The lateral splints are now applied, either wadding or pads being interposed between them and the soft parts. The transverse bar is at the same time secured by the pegs in the square openings made for it in the splints. The latter should pass a sufficient distance beyond the sole of the foot that the extending screw may be sc [illegible] ome to its ring handle before it can act upon loop. If this precaution be not taken, longer splints may subsequently be uired, unless the plaster loop can be sufficiently shortened to enable the ex- ding screw to act upon the limb. In hospital practice having to change the st applied splints for longer ones would be of little moment, but in private actice such evidence of miscalculation had better be avoided.

The ends of the perineal band when passed through the holes at the axillary d of the long splint are then tied. All the straps are next buckled.

When necessary, a scored splint and an underlying pad are to be secured the front of the thigh by a couple of straps.

Unless the case be such as to require previous co-aptation of the fragments, is may be left to the influence of the extending screw, the hooks of which are ssed into the plaster loop, the screw turned, and extension gradually effected.

A few turns of the screw morning and evening, for a couple of days, cause the tient but little annoyance, the muscles appearing to offer less resistance when us overcome than by a more rapid extension.

The following advantages may be claimed for this modified American paratus:—

1. Facility of application.
2. Limb can be accurately measured without being disturbed.
3. Traction is made in the natural axis of the limb.
4. There being no bandages to become loose, it rarely requires re-adjustment ter the first few days. It is, therefore, admirably suited for provincial prac- e, when, as too often happens, long distances have to be travelled, and visits short intervals may be impossible.
5. Whenever the fragments are capable of being united without shortening, is thoroughly efficient in producing this result. Speaking of the American paratus, Nélaton says that it has over Desault's, Boyer's, Bonnet's, Velpeau's, d Gariel's splints such a superiority, that he has cured without shortening all fractures of the body of the femur, for the treatment of which he has plied it, either in hospital or in town practice.
6. The expense of the apparatus is but trifling, the straps, transverse bar, d extending screw, costing only a few shillings.—*Dublin Quarterly Journal*, v. 1871, *p*. 285.

45.—ON EXCISION OF THE HIP-JOINT.

By Henry Hancock, Esq., Senior Surgeon to Charing-cross Hospital.

e objections to the performance of this operation, which are most usually ed, are as follows:]

f the acetabulum is carious, the patient, whether operated upon or not, must

When caries attacks the surface of a joint, it is never limited to one of the es which compose it; therefore, as the acetabulum does not admit of oval, the operation is useless, as it does not get rid of the whole of the ase.

hat the operation is bloody and formidable.

hat, to be successful, the head of the bone must have been previously ocated.

hat at the time of operation there must be no, or at all events very little, ase of the acetabulum.

hat there should be no communication between the outer abscess and the vis.

hat the disease must be in its last stage.

Whilst, lastly, it is objected that the disease may possibly be mistaken for of the sacro-iliac synchondrosis or of the pelvis itself; and that, moreover, the ration is useless, as death in hip disease does not occur from local mischief, from the exhaustion resulting from the constitutional malady.

Now, in the first place, Is the acetabulum always implicated or not? It y be in a very large majority of cases: it certainly is not so in all. Of eighty- e cases in which the fact is specially alluded to, I find that in eighteen, or rather more than one-fifth, the acetabulum is expressly stated to have been healthy.

Although these exceptions are too numerous to prove a general rule, they are by no means sufficient to divest disease of the hip-joint of its gravity and fatal character. If it be true that he who suffers from caries of the acetabulum *must die*, caries of the hip-joint must take its place amongst the most destructive and malignant diseases, even rivalling in its fatal character cholera, typhus fever, small-pox, &c., since for four-fifths of their number there is no hope, they must inevitably die. But is this actually the case? Is caries of the acetabulum of so peculiar a character that it is not amenable to the same laws as govern that disease elsewhere? Is the cotyloid cavity so entirely without the pale of vitality and so wanting in reparative power that when once attacked with caries it cannot be cured, and the patient must inevitably die?

This seems to me to be an extremely difficult position either to maintain or to prove.

There are many who assert that these cases, if left to nature, rest, and proper nourishment, will do well of themselves. There are many of these cases which recover without operative interference, and of which post-mortem examinations are never obtained. There are patients, again, who pass through the successive stages of the disease—inflammation, ulceration, suppuration, spontaneous luxation, flattening and absorption of the head and neck of the femur—and yet who preserve their lives at the expense of the joint. True it is that in these cases the acetabulum as an acetabulum is not cured—it is obliterated; it undergoes the double process of disintegration and filling up; it becomes, in fact, in process of time, a thing of the past. But the patient does not die; for whilst his life has been endangered by the progressive destruction of the joint, it has now been saved by completion of that destruction and the subsequent reparation and reproduction of bone and soft parts.

As illustrative of this fact take the case operated upon by Mr. Anthony White, in which there was not a vestige of the acetabulum remaining; take also the following case, operated upon by Sir William Fergusson in 1849:—E. N., aged 10 years, two years and a half before had been knocked down by another girl, struck her left hip, and was at once lamed by the accident. In process of time abscesses burst around the joint, and she suffered much from pain and swelling extending from the knee along the back of the thigh. Twelve months after the disease began, the parents one morning observed that her thigh was altogether flexed across the abdomen, and that she was unable to move it from this position. Fresh abscesses occurred, and the suppuration with the constant pain destroyed her health, and she became weak and emaciated to the last degree. After remaining at a water establishment for six months she was taken to Kings College Hospital. The left buttock was unnaturally prominent, smooth, and tense, and over the back of the ilium was a hard globular swelling, evidently the head of the femur dislocated upwards. Around this were various sinuses burrowing in all directions, and communicating with the carious head of the femur. Some of these sores were situated in the groin, some over the sacrum. A very large continuous one extended down the thigh, and they all discharged much unhealthy pus. The slightest movement or pressure caused her great pain. She lay on the sound side of her body, with the knee of her diseased limb thrown over the opposite thigh.

On January 11 Sir William Fergusson enlarged one of the sinuses, and removed a portion of dead bone which was loose. The head of the bone was found in a carious condition, and he consequently removed it on January 13. The head of the bone was much reduced in size, and carious. There was hardly any trace of the cotyloid cavity, it being filled up with new bone.

Do not these and analogous cases also offer a very cogent reply to the objection that the operation must have originated and been conducted in forgetfulness of the well-known pathological fact that when caries attacks the surfaces of a joint it is never limited to one of the bones which compose the articulation, and that as the acetabulum cannot be removed with any prospect of advantage in the living body, therefore excision of the head of the thigh-bone for caries of the joint should be regarded as no less erroneous in theory than objectionable in practice?

It seems to me that, so far from this being the fact—so far from regarding it as either one or the other—by performing excision of the head of the thigh-bone in caries of the joint we emulate the proceedings of nature, and assist those whose powers of constitution are too weak, and who are consequently unable to carry those proceedings to completion.

Mr. Rankin, of Carluke, has an illustrative specimen in his possession. "The head of the femur in this case was discharged spontaneously, and bears the brand of what has long been called caries; yet after many years the person is alive and healthy, with a free joint over the pelvis, though the knee is flexed and anchylosed." According to Mr. Burford Norman, the late Mr. Liston possessed the head of a femur which had been detached and spontaneously expelled; and on the table before you is a similar preparation spontaneously expelled from a patient of Mr. Gant, to whose kindness I am indebted for the opportunity of exhibiting it to you. How frequently, also, has the head of the femur, so separated, been removed by operation. We have already seen that the very first removal of the head of the thigh-bone from the acetabulum was a case of this description performed by Schmalz in 1816. In 1730 John Daniel Schlichting dilated a fistulous opening over the hip of a young girl aged 14, long the subject of hip disease, and through it extracted the head of the femur, which had become detached. Similar cases are recorded by Vogel, Harris, Reid, &c.

Watch a patient suffering from scrofulous disease of the hip-joint: for many months the symptoms may consist only of stiffness, tenderness, and more or less weakness and limping, without hectic or constitutional disturbance of any kind. Watch the same patient during the progress of ulceration, suppuration, destruction of the ligamentum teres, and consequent dislocation: then the hectic and constitutional irritation are at their height. Watch the same patient again when dislocation has completely obtained: the disease now appears to have exhausted its rage, to have overcome all obstacles to its course, and to be satisfied with the mischief it has caused; the constitutional symptoms subside, the abscesses frequently heal up, and a cure takes place.

Can it be asserted that a case such as this is not one of caries of the joint? Can it be also asserted that the acetabulum is not here affected? that the fact of a cure is conclusive against such a proposition?

It would seem that in such instances the luxation of the head of the bone is essential to recovery; and when it does take place spontaneously, other symptoms being favourable, I would not urge too great haste in proposing an operation. But, alas! how frequently is it otherwise; how frequently does the strength of the patient prove insufficient to cope with the disease. The bone becomes carious, rough, or broken up; sinuses form, they continue to discharge, and still further to weaken the patient; hectic fever, night sweats, emaciation, loss of appetite, restlessness, cough, in some cases, spitting of blood, ensue; the lungs become affected, and the patient dies. It is in such cases as these, when the irritable and frequent pulse, the hectic flush, the wasting strength, the anxious and suffering countenance too surely denote the undue severity of the trial and the too great probability of death, that the Surgeon should throw aside his doubts, should boldly confront the mischief, and, by imitating the wise and beneficent workings of nature, endeavour to snatch his patient from that destruction which otherwise inevitably awaits him. We have seen what takes place in the acetabulum when spontaneous dislocation of the head of the bone has taken place. What, I should like to ask, is there in the operation to prevent the same process taking place after the head of the bone has been removed artificially?

But if these objections have had their influence, and been allowed to prevail as they undoubtedly have done, what a signal triumph must excision of the hip-joint have achieved, since we find that, of 126 patients, suffering from caries, who according to these theories must inevitably have died, no less than seventy-one recovered with more or less useful limbs!

Again, we are told that the operation for removal of the head of the thigh-bone should be restricted to those cases in which the head of the bone is dislocated. Although this was a point mainly insisted upon by those who advocated the operation prior to the year 1857, no one attempted to assign a reason for the restriction save Mr. Coulson, who says—"The first condition is the absolute displacement of the head of the bone; for as long as the neck and the trochanter are in relation to the acetabulum there is a chance of anchylosis taking place, which is the most favourable termination to be looked for."

This appears to me to be a scarcely tenable position; it has never as yet been proposed to operate whilst a chance of cure by other means has been apparent. Anchylosis, it must be remembered, is the last process of hip-disease; it must have been preceded by the inflammation, ulceration, and suppuration, and by the hectic fever, constitutional irritation, and consequent weakness and emaciation. It is by no means all who die of hip-joint disease who have attained the stage of anchylosis.

We know that whilst in some cases the head and neck of the femur may be destroyed by caries, in some they are comparatively sound; and in others again, both they and the acetabulum are free from disease, the patients dying from the intensity of the constitutional irritation rather than from the amount of local mischief. Take the following case, related by Sir Benjamin Brodie:—A young lady, aged 9 years, being at play on Jan. 1, 1808, fell, and wrenched her hip. She experienced so little uneasiness, that she walked out that day as usual. In the evening she went to a dance, but while there was seized with a rigor, was carried home, and put to bed. Next morning she was much indisposed, and complained of pain in the thigh and knee. On the following day she had pain in the hip, and was very feverish. These symptoms continued; she became delirious, and died just a week from the time of the accident.

On inspecting the body on the following day, the viscera of the thorax and abdomen were found perfectly healthy. The hip-joint on the side of the injury contained about half an ounce of dark-coloured pus, and the synovial membrane, where it was reflected over the neck of the femur, was destroyed by ulceration for about the extent of a shilling.

In another case, the preparation of which was shown to the Medical Society of London by Mr. W. Smith, the patient, a boy in the employ of one of the railway companies, was delivering parcels on a Christmas morning. In getting out of the waggon he slipped, and sprained his hip. He thought nothing of it at the time, but after a few hours he became so ill that he was obliged to be taken home. The subsequent progress of the symptoms closely resembled those in Sir Benjamin Brodie's case, and the boy died within a week after the accident. The hip-joint after death was found to contain about a tablespoonful of decomposed blood and matter, and the synovial membrane presented a rent about three-quarters of an inch long at the point where it was reflected over the neck of the femur.

Fortunately, such cases as these are rare, but I think that in the present day few Surgeons would fail, when the delirium came on, to recognise the gravity of the case, or hesitate for one moment to excise the head of the femur, and thus get rid of the poisonous fetid discharge and its source, and at the same time afford the patient a chance of recovery, since the conformation of the joint is such as to render a mere opening into the capsular ligament insufficient for the evacuation of the pent-up discharge.

We know, also, that the head of the bone may be entirely detached from the neck, and still remain in the acetabulum; and, lastly, that caries and anchylosis may coexist in the same joint, and the patient sink worn out before the latter is completed notwithstanding the head and neck of the femur have remained in relation with the acetabulum.

It is possible that the dread of exposing so large a joint-surface as the acetabulum, and of the supposed consequent tax upon the patient's power of endurance in getting rid of the joint structures and the obliteration of the cavity, may have influenced this recommendation; but experience has proved that the danger of exposing joint-surfaces is for the most part inverse to the extent of surface exposed, whilst it has also shown that in this operation the presence of the investing cartilage exerts but comparatively slight influence over the healing of the wound.

The stress laid by Mr. Anthony White upon the displaced head of the bone acting as a foreign body, and also upon the complete obliteration of the acetabulum, with the successful termination of that case, doubtless had its weight;

t it is curious that in Schmalz's case—the first, as we have seen, on record oth conditions obtained. The head of the left femur was dislocated on to dorsum ilii. That of the right was still in its cavity, which, however, was a state of suppuration. It was the latter which Schmalz excised, and the ient did well.

A *priori*, it is reasonable to suppose that when the head of the bone has en dislocated for sufficient time for the acetabulum to be obliterated, the eration would offer a greater chance of success than when performed under ıer circumstances; that, in point of fact, the process of healing would be plified and shortened. But against this must be placed the intense strug- of that fearful period preceding dislocation, which tries the patient's powers endurance to the utmost limit; and it should, moreover, be remembered at the value of this recommendation, if any, exists, not in the mere displace- ent of the head of the bone, but in the obliteration, cure, or absence of dis- se, of the cotyloid cavity. And this coincides for the most part what is oved by the statistics of the operation. Sixteen operations were performed which spontaneous luxation had taken place, and wherein the acetabulum as found to be either entirely obliterated or in a healthy condition; of these were cured, 2 died, and the results of 6 are stated to have been doubtful. orty-three operations were performed in which spontaneous luxation had ken place, but wherein the acetabula were found diseased, and in many in- ances were gauged, in some few cauterised; of these 43 operations, 16 sulted in cure, 18 died, and the results of 9 were doubtful. Forty-four perations were performed in which the head of the bone was found in the cetabulum, but in which the latter was implicated in the disease; of these, 8 were cured, 10 died, and the results of 6 were doubtful. Eight operations ere performed in which the head of the bone was found in the acetabulum, e latter being healthy; of these, 3 recovered, 4 died, 1 was doubtful. Ten perations were performed in which the head of the bone was found absorbed nd the acetabulum diseased; of these, 5 were cured, and 5 died.

Hence we find that, had the recommendation that the operation should be stricted to those cases in which spontaneous luxation had already taken lace prevailed, the sphere of its utility would have been very much and very rejudicially contracted, since of 123 cases, 52 were performed in which the ead of the femur remained in the cotyloid cavity.

It is sometimes extremely difficult, if not almost impossible, to tell with any gree of certainty prior to the operation whether the head of the bone be dis- cated or not. And after all, the success of the operation would appear to pend very much less upon this point than those who made this recommen- tion seem to have supposed; for we see that in the 16 cases where the head the bone was dislocated and the acetabulum free from disease, the mortality s 12 per cent. In the 43 cases where the bone was dislocated and the ace- ulum diseased, the mortality was 56 per cent. In the 10 cases where the d of the bone was absorbed and the acetabulum diseased, the mortality s 50 per cent. In the 44 cases where the head of the bone remained in the yloid cavity which was diseased, the mortality was 28 per cent.; whilst in 8 cases in which the head of the bone remained in the cotyloid cavity, the er being healthy, the mortality is by far the highest, being 62 per cent.— *Times and Gazette, March* 16, 1872, *p.* 305.

—DR. BIGELOW'S VIEWS ON DISLOCATION OF THE HIP-JOINT.

ONATHAN HUTCHINSON, Esq., Senior Surgeon to the London Hospital, and Lecturer on Surgery.

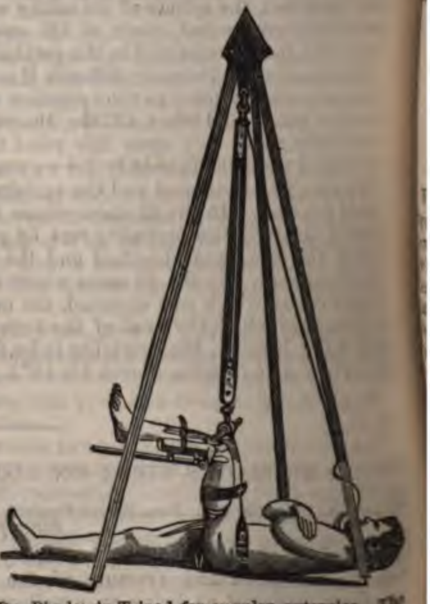
Bigelow, of Boston, U. S., has lately published an essay on the Mechan- of Dislocation and Fracture of the Hip. Mr. Hutchinson recommends s essay strongly, as being an excellent example of experimental investiga- n applied to a subject concerning which but too many have been contented rely to speculate.]

The main fact to which I must ask your close attention is, that the ilio-femoral ligament is the strongest part of the envelopes of the joint; that it is very rarely torn through; and that it is to it that the peculiar positions of the bone in the several displacements are chiefly due. This ligament must be described as arising from the anterior inferior spine of the ilium, passing downwards and outwards, and being inserted into the whole length of the anterior intertrochanteric line. Thus at its origin it is not more than half an inch broad, and at its insertion more than two inches. It is supplementary to the capsular ligament, over which it passes, and with which it is inseparably connected. You will find in most anatomical works that the term ilio-femoral or "accessory intertrochanteric ligament" is restricted to the lower part of the structure described; but as the whole arises from one part, there can, I think, be no question that it is well to regard it in its entirety. Dr. Bigelow proposes for it the name of the Y-ligament, because its lower fan-shaped expansion has a space between its upper and lower portions like that between the arms of an inverted ⅄. For myself, I doubt much whether anything is gained by re-christening it. However, whatever name we give it, the truth remains the same, that it is this ligament—as strong as the ligamentum patellæ or the tendo Achillis—which in dislocations restrains the movements of the great trochanter, and thus regulates the position of the limb. It is this ligament which, when we reduce by manipulation, guides the movements of the bone. We have long known that the ligamentum teres has nothing to do with these matters, being indeed powerless to keep the bone in the socket; and we may now, I think, discard, to a large extent, the supposed influence of muscles.

In the common form of dorsal or posterior dislocations, the Y-ligament holds the great trochanter to the pelvis, and thus inverts the limb. It also, assisted by other lower fibres of the capsular, hinders the passage of the head of the bone upwards, and thus keeps the shortening within limits. Upon it, as upon a pivot, the neck of the bone will move, if you commence manipulations and use the shaft as a lever. It will be by its aid, and not, as erroneously supposed, by that of the adductor muscles, that the head will be directed into the socket if you flex and then abduct (or, still better, circumduct outwards), as in our usual method of reduction.

Amongst the special services which Dr. Bigelow has done, is that of explaining the peculiarity of "the dislocations into the ischiatic notch," and of teaching us how to reduce them. His assertion is, that they differ very little from the majority of so-called dorsal dislocations, for in both the head of the bone lies not so much on the dorsum ilii as just behind, or behind and above, the acetabulum: and in neither does it really pass so far back as the sciatic notch. The real difference between the two consists in the relation of the head and neck of the bone to the tendon of the internal obturator muscle and to the portion of capsular ligament beneath it, these being below the neck in a true dorsal, and above it in the so-called ischiatic dislocation. This fully explains the increased difficulty in reducing the latter dislocations by the old method, and also the ease with which the *flexion* plan succeeds.

Dr. Bigelow believes that the greater or less laceration of the



Dr. Bigelow's Tripod for angular extension. The pelvis is buckled to the floor. The flexed leg is suspended by pulleys from the apex of the tripod. A bar fixed to the splints, and crossing the knee transversely, serves to rotate the limb.

capsular ligament is a matter of very little consequence to the patient's chance of good recovery; and he instructs us how, in cases when the smallness of the hole in this structure is supposed to be the impediment to reduction, we can easily and safely tear it as much as we wish.

It is not my intention to bring before you in detail at the present time the important statements made by the author I am quoting respecting the other special forms of dislocation. These may be better reserved until we have an opportunity for demonstrating the dislocations on the dead body. It will be sufficient at present to say that they seem to me to simplify greatly a very difficult subject, and that I have no doubt that they will speedily bear good fruit at the bedside. What I chiefly wish you to recollect is, that it is the strong ilio-femoral ligament which in the backward or dorsal dislocations, holds the great trochanter forwards and inverts the limb, which in the dislocations downwards, still holding the trochanter, flexes the limb, and in dislocations of the head of the bone inwards (thyroid), still restraining the movements of the great trochanter, everts the limb. All these results can be easily witnessed by experiment in the dead body. Lastly, in some rare cases in which the displacement follows no rule, the explanation is to be sought in the fact that this ligament has been wholly or partially torn through. It is, however, very strong, and its rupture is very infrequent.

I need scarcely add that Dr. Bigelow is of course a warm advocate of the manipulation, or flexion, method of reduction. He urges that almost anything may be done if you only recollect the manner in which the ilio-femoral ligament will be stretched in the different positions of the bone. Extension horizontally, whether in a straight line or obliquely, he for the most part opposes strongly, as only likely to be successful after forcibly tearing the important structure referred to. Extension with the thigh bent at right angles to the pelvis he as warmly advocates, for in this position the ligament is relaxed. For the accomplishment of this he has devised a special apparatus, which looks likely to be very useful. As a practical suggestion likely to be available under the frequent conditions when this triangle may not be at hand, he advises that the surgeon should place his foot (unbooted) on the patient's pelvis, and then lift from the knee.—*British Medical Journal, Dec.* 28, 1872, *p.* 718.

47.—THREE CASES OF WOUNDS OF JOINTS.

Under the care of Mr. Hey and Mr. Jessop, at the Leeds General Infirmary.

We are very glad to bring before the notice of our readers the three following cases of injuries to joints which have all terminated favourably. They will be read with great interest by all who perused Mr. Couper's case of injury to the knee-joint which we reported a week or two since. It will be seen that the antiseptic treatment was strictly followed only in the first two cases, but that the result was equally favourable in all of them. The first case was undoubtedly the gravest. Injuries to joints, except as part of very severe injuries, are not common, and we here repeat that we shall be glad to receive details of any similar cases.

We are indebted for the notes of these cases to Mr. McGill, the resident medical officer.

Case 1.—*Wound of the ankle-joint.*—J. B., aged thirty-five, was admitted under the care of Mr. Hey on Aug. 2nd. There was a wound one inch long situated over the left internal malleolus; through this wound it could be felt that the malleolus was broken, and that the ankle-joint was opened, the finger passing between the tibia and the astragalus. There was also a small wound on the anterior part of the ankle which did not communicate with the joint. The accident was caused by a block of stone falling on to his leg.

The joint was syringed out with carbolic lotion (1 to 20) and the antiseptic dressing applied with all care in the usual manner. The leg was placed on a swing cage. The *antiseptic* dressing was continued for a fortnight, by which time the wounds were healed without suppuration. On the evening of the first day the temperature was 100°; it was never any higher, and was normal by the fourth day. The splint was kept on for a month. The patient was discharged with the leg in a plaster-of-Paris bandage on Sept. 5th. He is no walking about with a perfectly movable joint.

Case 2.—*Wound of the knee-joint.*—J. S., aged thirty-one, was admitted Ju 2nd under the care of Mr. Jessop, with a wound four inches long across t thigh, one inch above the patella; through this the finger could be pass into the joint, and the posterior surface of the patella plainly felt. Round the lower and inner sides of the patella were three other wounds, each about one inch long; no communication of these with the joint was made out. The injury was caused by the patient falling with his knee bent through an earthenware pan.

A back splint was applied. The antiseptic dressing was applied, but the joint was *not* syringed out with carbolic lotion. The antiseptic dressing was continued for eleven days, by which time all the deep part of the wound was healed, and a small superficial granulating surface was left. Twelve hours after the accident the temperature was at its highest, 99·8°; it became normal by the fifth day. The pulse was never above 92. On July 30th the splint was removed. The patient was discharged a few days after; he now walks with a movable joint, without any limp.

Case 3.—*Wound of the knee-joint.*—P. M., aged twenty-nine, admitted Jan. 26th, under the care of Mr. Hey, with a small perforating wound below and to the outer side of the patella, through which synovia was flowing. A probe could be passed up into the joint through the opening. The wound was caused by the man falling three feet on to a sharp-pointed iron rod belonging to some machinery in a flax-mill.

A back splint was applied, and the wound left uncovered. After twelve hours, as the discharge of synovia still continued, carbolic acid and glycerine was applied, and was changed daily for a week; no precautions, such as are used in the antiseptic dressing of wounds, were observed. The wound healed entirely in two days. The temperature reached 100·2° on the third day, when there was some effusion in the joint, but became normal two days after. Now (February 11th) the effusion is all gone, the splint has just been removed, and the movements of the joint are perfect. The patient will probably be discharged in another week.—*Lancet, Feb.* 24, 1872, *p.* 256.

48.—ON AMPUTATION FOR DISEASE OF THE KNEE-JOINT.

By J. Cooper Forster, Esq., Surgeon to Guy's Hospital.

[In the first part of this paper the writer describes four cases of amputation for knee-joint disease, and then passes on to one or two points in the operation.]

You will have observed that, except in one instance, where disease of the soft parts prevented its application, two lateral skin flaps were made in amputating, and then a circular section of the muscles to the bone. My colleague, Mr. Poland, as you saw the other day, performs the ordinary flap operation. It is impossible, and indeed unnecessary, to institute any comparison between the two; either procedure would have been equally suitable, as far as the mere amputation was concerned, in all my cases. I prefer, however, my way because we practise torsion here; and I believe on this plan the twisting of the vessels is rendered much more easy of accomplishment. In muscle-flaps the vessels are *slit* often for some little way along their channel, and so torsion becomes a little difficult in its application. Remember this in practice; for no student leaving Guy's Hospital will, I apprehend, ever use, except, may be, in very exceptional instances, any other method of arresting hemorrhage than this. I *sometimes* fear, indeed, that some of you may not know how to apply a *ligature to an artery* on the living subject.

But I have other objections to the flap operation as usually performed; and of these you saw during the last amputation. I had to cut out a piece of the tic nerve from the flap. This is a proceeding first, I think, suggested by Hilton; and it is done with the idea of obviating any liability to what is ed "irritable or painful stump." All nerves, when they are cut across and divided ends separated, become what is styled bulbous—that is to say, new ue is thrown out in and around their sheaths from inflammatory action; there- a, in this respect, the circular and high division of the muscles would be no ter than the other. But in the ordinary amputation, unless the nerve be cut oss a second time, and dissected out of the flap—a method which I rather ect to,—the bulbous ends become involved either in cicatricial tissue, and is are pressed upon and made painful, or they are bound down at the ex- me end of the stump, and become liable to pressure from various causes. A ular-division of the muscles at the bottom of the wound allows the nerves become swollen out of harm's way; and therefore they produce then no nptom.

We have been remarkably free from such after ill-effects as irritable stumps this hospital, and I attribute it entirely to the cutting away of the nerve or king a high section of it. When, on the contrary, this is not looked to, haps in a few weeks, or it may be months or years, according as the scar- sue contracts rapidly or the reverse, or pressure is brought to bear upon the mp soon or late, the patient will return to you complaining of agonising in, and with tonic contraction of the muscles of the limb as its result, and entimes with some indolent ulceration about the cicatrix. The only treat- nt then available will be to submit the sufferer to another operation, opening the stump, and dissecting out the offending nerves.

In all these cases torsion was used, and in one of them—the first case in four ars—some bleeding occurred a few hours after the operation; possibly this y have come from a twisted vessel, but I do not think so, and for the fol- ving reason. We have been using lately a fine spray of carbolic-acid lotion ected over the stump during the twisting of the vessels, and I have frequent- noticed, and my dressers with me, that the backs of our hands, over which spray plays in common with the flaps, have been quite benumbed at the mination of the operation; and if this be so with regard to parts covered h skin, much more so, I apprehend, will it be the case when the spray is ught into contact with the exposed muscle and vessels. It thus becomes to see that very probably some small vessels contracted at the time of the ation and ceased to bleed; but when the parts became warm again, and muscular fibre relaxed, then blood could flow a second time from their open s by simply pushing away by the force of its current any slight clot that ht have formed during the period of closure. If you accept this explana- of the bleeding in this case, you will be ready to admit that any like in- ce with it must not be used as a fact against torsion by the supporters of other custom. If the hemorrhage occurs as the result of overlooking a sel, it is quite certain that, surrounding conditions, such as cold or spray, g the same, the oversight would be just as likely to occur with one method nother. You must separate in your minds an occurrence like this, which nly a return of bleeding from temporarily closed vessels, within a few rs of, and the immediate consequence of the operation, from cases of true *ndary* hemorrhage, such as occur after ligature at the time of its separation, possibly shortly before. An instance such as this has not occurred to me e during the time that I have practised torsion, now some four years and re.

n one or two of our cases we had a little difficulty in arresting the bleeding the time of the operation by its means; but this is a matter depending part- on the number of vessels discharging blood, and partly on the general health the patient. With respect to the former element, in Case 4, it so happened t only one vessel required twisting, that one being the femoral; and a like sence of small vessels in the flaps has occurred to me once before. On the er hand, if the powers of the patient be feeble, there is often a trouble aris- ; more from a general oozing from the whole surface than from any definite int that can be seized and closed.

Mr. Lister has discarded *torsion*, and now uses gut ligatures carbolised, which are cut off short and left in the wound. It is supposed that by some means they become absorbed, or possibly even revitalised by blending with the lymph and new tissue which unite the flaps. I must confess, however, that do not feel inclined to abandon torsion; for a method that can be applied fo four years to all cases of hemorrhage from wounds that have come under notic indiscriminately, which is quite free from any necessity of selection of fit an proper cases in order to ensure its success, and which, moreover, has never my knowledge during that period been followed by *secondary* bleeding, requir no advocacy by word of mouth. The fact need only be stated to demand trial by those who have not yet attempted it, and to ensure a continuance of method by those who have commenced its use.

You will have noticed that I now perform all amputations under the antiseptic method with carbolic acid and all its paraphernalia. Last year I had eleven amputations of the thigh—all done without any such precautions; now all are performed with them. I hope by this means to arrive in a year or two at some definite conclusion on the subject, and to form some rule for future practice. Possibly, if hemorrhage again takes place soon after the operation, we may have to abandon the spray for some other means of disinfecting the surrounding atmosphere.—*Lancet, March* 30, 1872, *p.* 425.

49.—ON THE INSTRUMENTS DESIGNED FOR EXPLORING GUNSHOT WOUNDS.

By T. LONGMORE, Esq., C. B., Professor of Military Surgery at the Army Medical School, Netley.

[Whenever a gunshot wound is large enough to admit of the insertion of the Surgeon's finger, this alone should be used for exploring purposes. No artificial instrument can give the same amount of information. If necessary, the orifice of the wound may be enlarged by incision, for this is preferable to using undue force in introducing the finger.]

If the finger be not sufficiently long to reach the bottom of the wound, even when the soft parts have been approximated by pressure from an opposite direction, and when the lodgment of a projectile is still suspected, or some other point of doubt remains to be solved, such as the direction the projectile has taken in the latter part of its course, we are compelled to make a further exploration by other means. Generally, a long silver probe, that can be bent if required, and that can be guided into a definite direction at the will of the surgeon, will be found to be the best substitute for the finger. The probe should be used with great discretion, for, without care, it may readily be made to inflict injury on vessels or on other structures which have escaped from direct contact with the ball, but have returned by their elasticity to the situations from which they had been pushed or drawn aside during its passage. It seems hardly necessary to observe that these directions for examining gunshot wounds apply only to such as penetrate the extremities or extend superficially in other parts of the body; where a missile has entered any of the important cavities, such exploration would obviously be as useless as it would be mischievous.

In the majority of cases of gun-shot wounds there will be no difficulty in detecting the lodgment of foreign bodies, especially heavy ones, as bullets and fragments of shell, when the examination has been made early by the finger in the manner described. Sometimes, when the finger in the wound fails to find a lodged projectile, the particular spot in which it is lying may be detected simply by relaxing the muscular tissues, so as to give a loose and pendulous condition to the parts concerned, and then lightly tossing up the flesh at different points from below with the tips of the fingers. A bullet lodged among the soft parts will occasionally make its presence known, under such an action, by the impulse which it communicates to the top of one of the fingers when the parts which have been shaken upwards return to their previous position. Sometimes a gentle kneading pressure in the neighbourhood of the injury, assisted by information derived from the sensation of the patient, will lead to the detection of such a foreign body. Sometimes, as mentioned in the English official history of the Crimean war, when a lodged bullet could not otherwise be discovered, it was found by passing the flat palm of the hand down a limb. Its presence was occasionally detected in this way when the points of the fingers had utterly failed to feel it.

We come now to the case in which the exploration by the surgeon's finger is altogether impracticable, and in which the use of the probe is attended with so many sources of doubt as to prevent any satisfactory conclusion in respect to the lodgement or absence of foreign bodies from being arrived at. These difficulties sometimes happen with wounds in situations where they might be least expected to be met with, but generally occur in such as have their terminations at or near some of the solid structures of the body. They are every now and then experienced in recent wounds, especially deep and narrow wounds made by small-bore rifle or pistol shot, but are more particularly so in chronic wounds in which foreign bodies are suspected to be lodged; wounds in which the original bullet tracks have become contracted to narrow sinuses, and in which other changes have taken place rendering the directions of these sinuses tortuous or otherwise intricate. The finger may not be able to penetrate the small and constricted passage of such a wound; and the probe, even if it be enabled to traverse it, and happen to reach a hard substance, may fail to give the desired information as to its nature—whether it is striking against bone or the foreign body which is suspected to be lodged.

It may be readily ascertained by striking or rubbing a leaden bullet out of the body with a silver probe, and comparing the peculiar dull sensation conveyed to the fingers with the sensation experienced when a piece of bone is struck or rubbed, that one cannot under such circumstances be deceived with the eyes shut as to the respective difference between the two substances. But when the bullet or piece of bone is at the bottom of a wound, and the probe comes in contact with the side of the wound, especially if this side happen to be bone; or if the walls of the track be fleshy, and, while the probe is in contact with them, its extremity is pressed against bone which has become smooth and eburnated on the surface; or if any soft tissues intervene between the end of the probe and the object impinged upon it; it will be found such a complicated sensation is given to the fingers that the diagnosis is rendered exceedingly difficult and uncertain.

No more remarkable illustration of the difficulty of diagnosis just adverted to could be adduced, perhaps, than was afforded in the instance of the wound received at Mentana by General Garibaldi. In that case, the opening presented to the surgeon beneath the integument consisted of a fissure across the base of the inner malleolus. This fissure was not wide enough to admit a finger; and ordinary probes, when inserted, so failed to give satisfactory evidence on the important question of a foreign body being impacted in bone near the ankle-joint, that some of the ablest surgeons in Europe, after exploring with them, were led to declare that no bullet or foreign body had become lodged in the wound. And this occurred in a case where the opening made by the projectile was not much more, if any more, than an inch in depth, in which the track was not tortuous, nor among tissues of different kinds or of intricate arrangement, as frequently happens in doubtful cases.

M. Nélaton, after his visit to General Garibaldi, was led to think of various devices for determining whether the ball was lodged in the wound or not. His first idea was to obtain a steel probe cut like a file at one extremity. He presumed that, by passing such an instrument down to the substance which was suspected by some to be the bullet, and by giving it a rotatory motion, sufficient would be brought away on the teeth of the file to determine its nature, whether bone or lead. At the same time that he was having this instrument made, he reverted to the idea of a chemical reagent, which had been tried before in similar cases of doubt, but without success. Acting on this motion, M. Nélaton applied to M. E. Rousseau, the chemist, to furnish him with some ample means of determining the presence of lead in a wound by chemical analysis. M. Rousseau then suggested the introduction of a body capable of bringing away a metallic impression, should metal be present, such as rough porcelain; thus making the metal capable of being recognised not only by chemical reaction, but by all its ordinary characteristic physical signs. This suggestion led to the construction of the instrument which, since its successful application in Garibaldi's case, has become known as Nélaton's probe.

Nélaton's probe consists of a slender rod of metal, five or six inches in length, terminated at one end by a small knob of white unglazed, biscuit china. The other extremity of the probe is furnished with a small handle, grooved ridge-and-furrow fashion, in order that the finger and thumb may the more easily roll it between them, while the porcelain knob is being pressed at the bottom of the wound against the suspected foreign body. If it be a leaden bullet against which the porcelain is rubbed, a very distinct mark of lead is impressed on the latter, which is not easily obliterated. The bullet itself is thus caused to give ocular demonstration of its presence and place of lodgment. If the foreign body be iron, having a rusty surface, a stain of rust will be found on the china.

The round ball of china which is fitted to the Nélaton test-probes usually has a diameter of rather more than a quarter of an inch. This is often large enough to cause a difficulty in introducing it through the small fistulous tracks among fibrous tissues which are frequently met with as one of the chronic effects of gun-shot wounds. A sinus of this kind, unless connected with necrosed bone, usually leads to suspicion in the mind of the surgeon that it is prevented from becoming completely closed by the lodgment of some foreign body which passed along it at the time of the original wound. It is, therefore, just one of the cases in which such a test-probe offers itself as a valuable diagnostic aid to the surgeon, but where the size of the round china knob too often prevents the attainment of this object. A probe tipped with a piece of the biscuit china of less diameter, and more oval in form, is required for these narrow fistulous tracks. In a recent wound, the larger round ball is more convenient. It is not so likely to be impeded in its passage along the wound, and the impression made by the lead upon it is more obvious to observation.

The porcelain test-probe was much used during the war of the rebellion in the United States, and many favourable reports of the results of its employment have been published. A very ingenious modification of it, but one of doubtful practical utility, was manufactured during the war by the eminent surgical instrument-makers, Messrs. Tieman, of New-York. In this instrument, the porcelain is placed at the extremity of a long flexible gum-elastic tube, which takes the place of the metallic stem in the ordinary probe. The length of the whole instrument is thirteen inches, the flexible part being nine inches long. By turning a screw at the handle in different directions, and by means of a mechanical contrivance concealed within the tube, the lower part of the instrument, at the end of which the porcelain is placed, can be caused to turn slowly round to the right or left, at the pleasure of the operator. It may be moved round so as to assume a considerable curve in either direction.

This instrument has obviously been designed with the purpose of applying the porcelain test in cases where the ordinary Nélaton probe cannot be used; more especially along deep and tortuous bullet tracks, whose ultimate direction is either uncertain or known to be turned from a straight course. The operator is supposed to cause the porcelain to *search* for the foreign body, as well as to identify it when found by bringing to light the usual evidence of its presence. I have only tried the instrument on the dead body, but the experiments have not been attended with favourable results. The chief sources of failure with it, were found to be, firstly, the uncertainty concerning the direction taken by the end of the instrument when turned in search of the bullet among yielding tissues, at distances far removed from the aperture of entrance and from sight; and secondly, the difficulty of applying sufficient force to get a metallic impression, even when the point of the instrument was directed against the foreign body, owing to the flexibility of the long tube at the end of which the button of china is placed. Another difficulty experienced was the following. If the china knob became entangled in the tissues at the end of a long track, or was prevented from moving freely by any cause, then the screw, not being able to act on the extremity, caused the middle flexible portion of the instrument to become curved, so as to press on one side the soft tissues of the track of the wound, along which the probe had been passed. It was only through the displacement of the soft tissues being visible externally after it had occurred to some considerable extent, that a knowledge was gained of the fact that the porcelain end was not moving in obedience to the screw, but only that part of the probe lying in the track of the bullet.

It is evident, from the nature of the porcelain test-probes just described, that direct and firm contact between the porcelain and a bullet is essential, in order that the former may furnish the evidence which the surgeon requires. If it be merely a little blood, serum, or soft coagulum in front of the bullet, pressure by Nélaton's probe will squeeze it away, and the leaden mark can be obtained; but if any resisting medium, however thin—the thinnest membrane, for example—happened to be placed between the surfaces of the metal and the china, no impression will be made on the latter. And there are various substances which are liable to be interposed—such as muscular or cellular tissue pushed by the knob of porcelain itself before it; pieces of linen, cloth, paper, or other substances which have entered with the bullet; a piece or edge of bone projecting in front of it, and other such matters. The evil in such occurrences may not simply be the impediment to obtaining an impression from the lodged bullet; but possibly a surgeon may be led to an erroneous conclusion that he has obtained proof of no foreign body being lodged, because the usual evidence of its lodgment is absent from the porcelain. This may lead to delay in the healing process and protracted suffering to the patient, which might have been avoided had a more correct diagnosis been arrived at.

To obviate these difficulties and sources of fallacy, a surgeon of the French army, Dr. Lecomte, invented an instrument, to which he gave the name of "probe-nippers" (*stylet-pince*). His design was not merely to indicate the presence of a leaden bullet, by bringing away a stain or mark of its presence, but by bringing away a small portion of the lead itself. Such an instrument could not only be used for bringing away a scale of lead, but also a minute portion of paper, cloth, wood, or any other foreign body capable of being cut in a similar manner. If the supposed foreign body were a fragment of bone, it would equally bring away a particle of it. It was evident that, in addition to its other qualities, the *stylet-pince* would require to have its nippers smaller in size than the porcelain knob of Nélaton's probe, with a stem of sufficient length to be passed along narrow sinuses, and at the same time solid enough to bear a certain strain in use. I have ascertained by practical experience that the *stylet-pince* does possess the qualities aimed at by Dr. Lecomte.

The instrument consists of two portions. The first is a central steel rod of small diameter, fixed in an ivory handle at one extremity, and cleft at the other into two small branches, each of which terminates in a little cup-like blade or curette; the second is a slender canula, which glides backward and forward, but only within a limited distance, along this rod. The central rod is fixed in the handle by means of a side-screw, which can be loosened at pleasure by the surgeon, so as to increase the length of the exposed part of the stem, or to enable it to be removed altogether to be cleaned. The two steel curettes have very fine and sharp edges. They separate from each other by the elasticity of the two little steel branches, of which they are the terminations; but they are easily brought together by a slight pressure, such as that exerted by causing the canula to glide along the central stem up to them. When they are thus brought together, the two curettes so fit one to the other that, united, they form a small smooth steel knob or rounded extremity, about one-third of the usual size of the china knob of a Nélaton probe.

It will be apparent, from the description, that the gliding of the canula determines the opening and closing of the curettes: when it is slipped back, the curettes open; when it is pushed forward, they are closed, and form a little hollow globe. There is no difficulty in the manipulation of the instrument. It is inserted with the curettes closed, and it may be used then precisely as the long silver probe in a surgeon's capital case of instruments. When about to be employed for determining the nature of the substance with which it is put into contact, the canula is drawn towards the surgeon, at the same time that the extremity is retained with an equable and steady pressure against the substance. *This movement has* opened the curettes. The same even pressure is sustained while the canula is pushed home, and this causes the curettes to be brought together again; their edges, as they close towards each other, nipping off a small particle of the subtance over which they are moved. The instrument is then withdrawn, and, supposing it to have been brought into contact with an ordinary bullet, a small scale of lead will be brought away enclosed within the cavity of the rounded extremity formed by the two curettes. The glistening surface of the freshly cut shaving of lead will sufficiently indicate its nature. If any difficulty should occur in distinguishing the particle which has been brought away, it can be removed from the curettes, and observation under an ordinary magnifying lens will show what the substance is. The *stylet-pince* is thus a most useful explorator for deciding doubtful cases of lodgment of foreign bodies; it responds as an indicator with even more distinctness than the Nélaton probe in all cases in which that test would be of service, while it answers for a variety of other cases in which the Nélaton probe would give no indication at all.

Electricity was some years ago suggested as a means of detecting lodged bullets and other metallic substances in wounds; and some very ingenious apparatus depending on this agent have been lately contrived for the same purpose. One of the first to experiment upon an electric appliance of this kind was a French military surgeon, M. Fontan, assisted by M. Favre of Marseilles. A description of the somewhat complicated apparatus employed in these experiments is to be found in the *Gazette des Hôpitaux* of the 29th November, 1862.

The improvements which have taken place in the modern applications of electricity have paved the way for more simple and yet more sensitive bullet explorers. One of these is the invention of M. De Wilde, a civil engineer, and is very compactly arranged in a box of small dimensions. The electric action is excited in a suitable cell; the electricity there developed is increased in intensity by the intervention of a multiplying coil; an exploring probe is connected by insulated wires with the apparatus; and the indication, when the circuit is completed by contact of the two points of the probe with a leaden bullet or piece of iron, is given by the striking of a hammer against an alarum-bell. The bell sounds at each interruption and renewal of contact of the points with metal. The exploring probe consists of a long slender tube of smooth vulcanite, containing two insulated needles, the points of which can be withdrawn within the tube, or be made to protrude, at the pleasure of the operator. Altogether it is an effective appliance as an exploring instrument, owing to the strength of the electric current developed, and the marked manner in which the indications are given by the sound of the bell when a bullet or other metallic substance is met with. There is also attached to the instrument a bullet-extractor, the two arms of which are insulated, and so arranged that, when they are connected, in the same way as the explorer, with the battery, they indicate the grasping of the foreign body similarly by the sound of the bell. Unless the metal be firmly grasped by both blades, without any other substance intervening, the indication will not of course be given.

Another instrument of a similar nature has been made by Messrs. Krohne and Sesemann of London. The indications of contact with a lodged bullet or other metal are in this instrument afforded by the movements of a galvanometer, and of a fine needle working upon a dial-plate, in the same manner as is seen in the ordinary single needle telegraph. I have experimented with both these instruments, and have found them equally effective in their indications. Attached to the latter instrument is not only a bullet-extractor as well as the explorer, but also a pair of acupuncture needles, for use in cases where metallic bodies are supposed to be lodged in soft tissues, away from any means of approach by a wound or sinus.

A rough but sufficiently effective electric instrument for facilitating the discovery of metallic substances lodged in gunshot-wounds has been made in the following way. The magnet of an ordinary pocket compass, which has had some turns of wire covered with thread wound round it as an induction coil, is employed for the electric indicator, while a piece of copper sheeting, bent round a small plate of zinc, but separated from it by flannel padding saturated with the usual diluted acid, forms the voltaic pile. The exploring instrument is

ormed by two insulated wires, bound together, but with the points left free.
hese parts being connected, when the circuit is completed by contact with
etal, the indication is given by movement of the magnet of the compass.

Lastly, the endoscope has been suggested for use in exploring for foreign
odies in wounds. Dr. Fenger of Copenhagen, in 1869, made some experi-
ents with the instrument on horses, and came to the conclusion that pieces
f cloth in wounds, or bullets driven into bones, could be seen by its means.
r. Fenger has stated that during the late war he was enabled in several in-
tances, on examining wounds some weeks after they had been inflicted, to see
heir interiors distinctly by means of the endoscope, without causing pain, he-
orrhage, or any subsequent irritation, in consequence of the introduction of
he instrument. I have no experience of such an application of the endoscope,
nd can hardly think that, with so many other effective appliances available, it
s likely to be turned to much practical account in this direction.

It must be sufficiently obvious that several of the exploring instruments just
escribed are not suited for use in the field. The circumstances of gunshot-
ounds themselves, as they are ordinarily presented shortly after their inflic-
ion, do not render such instruments necessary; nor are some of them—as
he electric-explorers for example, which may be easily disarranged—suitable
or use in the places where the wounds resulting from battle are usually treat-
d during their earliest stages. But it has not been the purpose of these re-
arks to discuss either the nature or circumstances of the wounds in which
struments of this kind may be especially serviceable; their object has been
nly to describe the instruments available for exploring for foreign bodies in
ases where the exploration is decided to be advisable. It is only right to re-
ark, however, in conclusion, that even in such cases there must be a limit to
earch. This limit must depend on the circumstances of each particular case,
nd must be decided by the judgment and tact of the responsible surgeon.
s a general rule, it may be stated that, whatever may be the reasons for con-
luding that a bullet or any other foreign body has lodged in or near a gunshot-
ound, if, after search by the finger in cases where a digital examination is
racticable; after external manipulation and observations made in varied pos-
ures of the part of the body concerned; after attention has been given to in-
cations derived from the patient's sensations; effects of pressure upon, or in-
ry to, nerves; and, lastly, after a moderate but careful exploration by one or
her of the exploring instruments just described, if, after these steps have
en taken, the site of the lodgment be still not ascertained, more especially if
e patient be suffering pain, or is in an exhausted condition, the exploration
ould be at once discontinued. The continuance of the search will not merely
d to the weariness and distress of the patient, but, if the several proceedings
ove-named have been properly carried into execution, it is not likely to be at-
ded with a successful issue. The foreign body will probably have passed
yond or out of the field of exploration, or have become deeply impacted in
ne, or entangled among tissues, where it could only be discovered by an un-
stifiable amount of meddlesome and injurious disturbance of the structures
plicated. We must rest in hope that, either during the process of suppura-
n, or under the influence of muscular actions, or by gradual approach toward
e surface, the escape of the foreign body, whatever its nature, whether hard
soft, smooth or rough, organic or inorganic, may be eventually effected with-
t such risks; or, if it be of a favourable kind and form, such as a leaden
llet is when its normal shape is retained, and if it be not in contact with a
rve, bone, or other important organ, that the wound may heal favourably,
twithstanding its presence, the foreign body becoming encysted, and remain-
g lodged without causing either pain or mischief for many years afterwards.
—*British Medical Journal, Dec.* 23 *and* 30, 1871, *pp.* 715, 751.

50.—ON SKIN GRAFTING.

By Thomas Bryant, Esq., Surgeon to Guy's Hospital.

[The well-known experiments of John Hunter, more than a century ago, may be said to be the first attempt at grafting on an animal body. The first attempt at skin-grafting was made in 1869 by M. Reverdin, at Paris, and the practice at the present time may be said to be an established one.]

For more than one year I have very extensively carried it out, and in most instances with success. I look upon the suggestion as a very valuable one, its adoption rendering many cases curable that were not so previously, facilitating the cure of as many more, and giving interest to a class of cases that had formerly but little. In the management of ulcers it is a great boon, and in the treatment of the large granulating surfaces so common after extensive burns its value cannot be over-estimated; as an adjuvant to many plastic operations, more particularly on the face, and in the case of deformities, it is invaluable.

Under the action originated by the transplanted skin centres, a process of repair goes on which at first appears almost magical; the grafts soon become islets of skin, round which cicatrization proceeds; the margin of the sore receives an impulse in cicatrization, which rapidly extends; and between the grafts themselves and margin of the sore connecting links of new skin rapidly form, which divide the sore into sections. By these means large surfaces speedily cicatrize that under former circumstances would have required many months, and that, too, without the contractions and subsequent deformities that under other conditions were too well known to follow in such cases.

The practice seems applicable wherever a large granulating surface exists, and the only essential point to observe is that the *surface of the sore should be healthy;* this clinical fact including another, that the patient's health is good, for there is no better barometer of health than a sore, its surface assuming a healthy or unhealthy appearance with every alteration in the general condition of the body. I have attempted, by way of experiment, to graft skin upon sores that were not quite healthy, and have sometimes succeeded; in an indolent sore, in which a small patch of healthy granulations sprang up, I have succeeded in securing, by transplanting, a new centre of "cutification," that proved of great value in aiding the healing process, but in a larger number I have failed in securing such an end. It may, therefore, be accepted as a truth *that a healthy granulating surface is an important requisite for success.*

Upon this basis I will now proceed to consider how the operation is to be performed.

Pollock tells us that Reverdin's method is to remove a very minute portion of the skin, and to replace it on the surface of the granulations, and there retain it with a strip of plaster. "I," he writes, "have usually removed the skin by nipping up a very small portion with a fine pair of forceps, and cutting it off close with sharp scissors. At first I made a slight cut in the surface of the granulations, and then imbedded the piece of skin; but of late I have only laid it on the surface of the ulcer. I cannot say that I have found any difference in the result. I do not think there is great, if any, advantage to be gained by the transplantation of a large piece, but where the ulcer is large, I think much is gained by the transplantation of numerous small pieces. The disadvantage of transplanting a large piece is the sore it creates; while the small sores formed by the removal of the minute pieces heal in a short time, and without trouble." (Clin. Soc. Trans., vol. iv.)

My own experience confirms in every point that of Pollock.

I employ for the removal of the sound skin a pair of scissors which Messrs. Khrone made for me after Macleod's suggestion in a medical journal, and find they answer far better than anything else, for they take away a portion of skin that will cut into three or four pieces, and the section includes only the upper layer of the true skin, with the rete mucosum; they do not cut into the fat beneath the skin, or divide the papillæ sufficiently far to draw blood or give pain. Patients never object, with this instrument, for a second or third piece

be taken away, should it be deemed necessary. I generally take the skin
om the forepart of the arm or the side of the thorax.

Having taken away the skin, the fragment should be cut into three, four, or
ore pieces, and then placed about *half an inch or three quarters of an inch*
om the margin of the sore, and about one inch apart. There is no doubt
at the engrafted centre has a stronger influence in exciting a healing action in
e margin of the sore when placed near it, than when isolated in the centre of
granulating surface away from the margin.

These pieces should be placed upon the granulations, and gently pressed in.
iere is no necessity to wound the granulating surface. They should be
vered with a piece of oiled gutta-percha skin, and the whole supported with
tton wool; a bandage being subsequently applied, so as to press moderately
on the part, and keep the dressing in position. On the third day, but not
fore, the dressings may be removed, the greatest care being needed; a fresh
ece of oiled gutta-percha skin being subsequently applied.

The appearances of the engrafted pieces on the removal of the first dressing
ry considerably; at times they will seem palpably to have taken root and to
alive; at others to have disappeared altogether; whilst in a third class the
rface of the cuticle will be seen floating, as a thin film, upon the secretion of
e sore, the basement membrane of the cuticle—the essential part—being
t.

Under all these circumstances, however, the surface of the sore is to be
aned with the greatest care, a stream of tepid water, either squeezed from a
onge or injected from a syringe over the part, being the best means to em-
y. The surface is on no account to be wiped, for the grafted portions of
in are easily uprooted, whilst those that appear to have died or that have
sappeared often reappear at a later period as "cutifying centres." As soon
the new centres are established in large sores, other pieces should be en-
afted, at about the same distance from the new pieces as these were origin-
ly inserted from the margin of the sore; and in this way the whole granu-
ing surface may be speedily covered with new skin, and a rapid recovery
ke place.

How the engrafted pieces act in the healing process is not yet satisfactorily
tled, and Reverdin's questions, as already quoted, have not yet been com-
etely answered. That they act as direct stimulants to the sore itself, and
re particularly to the margin of the sore, there can be little doubt, for as
on as the "grafts" have taken, the margin of the sore nearest to them is
n to cicatrize and to send out prolongations of new cicatrizing tissue to meet
ilar prolongations from the new cutifying centres; the sore in this way
coming subdivided by bands into smaller sores, and then rapidly healing.

That the engrafted portions grow by the proliferation of their own cells is
ewise proved by the fact that in the case of a white man upon whose
erated leg I engrafted four small pieces of black skin, the whole being no
ger than a barley-corn, the black skin grew twentyfold in ten weeks, the
ck portions gradually enlarging and sending out prolongations, which joined,
one patch of black skin had formed, the ulcer healing as rapidly where the
ck skin was grafted, as where the white was placed. The engrafted pieces
new skin thus themselves grew by cell development, as well as exciting a
w skin-forming power in the granulations near which they were placed,
d at the margin of the sore; the process of repair by this operation includ-
g far more than Mr. Dobson believed when he asserted "that the act of
nsplantation is simply to insert a pattern of skin, and this pattern is im-
essed on the granulations to a certain but limited extent; it simply supplies
em with a proper model whereby they may fashion themselves." (Med.
mes, Oct. 29, 1870.)

That the new pieces of inserted skin may do this is highly probable, but
at they do what has been already described is undoubtedly true. They
w themselves, they stimulate the skin-forming powers of the margin of the
re, and exert at the same time a like action on the granulations around.

The same result may likewise take place when large pieces of skin are trans-
anted, whether removed from the patient or from the amputated limb of a
cond; the *dressers at Guy's* have frequently engrafted large pieces of skin,

half an inch square, removed from amputated limbs, the pieces on removal being dropped into warm water in their passage to the wards where they are applied. But there seems to be no advantage in adopting this practice in the majority of cases, a large proportion of cases failing. In one case the grafts took root and excited a healthy action in the margins of the sore; they, however, grew but little; they remained, on the cicatrization of the sore, as bosses of skin with well-marked borders; they were grafted, it was true, but the grafts seemed to have no power of assimilating themselves with the tissues on which they were placed.

To take large pieces of skin from the patient's own body is an objectionable practice, on account of the large wound it creates; it is, moreover, unnecessary in the majority of cases, as small pieces appear to do better. To take them from another subject is also objectionable for like reasons; but still more so on account of the difficulty that sometimes accrues from a want of power in the new graft to assimilate itself to the tissues on which it is placed, and from the risk that is necessarily run of introducing into the blood of the living subject some new or poisonous element; a risk that I believe it to be wrong and unjustifiable to run, and that I would not allow on my own person. On that account I have forbidden my dressers adopting the practice.

In the case where black skin was transplanted I did it with the full concurrence of both patients; indeed, both were rather disappointed that the operation could not be repeated. They were firm friends, and the link I formed bound them closer.

There seems, however, no objection to mincing the portion of integument which is to be employed into minute fragments, that is, into pieces the size of millet-seeds; the thumb-nail of the surgeon being the best table for the purpose; in children, where it is unadvisable to remove much healthy skin, and the surface to be covered is large, the plan is a good one; I prefer, however, pieces the size of half a hemp-seed, when they can be obtained. Recently, the practice of applying "skin dust," or the products obtained by scraping the skin, has found favour, and met with some success. In the few cases in which I have tried it I cannot say I have been successful; the ulcers healed, it was true, but I have no evidence to show that any new cutifying centres were established by the proceeding. In the hands of Mr. Poland it has, however, been of value. I believe the idea originated with M. Marc Sée, of Paris, who called it "epidermic grafting." I might add, as an objection to the practice, that the scraping of healthy skin is far from painless, and to take dead cuticle is useless.

The clinical points alluded to in this paper are well seen in the drawings annexed. They have been carefully and ably taken by Mr. Clarke, my reporter, from the living patient, and from some excellent models lately made by our well-known artist, Mr. Towne, from patients under my care in the wards of Guy's Hospital.

They pretend, however, to show only the most striking features in this process of repair, the endless variations in the process requiring the close inspection of many cases.

Whether this newly engrafted skin possesses the same power of resisting disintegrating changes as the old skin is not yet proved. Some observations I have made lead me to suspect that it is somewhat prone to break down and ulcerate, on the patient walking, after the sore has completely healed, particularly when the subject is past middle age; they are enough to show the necessity of observing as much care in the after-treatment of the case as ought always to be observed after the cure of any other sore; that moderate support and protection are most valuable; and for this purpose there is nothing better than the binding on of a piece of sheet lead over the cicatrix when the seat of mischief is on the leg, as by it equal pressure is supplied to the seat of the original sore, as well as protection.

The new skin soon becomes as sensitive as the old; indeed, the sensibility of the cicatrix, under these circumstances, seems to be greater than it is when unaided cicatrization is allowed to take place.—*Guy's Hospital Reports*, 1872, *p. 237.*

51.—CASE OF SPINA BIFIDA TREATED ANTISEPTICALLY.

By Dr. John Wilson, Physician to the Glasgow University Lying-in-Hospital.

[The infant was a fortnight old, and the tumour the size of half a billiard ball, with the usual semi-transparent gelatinous aspect.]

We resolved, while keeping in view the risks incurred by all modes of treatment of such tumours hitherto reported, to make a free incision, under an antiseptic veil, and to dress it subsequently with great care, antiseptically. By the free incision, instead of small punctures which might soon close, we hoped to prevent the possibility of the cavity being even partially distended in future, while at the same time by the antiseptic, to obviate all danger of inflammatory disturbance of the chord or its membranes; the ultimate object being to secure complete obliteration of the cavity and the formation of a firm pad over the void in the vertebral column. The plan was duly carried out, and with gratifying results. By the end of the first month from date of operation, a flattened cake of the condensed tissues had formed, which has remained firm during the four months since then. The details of the case are these.

1871, May 22nd. A. I., aged two weeks. A well-formed, healthy boy. Spina bifida tumour, about size of half a billiard ball, in region of upper dorsal vertebræ, soft semitransparent, covered by thin membrane. Integument surrounding base of tumour extends up on its sides a short distance, then abruptly gives place to thin membrane. Fluid contents begin to ooze on very slight handling through two or three superficial punctures made by midwife. Infant evidently suffers pain whenever tumour is touched. Walls are felt to be thicker than they appear to the eye. They probably consist of many tissues more or less altered or incomplete, *e. g.*, arachnoid, dura mater, muscular fasciculi, fasciæ and integument.

25th. Tumour first moistened over with carbolized oil (1 in 8), and then opened by free longitudinal incision (about three quarters of an inch in length) under an antiseptic veil of surgeon's lint soaked in carbolized oil. Copious flow of serous fluid (cerebro-spinal) slightly tinged with blood, probably from the incision. Simultaneously with the removal of the lint, a large piece of carbolized lac plaster was applied, and over this another, kept in position by adhesive plaster, one edge being left comparatively free for escape of fluid. A soft folded handkerchief laid over this completed the dressing.

26th. Child has been restless, partly covered with a rash especially over nates. Has a sickly appearance. Anterior fontanelle depressed. Does not take to the breast as heartily as formerly. Has been vomiting considerably. Urine pale violet colour. Simultaneously removed lac and applied veil, pressure over which caused some escape of fluid. Veil then supplanted by fresh lac and folded handkerchief as before.

27th. Same mode of dressing. Tumour decidedly more solid—little or no fluid could be squeezed out. Child vomiting but not so frequently. Rash gone, except on nates. Urine still cloudy.

28th. Dressed as before. Tumour contracting. Some slight fibrous-looking threads seen extending from incision—exposure even for a moment to examine them, altogether unsafe. Some vomiting, with diarrhœa; slight depression and sleeplessness. Three grains of chloral in syrup every eight hours, and an occasional teaspoonful of toddy.

31st. Child much improved. Vomiting entirely ceased. Rests quietly. Wound dressed as before. On the lac was found the fibrous-looking material in the incision at last dressing. It seemed to be some loose areolar tissue.

June 2nd. Dressings have remained in good position. Very little discharge, but incision still patent. Tumour not much further lessened. Restlessness, and some more vomiting and purging. Repeat chloral, and give chalk mixture. Three layers of lac applied, to remain for three days.

5th. On removal of lac, slight sour odour could be noticed on it, but no other evidence of decomposition. Dressing re-applied as before. Child still vomiting occasionally and purging frequently. Ordered tincture of catechu, with the chalk mixture.

7th. Vomiting and purging both lessened. No odour on lac.

9th. Purging much less frequent, and vomiting only occasional. Tumour much diminished. At last two dressings child has not cried during process of moving, changing clothes, plaster, &c. Its mother also says it is less sensitive to motion.

12th. Tumour continues about same. Diarrhœa better, but still continues. Ordered gallic acid with brandy.

15th. Diarrhœa much better.

18th. Dressed with antiseptic gauze, six plies, in place of lac. Child doing well.

21st. Gauze changed. Not a trace of discharge.

(About this time several hard nodules were discovered in the larger muscles of the arms and legs, and seeming to involve portions of these muscles themselves. They were painful to the touch and when the limbs were moved. The mother gently stroked them several times a day with oil, and they very gradually disappeared.)

26th. Gauze changed and tumour inspected, as there was still no trace of discharge. It was found much contracted and flattened and incision healed. Child lively and well.

July 24th. Since last date the mother of the child has managed the treatment, keeping a small piece of the gauze over surface of tumour, in case there should be any oozing. Tumour contracting. Still a small patch of thin membranaceous covering in centre, *but no fluid in sac.* Child well and thriving.

September 15th. Summoned to see child in consequence of an accident. A young girl had been allowed to carry the infant out of doors for a little, when they were knocked down on the pavement by a large dog, and the infant fell out of her arms on its head. Found the whole head much swollen; anterior fontanelle distended; face very pale, as well as general surface of body; disinclination to remain for any time at the breast; distressed crying and at times a degree of torpor. Back, however, had apparently received no injury. Ordered evaporating lotion to head, and sp. æth. nit. in cold water, at intervals. (The child gradually recovered from this shock; and the head seems now, 19th Oct., to have regained its natural proportions.)

30th. Sac obliterated, and external tumour diminished considerably since last note.

October 19th. Child thriving, and very lively. Strong in back and limbs. Flattened disc, representing tumour still contracting. Small area in centre, about size of half a sixpenny piece, still covered by thin membrane. Gap in vertebral arch is felt to be nearly, if not quite closed, as if by cartilage. Child about to be vaccinated, being nearly six months old.

It is well-known that spina bifida tumours when left to themselves, almost invariably take some such course as the following,—they gradually increase in size, and their walls get thinner and thinner; if they be not ruptured accidentally—which often happens—superficial ulceration takes place, which ends in an opening by which the cerebro-spinal fluid escapes more or less rapidly; air is admitted into the sac, and even to the cord itself; inflammatory and suppurative processes set in, which speedily prove fatal. A few cases are on record where the fatal issue has been averted by treatment for a longer or shorter period; but the methods employed seem in every instance to be fraught with danger. Sir Astley Cooper kept a lumbar tumour constantly down with a truss, which was worn by the boy at school, where he "runs, jumps, and plays about as other children;" but when the truss was removed, the tumour "soon becomes the size of a small orange." Another lumbar tumour Sir Astley treated by successive punctures with a needle and constant pressure, so that it underwent adhesive inflammation, and became contracted and firm (Med. Chir. Trans., Vol. ii., p. 323, 326). The patient in that case was 28 years of age when we last heard of him. Mr. W. Martin Coates, of Salisbury, a few years ago, treated with success a lumbar spina bifida tumour in the following way, —chloroform being administered, he withdrew by a Wood's syringe a small *quantity of* the serous fluid, replacing it by an equal quantity of a solution of *iodine* and iodide of potassium. The tumour thereafter gradually solidified.

At the time he reports the case the child "runs about as vigorously as other children of her age." Last year, Dr. Lloyd Roberts, of Manchester, brought before the Obstetric Society of London two cases treated by passing setons through base of tumour. With regard to the first of these—"In a few hours the child became convulsed, and died on the evening of the next day." The second child was alive (2½ months old) at date of report; but pus was continuing to be discharged through the small openings by the side of the ligature (Obs. Trans., Vol. xii). Keeping these several methods in view, I have already sufficiently indicated our reasons for preferring the antiseptic method with free incision, and in the meantime, at least, the condition of the infant is satisfactory enough to warrant a similar procedure in like circumstances. In addition, it might be advisable to give chloroform, so as to lessen as much as possible the nervous shock undoubtedly caused by the incision and the sudden flow of cerebro-spinal fluid.—*Glasgow Medical Journal*, *Nov.* 1871, *p.* 1.

52.—THE USE OF SETONS IN THE TREATMENT OF STRUMOUS DISEASES.

By Dr. Edward Crossman, Hambrook.

Few writers on strumous diseases fail to mention setons or issues among the various remedial measures; but mention is made of them in a cursory manner, and without that stress being laid upon their employment which in my experience, it deserves. Moreover, they are generally described as applicable only to the early stage of these diseases, while in my opinion they are equally serviceable in the climax.

The wife of a farmer—one of an eminently strumous family—had during two years consulted various medical men, and, immediately before consulting me, had received her death-warrant from a leading physician. Her left lung contained a large cavity; the right was universally crepitant; and she had had several attacks of hæmoptysis. I was not consulted with any hope of cure; but, as she had come home to die, I was called in to watch her end. In this case, from the time when I put a seton in her chest, she began to mend. One set of silks having cut their way out, I put in another; and for six months a continuous discharge was kept up from the skin, with so happy a result that, fifteen months latter, I attended her in a confinement, and she has lived six years since.

A young woman, aged 19, presented herself to me, with extensive tubercular disease of both lungs, which had been running a constantly progressive course for more than two years. She was extremely emaciated, had profuse perspirations, and her legs were œdematous. Her case was one of advanced tubercular bronchitis. I put a seton of six silks in her chest, and at the end of a fortnight the symptoms began to abate. She wore the seton three months, by which time her cough had nearly ceased, and her expectoration was *nil*. She then pressed me to remove the seton, and, upon inquiring the reason of her urgency, she informed me that her banns had been twice asked, and she intended to be married the following week. She *was* married to a worthless fellow, who used her very badly; and, after an interval of eighteen months, I was called on to attend her in an attack of acute tubercular pneumonia, under which she soon sank.

I will not weary you with the details of cases. My memoranda supply me with the records of thirty-five cases of confirmed phthisis in various stages, in thirty-two of which marked benefit was derived from the employment of setons, while in three the seton was removed without benefit. In none of these cases had I reason to regret the introduction of the seton: emaciation and debility were not increased by the discharge from the skin, but, on the contrary, their progress was arrested *pari passu* with the diminished discharge from the lungs.

Passing to another variety of strumous disease—*Strumous ophthalmia*—an issue or seton in the temple is the most effectual remedy, and often the only means of stopping the progress of the disease. Probably in the present da setons and issues are used more often in the treatment of strumous ophthalm than in that of any other disease; and I take it that it is so because the remedial power can, in the eye, be actually seen and appreciated.

It is not only in the true strumous ophthalmia, attended with corneal ulce tion, that setons are of service—they are equally so in all those minor af tions of the eye and its appendages which take their origin in a strumous thesis.

For the relief of an eye affection, the seton is usually placed in the temple; but a serious drawback to its use in this situation is the scar which it leaves. I find that, placed in the arm or any convenient part of the body, it has an equally good effect.

Some ten years ago, a girl was under my treatment for many months in consequence of frequently repeated attacks of strumous corneitis. Having failed to eradicate the constitutional tendency by medicines, I put a seton in each temple. The result was a complete cessation of the disease; but, having to earn her bread as a nursemaid, she was naturally anxious to get rid of a disfigurement which prevented her from taking a situation, and, contrary to my advice, removed the setons at the end of a month. Not many weeks afterwards, exposure to cold wind brought back the disease, which again assumed a remittent character. Being in a situation at a distance, it was only when compelled to return home that she again became my patient. I then introduced a seton of six silks into her arm, and kept it discharging the whole winter, during which time she was entirely free from the eye affection, and has never had a serious return since.

A young gentleman seldom passed a month or six weeks for several years without an attack of strumous conjunctivitis. Being first at a private school, then at Eton, and latterly in London, he had been subjected to various plans of treatment. The attacks yielded sometimes to leeching, sometimes to blistering, and at other times to sedative applications; but exposure to cold wind or damp air always induced a recurrence. At last I persuaded his friends to allow me to put a seton in his arm. The suppuration was so severe at first that I had great difficulty in inducing him to retain it; he did so, however, and by degrees the fungoid granulations which had sprung up round the points of insertion disappeared, and a healthy suppuration was established. From the time the seton was inserted the ophthalmic attacks ceased; during the three months he wore it they did not once recur; and for three years since he has had but the most trifling threatenings of his old malady.

Two years ago, a younger brother of the same family was threatened with the same affection. I immediately put a seton of one silk in his temple and stopped the disease, and he has had no recurrence.

A third form of strumous disease, in which I have used setons with great advantage, is tubercular meningitis. My attention was first directed to the beneficial effect of a discharging surface in this disease by a case which occurred to me, in company with the late Dr. Symonds, soon after I commenced practice. A child three years of age, apparently in the last stage of hydrocephalus, was accidentally scalded on the chest, and a large suppurating surface was the result. Dr. Symonds was fully persuaded that the case was hopeless, and was equally convinced, when the child recovered, that the extensive suppuration turned the balance in its favour. Since then I have treated a considerable number of cases of this disease by the introduction of a seton in the poll, and am fully persuaded that, when the attack is sufficiently prolonged, the establishment of a free discharge offers the best hope of cure.

There is yet another form of struma in which I have used setons with advantage—viz., chronic glandular enlargements. I have found obstinate enlargement of the axillary glands yield to a seton in the arm, or below the clavicle. A seton above the clavicle or in the arm will facilitate the removal of enlargement of the cervical glands; and not long since a most obstinate enlargement of the inguinal glands, which for many months resisted all treatment, eventually yielded to a large seton in the arm, kept discharging for eight weeks. *Lastly*, I may mention chronic strumous ulcers as curable, when other means *fail*, by a discharge established at a distant part of the body.

have avoided extending this paper by the narration of cases, and have
itted the mention of any concomitant treatment. I would not have it sup-
ed on this account that I advocate the use of setons to the exclusion of
er remedies. I apprehend the part the seton plays is that of evacuant—dis-
ing, as it were, from the blood the morbid material. It remains to check
reproduction of this material by catalytic remedies.—*British Medical Jour-*
l, Dec. 16, 1871, *p.* 698.

ORGANS OF CIRCULATION.

.—NOTES OF CLINICAL LECTURE ON TORSION, AND ON THE DRESSING OF WOUNDS.

By George W. Callender, Esq., Surgeon to St. Bartholomew's Hospital.

n seven important amputations which occurred in Mr. Callender's wards,
ring three weeks in January, all the arteries (three of which were the femo-
l) were closed by torsion.]

There is, of course, no absolute novelty in the use of torsion, for quite re-
ntly Mr. Cooper Forster has placed before the Clinical Society evidence in its
vour; but it is strange that this method of arresting bleeding from a divided
ssel should have received so little attention since its first introduction by
nussat. After some trial had been made of it, it quickly fell into disuse,
iefly because Dupuytren reported that it was not safe in the case of large
teries; and now, although attention has been recalled to the operation, its
ployment is a rare exception.

And yet it admirably fulfils the purpose for which it is practised. Thus the
ry idea of torsion comes from observation of the way in which Nature her-
lf closes a torn artery. The practice of it insures the laceration of two coats
the vessel and the formation of a coagulum, whilst (in this respect unlike
e ligature) no foreign body is left attached to the artery, to be separated from
eventually by ulceration through the outer tunic. The twist of the artery,
e tearing of the inner and middle coats, and the formation of a clot—these
ree—provide for the permanent occlusion of the vessel. There is no record,
here the operation has been properly practised, of any sloughing of the twist-
end, or of any abscess along the track of the vessel; and, whilst the pre-
nce of a foreign body in the wound is avoided, the patient escapes the anxie-
which the prospect of the removal of ligatures entails. And to add one
ter, and this a strong argument, in favour of torsion, it is free from all risk
that secondary bleeding which is sometimes associated with the separation
a ligature.

When employed some forty years ago, various methods were used. By some
end of the vessel was twisted off; by others it was advised to hold the ves-
transversely a short distance from its cut extremity with one pair of for-
s, whilst the free end was twisted with a second pair. I recommend you to
neither of these two. By twisting off the end you lose that fraction of
urity which results from leaving the unbroken twist of the external coat
ond the line of the rent in the inner; whilst by fixing the vessel transversely
th forceps you needlessly bruise its tissues—that is, if the vessel be held
nly enough to enable you to twist against the resistance. A third method,
ginally known as "torsion libre," is that which you have often seen em-
yed; it consists in seizing the end of the vessel with any blunt artery-for-
ps—those which close with a spring being most convenient in practice; then,
th the artery as the centre, the handle-end of the forceps is made to describe
wide a circle as possible, slowly twisting the vessel in its rounds. For a
all artery the circle described may be small and the twisting rapid; but for
artery of large size the success of the operation is insured by the slow rota-
n *of which I speak.* The artery alone—free, that is to say, from surround-

ing structures—must be held by the forceps; and then, as the twisting progresses, you should look for a diminution in the resistance offered by the vessel.

At first, supposing the femoral to be the subject of the operation, on releasing your hold of the forceps they will be turned quickly around by the untwisting of the artery, but after four or five turns of the handle you will notice that this tendency to untwist is much lessened; and I may here add that it is well not to carry the twisting to the point of entirely overcoming the recoil of the vessel. You will then direct your assistant to sponge the end of the artery, so that, on loosening the forceps, you may be able to recognise the smallest escape of blood. If there be none, torsion is completed; if there be slight oozing, it will be necessary to reapply the forceps and to give the vessel a further twist. According to the condition of the arterial coats the amount of torsion necessarily varies, and in atheromatous vessels the coats often give way very quickly; but in the case of an old man with atheromatous arteries, on whom amputation at the fore-arm was performed, Mr. Morrant Baker informs me that he had no difficulty in securing the vessels by carefully applied torsion.

When you look at a femoral artery which has been twisted, you will be surprised to see how small is the portion of the vessel included in the knot-like twist—not above one quarter of an inch of its length has been disturbed. There is no considerable separation from the sheath or from surrounding structures; and from the number of wounds which heal by the first intention after torsion, there is every reason to believe that the life of the twisted extremity is not spoiled. Sometimes the appearance of the twist is modified by a little swelling, as if from blood which had escaped between the tunics of the artery; but I never saw this assume any but the smallest proportions, and its occurrence is of no consequence.

I have already told you that after torsion there is no risk from secondary bleeding. If the artery have not been properly secured, it is, however, possible (although I am not aware of any instances of its having happened since the revival of torsion) that it may untwist before the formation of a clot and adhesion between the divided coats have permanently secured it. Such untwisting must occur within the first few hours after an operation. Having myself no experience or fear of such a recurrent, as distinguished from secondary bleeding, I mention the possibility of its happening only to impress upon you the need of care in looking to the proper securing of the vessels at the time of the operation.

Speaking generally, I should say that wounds have healed far oftener by the first intention since I have used torsion than when other modes of arresting hemorrhage have been employed. All the amputations were healed in this way, or with a suppuration so small as to be referable to some corner only of the wound. So, too, with wounds from lesser operations, of which I have in mind a large and deep wound which resulted from the removal of a cancerous tumour from the wall of the abdomen, and several wounds from the removal of cancerous mammary glands.

The advantage of torsion was recently shown in an operation for the repair of a torn perinæum, when two considerable vessels were twisted, after bleeding of so troublesome a character as would otherwise have necessitated the use of ligatures, which I should have been reluctant to employ because they must have been left to traverse and perhaps irritate a wound which it was most important should be united by the first intention.

There are on the other hand, some circumstances under which it is almost impossible to use torsion. In the case of an excision of the knee-joint, some of you may have noticed that I could not twist the arteries which bled from the fibrous tissue on either side of the articulation. The vessels were held so closely in this tissue that either they could not be seized by the forceps, or, if seized, could not be turned round amidst their dense surroundings, so they had to be tied.

When I refer to the better healing of wounds since torsion has been practised, I touch upon a subject which has at present great interest for us. In addition to the attention which Professor Tyndall has attracted to the subject

is observations upon the atmospheric surroundings of wounds, Dr. Burdon
lerson, in a very instructive paper, has brought to our notice the presence
rater, such as our wounds are constantly cleansed with, of bacteria or
ozymes, as being a probable cause of putrefactive changes; and certainly
well that we should be reminded of such possible dangers. It is remark-
how rapidly as a rule wounds are repaired in our wards. When we read
ear of the care taken over the wound, for example, of a compound fracture,
ecall the fact that the loss of a patient from compound fracture, after the
k of the accident has been recovered from, is, in our experience, an excep-
al occurrence. Thus during the last two years, of fourteen cases of com-
nd fracture which have been treated in Kenton, Harley, and Lawrence
ds, by Sir James Paget or by myself, thirteen have recovered—the one
l case having been managed by the antiseptic method; and I attribute this
lt, which is an illustration of the general well-doing of wounds in our
ds, to the care which has always been taken of their first dressing,
to the avoidance of all but absolutely necessary disturbance of them.

here are some details which have to be considered in the making of
nds if we are to secure their healing by primary union. The incision, for
mple, should be clean, leaving, so far as is practicable, a smooth even sur-
a. Thus in making a circular division of the muscles of the thigh in an am-
ation, one sweep of the knife suffices to divide all structures down to the
iosteum. The surface of the wound should be handled tenderly—no rough
nging or washing of it should be permitted. All bleeding must be carefully
nched. In the case of a plastic operation for the cure of a torn perinæum,
will almost certainly fail unless you make a point of thus absolutely stop-
g all hemorrhage and of removing all clots, before you bring together the
faces of the wound: and what is so well known of this operation holds good
all others. The time, too, which is occupied in the securing of vessels is not
t, for it permits that glazing over of the wound which indicates the moment
st favourable for closing it, and which is the first step towards repair.

n the case of an amputation, the edges of the wound are brought together
h silver sutures, inserted close to one another, so as accurately to oppose
cut margins; the stump is then covered with lint, once folded, and pre-
usly dipped in carbolic oil of the strength of about 1 in 30. Pads of lint or
cotton-wool are then applied above and below, so as to envelope the wound,
l the whole dressing is secured with a few turns of a bandage. When the
ient is placed in bed the stump is supported on pillows in the position which
es him greatest ease, and, if necessary, is steadied by means of a broad ban-
ge passed over it and secured to the bed.

You will notice that I follow a pretty uniform plan in the first dressing of
unds, of which the principles are, first, the leaving of the surfaces to Na-
e, after carefully arresting bleeding and after placing them in apposition;
ondly, thorough covering up of the wound with lint or with cotton-wool.
to carbolic oil, or simple olive oil, which I sometimes employ, it is used as
oft and supple application to the flesh. The late Mr. Stanley was in the
it of using in this way, as a local application (I believe at the suggestion of
Gatty), the balsam of Peru; and whilst Mr. Edlin was house-surgeon in
2, Sir James Paget began, and afterwards continued, the use of carbolic oil
cleanly dressing for amputation and other wounds. Intermediately Dr.
nhouse had recommended to us the use of powdered charcoal; and this was
ployed between layers of cotton-wool, with the view of keeping wounds
et and free from putrefactive changes. Whether the balsam or the carbo-
oil or the powdered charcoal was in repute, its use was combined with the
ering of the wound with lint or with cotton-wool; and it cannot but interest
to observe how well this old-established practice fits in with the recommen-
ion, by Professor Tyndall, of cotton-wool as a means for protecting wounds
n the impurities of the air.

ollowing the progress of the case in the wards, you will observe, the next
, that no change has been made in the dressing, unless, in consequence of
patient's complaint of tightness of the bandage, the house-surgeon has cut
ough some of its folds. It is always necessary to pay careful attention to
h a *complaint, as the so doing* averts the restlessness which neglect of it is

sure to engender. And so, too, on the second or third day no change is required. On the fourth the bandage is cut through on either side, and the entire dressing, which has been kept soft with repeated applications of oil, is slipped off without trouble and without disturbing the stump; and we expect to find that the wound has healed, as it had healed in each of the seven cases of amputation of limbs.

There are cases in which, from a variety of causes, primary union cannot be attained: for instance, after such an operation as one which I performed quite recently for the removal of a recurrent colloid tumour from the inner side of the thigh. A large tract of skin was taken away, and the internal saphenous system of veins was destroyed. The very large wound which resulted was dressed in the manner I have described, and subsequently water-dressing, and occasionally poultices, were had recourse to. The process of repair was hindered in this case by the œdema which resulted from the hurt to the venous system; and it was some weeks before the external saphenous and the deep veins compensated for the loss of the internal saphenous vessels.—*British Medical Journal, Jan.* 20, 1872, *p.* 64.

54.—NOTES OF A CASE OF POPLITEAL ANEURISM TREATED BY COMPRESSION.

By G. E. Walker, Esq., Surgeon to the Liverpool Hospital for Skin Diseases, &c.

[The patient was a man of middle age who stated that about four hours previously his foot slipped in getting out of a railway carriage and he came with his whole weight (14 stone) on the toes, just resting on the station platform. There were the usual signs of a popliteal aneurism, the size of a small egg.]

In forty-eight hours, in spite of the use of tourniquet, digital pressure and flexion, the aneurism had increased in size so as to bulge out considerably beyond the lateral and posterior boundaries of the ham, and to extend 2½ inches above the adductor opening. The rapidity of the enlargement led me to believe that there had been rupture of the outer, as well as of the middle and inner coats of the artery, though if so the aperture must have been at first very small.

On the fourth day the aneurism and the treatment together had produced such distress as to induce me to request Mr. Stubbs to see the case with me, and to urge the propriety of ligaturing the superficial femoral. Mr. Stubbs' advice, however, was to continue the pressure. Up to this time we had endeavoured to keep up continuous pressure day and night, but owing to the bad state of health of our patient, superadded to the ordinary difficulties of the process, we had been unable to prevent the increase in size before mentioned. There was also great œdema of the whole limb, and much engorgement of the vessels leading to the popliteal vein. Much of this was, doubtless, due to the compression of the femoral vein, involved necessarily in that of the artery. The walls of the sac had, however, attained some degree of strength, so that after a six hours' rest, rendered absolutely necessary by the distress of the patient, the aneurism had not further increased, and much of the swelling of the limb had disappeared. We could now apply pressure only very partially, and for some weeks this was the plan. At bedtime the leg was bent on the thigh as far as could be comfortably borne. He could never tolerate for more than a few minutes such an angle as would ensure stoppage of pulsation; still, we were able to lessen pulsation slightly on some occasions for several hours. In the morning he was moved to a low iron bed, which had a framework at each end about 2½ feet high. Between these a half-inch manilla rope was tightly stretched, for the double purpose of supporting, and by its elasticity enabling us easily to vary the power of, the weight employed. The weight was made up in this fashion: a disc of brass, 1½ in. in diameter, had a stout pin 6 in. long screwed into its centre; leaden weights of various convenient shapes were threaded on this pin; to the bottom of the disc was glued an India-rubber hemispherical air-pad, covered with wash leather, and a stout string connected the top of the pin to the rope, the object of the whole being to maintain closure of the artery with the minimum amount of pressure. The weight directed diagonally against the edge of the pubic ramus could be borne with comfort for about four hours, but in practice we found objections to its use which led to its discontinuance. The slightest lateral movement on the part of the patient would throw it off, and such was the delicacy of the balancing that under a minute increase of blood pressure, as in coughing or sneezing, the weight was lifted, and pulsation caused. In the afternoon the patient was changed to a sofa, and had on a Signorini tourniquet, or controlled the pulsation by flexion, or still better by crossing the affected limb over the other and placing a tightly-rolled towel underneath the aneurism. This was the treatment carried out, with a few unimportant variations, for many weeks. A tourniquet, with elastic bands, though made by one of the best surgical mechanicians, proved utterly useless; the old horseshoe, with an air-pad, answered very fairly.

It will be observed that we were trusting to the intermittent pressure treatment so much extolled by the Dublin writers; and although we did not cure the aneurism by it, we lessened its size so considerably that on the 27th of March, when, owing, to an attack of acute rheumatism, we were compelled to refrain from all surgical treatment, the girth of the joint was only one inch more than that of the sound limb, the pulsating tumour being about 2½ in. in diameter. For the next fortnight the rheumatism afforded us sufficient occupation, and no attempt was made to control the aneurism, as, indeed, none could have been tolerated. When we again began to attack it, we found some little increase in size, and having now lost all faith in the three sorts of treatment we had tried, namely, intermittent pressure, incomplete pressure—that is endeavouring to shut off almost, but not quite, the whole current of blood to the sac, which I had attempted by using the delicately-balanced weight—and, lastly, the let-alone treatment to which we had been driven by the rheumatism, I tried to induce my patient to submit to continuous pressure. Such, however, was his dread of the repetition of the torture which he endured in the first four days of his illness that, though endowed with exceptional powers of endurance, I could not persuade him to make the attempt until the 5th of May, when he consented.

For the first few days no alteration was made in the mode of applying pressure; it was merely made more continuous, but after that the delicately-balanced weight was laid aside, and a block tin weight of 14lbs., shaped like a rifle bullet, was substituted. It was kept in position by means of a rod screwed into its flat end and made to pass through a hole in the apex of an elliptical arch of steel springing from a hollow pad, on which the thigh rested. The hemispherical end was placed diagonally against the pubic ramus; it effectually controlled the artery, and had several pounds to spare for emergencies.

After sixteen days of this treatment I found the aneurism smaller, but no signs of its occlusion; it was soft, and there was a considerable space unfilled by clot. For reasons detailed below, I now, after procuring another weight, differing from the one already in use by being about some 2¼lbs., heavier, and having a more conical apex, so as to sink deeper into the soft parts, proposed to make pressure on one artery alone—the common femoral, alternating between the pubic ramus and just above the bifurcation of the vessel. We had even with improved appliances great difficulty in compressing the vessel; the groin was sore, the glands were enlarged, and there was great pain down the course of the artery. Changes in the weights had to be made frequently; they were rendered more tolerable through being cooled by a freezing mixture, but it required all the untiring energy and patience of Mr. F. Young, who assisted me throughout the case, to keep up pressure at all, and in spite of every possible care, the horse-shoe tourniquet, in order to give a rest from the weights, had to be fixed on the superficial femoral several times—a course which, I believe, involved actual retrogression.

On the night of the 23rd, whilst being changed from a hard bed to a soft one, the patient extended his limb violently, throwing out of gear the tourniquet, which had been temporarily adjusted to permit the removal, and thereby caused the aneurism to pulsate fully as much as it had done three weeks be-ore. All our labour seemed, therefore, to have gone for nothing. Next mor ing showed no change, and at noon, being sick with disappointment, our tient wished to be left to die in peace. To allow him a little time to recover removed the weights and everything, and laid the limb in a comfortable p tion, with a cushion under the aneurism and a sand-bag over the knee to li the pulsation. He had then some food and wine, and, without being m aware of it, for he had a great objection to the drug, a grain of morphia. fell asleep at once, and in half-an-hour the weight was replaced on the gr in two hours he awoke much refreshed. The heavier weight was placed on the lower spot, and another grain of morphia administered. With the exception of a very few minutes, during which time he was in a sleep, almost comatose in character, I sat by him from one p.m. to eight p.m. with my fingers and thumb embracing the aneurism, so that I was perfectly certain no blood had passed beneath the weights, which were changed with the greatest care. Towards five o'clock I had the satisfaction of feeling the previously flaccid sac gradually fill out and harden, and at eight, when I left, matters appeared so promising that I ventured to prophesy, could pressure be kept up for another seven hours, the cure would be complete. At nine another dose of morphia—half a grain—was given, and again the patient slept. Towards midnight the nurse in changing the weights, thought the pulsation had stopped, and soon afterwards, on Mr. Young coming on watch, he established the fact conclusively. As matter of precaution, the pressure was kept up during the night; the patient, now that his six months' anxiety was relieved, bearing the physical discomfort with comparative ease.

The considerations which led me to alter the method of pressure were these. The Hunterian operation, as a rule, causes consolidation of the aneurism in a few hours; if then we could make compression act in the same way as ligation, we might expect similar good results. I therefore followed up the steps likely to take place after the application of a ligature to the superficial femoral, as being the artery in which I had an immediate interest, reasoning thus, the blood would pass in increased volume down the profunda and through its anastomoses with the superior articular arteries and the muscular branches of the popliteal, with the inferior articular, and with the anastomotica magna. By means of some of these vessels, according to the position of the aneurism, the blood would reach the diseased part of the vessel either from above or below, but, whatever the direction, the current would be at first a mere trickle, drop by drop, and as the coagulum already in the cavity is rough, there would be present the most favourable conditions for the sealing up the vessel, namely, a current not strong enough to make its way through the aneurism, and, opposed to such current, a rough surface highly provocative of coagulation.

Now to make compression act as ligation, it seemed that one had only to make it continuous. But as I had been doing this as well as possible for more than a fortnight without success, I felt there must be a condition unfulfilled of which I was yet unaware. As I had reasoned out the effect of the ligation, so I did of the compression method which I had used. Granting that my pressure for the time being was perfect, this would be the result. After, say four hours' (an extreme) closure of the artery at the groin, I should expect the blood about to seek a new channel by means of the anastomoses of the gluteal and circumflex iliac arteries above, with the various branches of the profunda below, and therefore, to reach the aneurism through the profunda—its ordinary current being reversed—and the superficial femoral. But before this new channel could be made available,—for it takes some time to make a collateral circulation, as I will show further on,—the pressure would be shifted from the groin to the mid-thigh, where the superficial femoral would be compressed, and the onus of forming a fresh channel transferred from the capillaries of the branches of the internal iliac, and the ascending offsets of the profunda, to the descending branches of the last-named vessel and its anastomoses, with the anastomotica magna and the muscular and articular branches of the popliteal. Now, supposing the change from one artery to another to be made too quickly to allow of the production of an efficient collateral channel for either artery, I was driven to the conclusion—always supposing the pressure was complete—

at it was possible to starve an aneurism, and therefore to increase its cavity. or I believe a process of absorption, similar to that which takes place after e consolidation of an aneurismal sac, is constantly going on during what may styled the life of an aneurism. The clot is a foreign body, which nature is ways engaged in removing. When, however, the blood has free access to the c, the deposition of new material takes place with greater rapidity than the moval of the old, and, therefore, the aneurism increases in size.

This explained what had puzzled me before, namely, the stationary condition the aneurism during the months of intermittent pressure, and the fact that, ter a fortnight's continuous pressure, though the aneurism was smaller it was fter, and a very short time of free access to it of the blood sufficed to render ulsation as vigorous as it was weeks before.

The next deduction was obvious. As I could not keep the weight on one ot long enough to produce the desired effect, I must, in changing it, so choose y second site as to keep up the formation of the one collateral circulation hich I had begun. I therefore used pressure only on that small portion of ssel bounded above by the inner margin of the pubic ramus, and below by e origin of the profunda. Treatment according in intention with this reason-g was begun effectively three days before the final act. I believe some of this ne was wasted, firstly, because many times the weight was raised to see the ndition of the aneurism, for it was impossible at once to convert patient and sistants to the necessity of obedience to the requirements of the theory; and condly, to the unfortunate accident by which two days' very hard labour was most completely spoiled. For the latter, I was responsible, as it was done hilst I was removing him from one bed into another with the well-intentioned ea of giving him a good night's sleep. Unhappily, I caused him so much pain at he extended the limb violently, and started the pulsation in all its old vi-ur. Doubtless by this time the new circulation was almost finished, for the ling and hardening of the sac began next day, very soon after pressure was ain efficiently used.

Such then are the considerations which led me to adopt the treatment by pres-re on one artery.

For the more easy carrying out of the pressure treatment, I would submit e following recommendations, which refer specially to the lower limb, but hich may be applied elsewhere.

The edge of the pubic ramus is the place where the crural artery is most easily mpressed. In the case above related, as well as in others, I have found, by tching the artery with the finger diagonally against the edge of the pubis, it uld be closed with less pain to the patient, and whilst the surgeon could keep pressure in this fashion for considerable time, he could only do so perpen-cularly to the bone for two or three minutes. The difference could be well timated with the weight; it required for perpendicular pressure some two or ree pounds more than for diagonal.

The groin ought to be well shaven and powdered very thickly; prepared ful-'s earth is the best powder.

But of far more importance is the cooling of the weight. For another case ould have the upper part of it deeply hollowed for the reception of a frigo-ic mixture. It was only by keeping the weights icy cold that we were able the case above related to use them at all in the final act, for crops of pustules re appearing with most annoying rapidity, and the glands were enlarged and inful.

Finally, as there is under the use of complete pressure a reasonable prospect quick cure, I would enjoin the free exhibition of opium or cannabis indica to ll sensibility, not necessarily to the extent of insensibility, but to prevent rry and fidgeting. Mental anxiety intensifies pain wonderfully. The pa-nt whose case is the text of these remarks, after his mind was set at rest by knowledge that his disease was cured, bore without a murmur for hours on ore skin the weight which twelve hours previously he could scarce tolerate enty minutes, and this too without opium.—*Liverpool Medical and Surgi-l Reports, Oct.* 1871, *p.* 66.

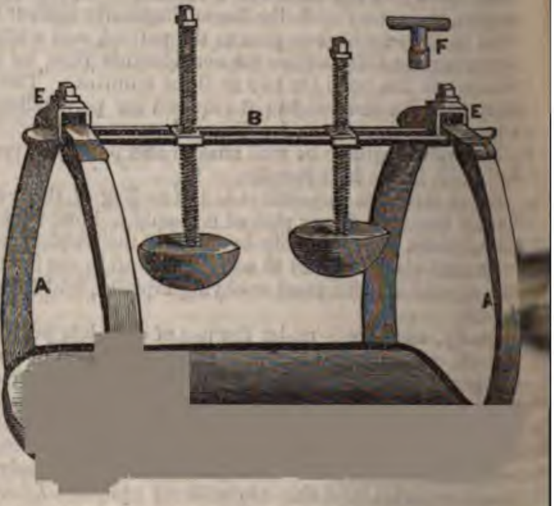
55.—A DOUBLE TOURNIQUET, WITH SHIFTING PRESSURE F THE TREATMENT OF POPLITEAL ANEURISM BY COMPRESSION.

By Francis A. Bulley, Esq., Consulting Surgeon to the Royal Berkshire Hospital, Reading.

Having on several occasions noticed the difficulty of accurately regulating the pressure upon the femoral artery in the treatment of popliteal aneurism by compression, I was induced to direct my attention to the action of the instruments commonly in use for the purpose, with a view of securing a more exact application of the required pressure, and at the same time insuring as much as possible the comfort of the patient during the occasionally protracted progress of the cure.

It may have been observed during the treatment by this method, that the single tourniquets usually employed, with the exception, perhaps, of the ingenious invention of Dr. Carte, cannot be made to maintain a fixed and immovable position upon the thigh, without screwing down the compressing pad upon the artery so tightly as completely to obliterate its tube, and in some cases to produce agonizing pain in the subjacent nerves, with great venous congestion and swelling of the limb below the point of pressure, thus frequently necessitating the removal of the instrument from the limb. I believe it is now generally admitted that the complete obstruction of the arterial stream is not only not required in the treatment of aneurism by pressure, but, on the contrary, is calculated to defeat the object for which it is designed. If, thus, no blood were allowed to pass through the sac, there could be no deposition and lamination of fibrine in its cavity; and it is only by the production of a languid, retarded stream through the conducting artery that the necessary deposit can be certainly secured. The instrument of which I have given a drawing is capable of producing a regular retardation of the arterial current, with comfort to the patient; and I may further observe that it is in accordance with Dr. Bellingham's recommendation upon the subject. "Compression by two or more instruments, one of which is alternately relaxed, is much more effectual than any single instrument, and in many cases the pressure can be maintained by the patient himself." (Dr. Bellingham on the Treatment of Aneurism, Corollary 5.)

The instrument is composed of two hinged steel hoops, A, united at the bottom by a padded iron plate, and above by a slotted bar, B, made movable at each end upon the horizontal portion of the hoop, to allow of its adaptation to either limb of the patient. The compressing pads are made to slide easily along the slot in the bar by means of screw-nuts. The rings can be opened for use or removal by means of a nut and screw at D, where, also, they may be made larger or smaller, as the size of the limb may require. The whole of the nuts and screws are regulated by a key, as at F. The instrument from which the drawing was taken was made by Messrs. Weiss and Co., of the Strand.—*Medical Times and Gazette*, Jan. 27, 1872, p. 95.



ALIMENTARY CANAL.

56.—ON STRANGULATED HERNIA.

ames Paget, Bart., F.R.S., Consulting Surgeon to St. Bartholomew's Hospital.

o on to speak of the guidance to be derived from the remoter symp- d, first, from the inaction of the bowels. This inaction, or rather, ance of expulsion, is a nearly constant sign in strangulated hernia, s in so many other cases that its sole presence is of little weight tives for operating. Its absence is, rather, that which needs study. more actions of the bowels, after other signs of strangulation have of no weight at all against the propriety of operating. They com- ur, because usually after strangulation the part of the bowel below icted part empties itself. These actions of the bowels are not to be n either side of the question about reducibility or operation; and gular and frequent action is not an absolute prohibition, for the ion may involve only omentum, or only a part of the circumference on of intestine. In these conditions, fæces may pass along the canal charged.

see a lady for what was considered an abscess in her groin. The ras just over the femoral ring, and contained fluid and air. I punc- nd let out pus and air and liquid fæces, and presently I found a piece sac sloughed off and lying in the abscess. This, and the history of proved that the hernia had, about a week previously, descended patient felt a sudden pain while driving. The hernia was so small s overlooked; its pain and the griping which it caused were thought ic, and were so treated. The bowels acted sufficiently, and gradually ernial sac and its enclosed piece of the intestinal wall died and off. Then came the signs of abscess over the femoral ring, and its d this was followed by complete healing and many years of health. r, such cases as this are very rare; and you may hold by the general you should not operate when the bowels act frequently or regularly, the other signs of strangulation, both local and remote, be well

te of the abdomen is as little or less decisive in cases in which you doubt. Of course, if it be sensitive and tender on pressure, either e or in parts near the hernia, still more if it be distended and the ard, this is an addition to the reasons for operating, and, I may add, sons for fearing that you may be too late. It is more to be remem- t, when the abdomen is not tense or tender, when it may even feel tural, yet you must operate if other signs of strangulation be present, ernia cannot be put back. For the changes of the abdomen are not ear the beginning of strangulation, and to wait for them would often till too late.

e asked which of the signs of strangulation I would most rely on as ng the operation, I should certainly say the vomiting. Time after the other signs were feebly, if at all, marked, the vomiting has been t guide to a timely operation. Many times, when all else was so t it seemed rash to operate, the vomiting proved that it would have h more rash to wait; and not one instance can I find in my notes in lect of the import of vomiting was not proved to be unwise. You hold the rule complete, that, when a patient has a hernia, recently reducible, and, with this, vomiting which cannot be clearly assigned ing independent of the hernia, you should operate. I am obliged to tly become irreducible, because a patient with an old irreducible y vomit, as any one else may, without any reason for suspecting tion; though in even these old cases you must be very watchful lest *ing be an early sign* of strangulation. But the rule is safe that recent irreducibility and vomiting are enough to justify the operation, even tho there be no other signs of strangulation present. Much more must the opera be deemed necessary when with these the other signs of strangulatio, i even slight degrees, coincide.

And in thus judging of the vomiting, do not be too scrupulous as to its manner or its products. There are indeed some notable modes of vomiting when intestine is strangulated. The patient vomits all he drinks, and that soon after taking it; and besides, he vomits fluids of his own secreting; and this vomiting is commonly (at least in the later stages), with gushes of large quantities of fluid, without much retching or violence, as if the stomach slowly became full of its own secretions and of those of the upper part of the intestine, and then, without any preceding nausea, suddenly emptied itself.

When you see these kinds of vomiting with a hernia lately become irreducible, or even with any swelling that may be a hernia, you may be sure that you must operate. But do not wait for any supposed characteristic mode of vomiting; do not be misguided by the absence of some peculiar fluid; nay, do not be misguided by the absence of all vomiting; for I have known it absent in the case of a very large hernia, which was certainly strangulated, and on which I operated successfully. And do not be misguided by an apparent diminution in the severity or in the frequency of vomiting, or by the vomiting having begun as soon as the hernia descended; for this it often does. Any kind of vomiting, if it be repeated, is enough to justify operation in a hernia recently become irreducible. Let me tell an illustrative case, which taught me the more, because it occurred in one whose life was of great value. He was aged 56, weakly, but laborious in literature. For three days after the descent of a hernia, which could not be reduced as it usually had been, there was not a single sign of strangulation, except this irreducibility, constipation, and occasional vomiting. He had no pain in or near the hernia; no feeling or aspect of illness; no hardness or tension of the sac; and the vomiting was only occasional, and there were often many hours of interval. But after the three days, abdominal pain suddenly set in, with coldness and faintness, and wretchedness. Within two hours I operated; but it was too late; peritonitis had already begun, and the operation was useless or worse than useless. He died in twenty-four hours.

Cases like this are frequent. All seems pretty well; and then comes an inrush of indomitable symptoms. The hernia is not acutely inflamed; the patient is not greatly distressed; he flatters himself that he is better, and the similar flattery of his friends is yet stronger: all are averse from operation, and you can hardly persuade yourself to be resolute about it. But there should be no hesitation. I lost the chance of saving this man's life, by underestimating the importance of occasional vomiting as a signal for operation. If you lose such chances, you will be still more blameworthy; for you will have had more warning than ever I had.

And once more; do not be deceived by the cessation of vomiting in the extreme condition of strangulated hernia. This sometimes happens; but it is a token of evil rather than of good, if general improvement do not coincide with the cessation of vomiting. So, again, sickness may be stopped by narcotism; but here again there is no evidence of such general improvement as might justify waiting.

In the recent stages of strangulation, if it be not very acute, the respiration and pulse are little affected. The pulse is usually accelerated, and at first may be full and firm. I find that it was between 80 and 90 in a large majority of the ordinary cases which I have recorded; and the respirations are, generally, I think, in due proportion to the pulse. As the other signs of strangulation become more marked, these, I think, always coincide with them. The pulse usually becomes quicker, feebler, smaller, unless indeed after the warm bath, when its strength and size may be greatly increased; the respiration, I think, keeps pace with the pulse.

Thus, these signs corroborate the others in urging to an operation. I have *not any notes* or knowledge of cases in which the pulse or respiration was so *inconsistent* with other signs of strangulation as either to justify or to forbid *the operation*. But you may have this for a safe rule; that if, while you are ıtching a case, doubtful whether there be strangulation, the pulse and breath- g should increase in frequency, you may believe that there is a commensurate crease in the reasons for operating.

Lastly, as to the patient's aspect and general condition, little that is definite n be said. You read of an anxious expression, and it is called characteristic, many other fallacious things are. I should rather call it an expression of stress or of misery; but, however you may name it, be careful not to think at you must see it before judging that it is right to operate for a strangulat- hernia. The worse a patient looks or feels; the more he looks shrunken, rn, and old-aged; the more miserable his sensations; the more is it unlikely it his strangulated hernia will be reduced without operation. But the re- se is not true. I have operated with full right, as proved by the result, on ients who neither looked nor felt miserable or anxious in any sense of the rd. And I have seen a patient looking well and tranquil, in whom a femoral nia, after seven days' strangulation, contained completely gangrenous intes- e.—*Brit. Med. Jour., April* 13, 1872, *p.* 389.

—ON THE REDUCTION OF UMBILICAL AND OTHER HERNIÆ.

By J. Cooper Forster, Esq., Surgeon to Guy's Hospital.

ıe following is from a lecture on umbilical hernia.]

ı golden rule for practice I believe to be this: never attempt to reduce a nia except with the patient under the influence of chloroform. I believe e harm is done by violent taxis when the patient is straining than by letting bowel alone. Strong language this, but I repeat that so much damage is e to the intestine by those who know nothing of the injury they inflict as ounterbalance much of the good which is unquestionably done when cases operated upon early. You had much better give an anæsthetic, and so do y with all chance of muscular effort, than push and squeeze, and push in, for one, three, ay, four hours, or several times during the evening—a ory of magnificent effort which has not unfrequently been narrated to me. ce, then, one decided attempt under chloroform; and if it fail, operate. You not hear this too often, because forcible taxis and delay in operating are the ıt causes of death of all cases in our hospital. First comes the private lical man—he tries perhaps several times; then the dresser; then the se-surgeon; and then the advanced student who may happen to be looking ; and, lastly, the surgeon is sent for, and he very wisely at once ope- s. I cannot put the case more forcibly than this—that, if I myself had a ture, no one should try to reduce it except the operator, with the proviso t if he failed after a very moderate attempt he should at once proceed with knife. I look upon the operation itself as one of the easiest, the safest, and plest of the severe procedures of surgery. Cases of umbilical hernia in ch operations have been performed are most fatal for this very reason, that nuch taxis and delay take place; and, aptly illustrating the converse of this, cases came under my care, not so very long ago, in which the medical gen- nan in attendance showed an amount of intelligence which was quite delight- I saw both these cases within four hours—one within two—of the de- ıt of the hernia, and in both I operated within an hour of seeing them, and y both got well—a very unusual thing in cases of umbilical rupture.—*Lan- Feb.* 3, 1872, *p.* 143.

—A PLAN FOR FACILITATING THE REDUCTION OF STRANGULATED HERNIA BY TAXIS.

By Dr. Philip Crampton Smyly, Surgeon to the Meath Hospital.

The objects to be attained in the treatment of hernia in a state of strangu- on, are the release of the protruded parts from stricture, and their replace- ıt within the *abdomen*, provided they are in a suitable condition. These objects are usually sought to be accomplished either by taxis or by operat with the knife."

Some years ago, a nurse in one of the medical wards in the Meath Hosp had a reducible femoral hernia. She neglected to wear a truss, and one it consequently became strangulated. My father, being the surgeon on d tried taxis, as did also the other surgeons, without success. After consulta an operation was decided on, but every argument failed to persuade the pa to submit—she would rather die than be cut. After the surgeons had left the clinical clerk (since a very distinguished medical officer in the army) and I thought it a good opportunity to study the relation of the ring to the sac. The result of our examination not a little surprised us. On withdrawing my finger from the ring into which I had inserted it, we heard a distinct gurgle. My fellow-student pressed the tumour, and it passed into the abdomen. The patient lived for many years afterwards, and performed her duties in the hospital. I have since frequently tried to repeat this happy manœuvre, and with most satisfactory results.

For inguinal hernia in the male, the index finger is applied to the lowest part of the scrotum. This is invaginated (as in Wützer's operation for radical cure), the finger being passed behind the testicle and cord up to the external ring. The hernial tumour is then pressed downwards over the finger towards the back of the hand, so as to make the structures in the ring tense, and consequently smaller. The invaginating finger is then forced firmly upwards and outwards in the direction of the internal ring. As soon as the finger is firmly grasped, the hand should be slightly turned, and the finger pushed towards the middle line. Considerable force may be safely applied in this way, as all the delicate structures are behind the finger, which acts mainly on the stricture. On withdrawing the finger, the hernia can usually be easily returned. The same principle is equally applicable to femoral hernia. This plan may have occurred to others; but if so, it is perhaps not generally known, and any suggestion by which a cutting operation may be safely avoided is acceptable to the practical surgeon. My colleague, Mr. Porter (surgeon to the Queen in Ireland), was much pleased with the success of this plan in a case of inguinal hernia strangulated four days; and he has since tried it himself with satisfactory results.

The advantages which I claim for this procedure are—1. The strangulating portion of the ring is dilated before any pressure is applied to the bowel; 2. Much greater force may be applied to dilate than could safely be brought to bear when the intestine itself is employed for dilatation, as in ordinary taxis; 3. There is much greater probability of returning the bowel into the abdomen in a good condition, and, consequently, in a number of cases avoiding a dangerous surgical operation.—*British Medical Journal, Dec.* 23, 1871, *p.* 724.

59.—THE TAXIS IN HERNIA.

By F. Le Gros Clarke, Esq., Surgeon to St. Thomas' Hospital.

In the British Medical Journal of February 10th, 1872, there is a short art cle from the pen of Mr. Fifield of Boston, U. S. A., entitled, "A Plan for Facil tating the Reduction of Strangulated Hernia by Taxis." This paper contain a long quotation from Baron Seutin on the same subject; and the practice o the Baron is approved by Mr. Fifield.

The proceeding recommended and adopted, I presume, when the ordinar taxis has failed, and therefore when the strangulation is firm, consists in for ing the finger into the hernial ring, whether inguinal or crural, in order to ru ture, "by continuous forced dilatation, some of the (strangulating) fibres," whereby a cracking is produced, "very sensible to the finger, and sometimes t the ear." It is added, that "considerable strength has sometimes to be exerte and the index finger becomes fatigued." In this event "the finger is not to b *withdrawn*; but it is to be supported by the fingers of an intelligent assistan who seconds the action it is desirable to produce."

have never tried this experiment. I should be sorry to do so; and venture protest against it as a proceeding fraught with danger, and altogether op- ed to sound practice, based on an acquaintance with the pathology of nia. A long hospital experience has taught me how much mischief may be icted by persistent violence in attempts to reduce strangulated rupture; I apprehend that I am not singular in entertaining the opinion that the lity of our cases is chiefly due to delay, and the repetition of unsuccessful rts at reduction by taxis.

t is difficult to conceive of any method by which a perilous condition of ngulated intestine could be more effectually aggravated, even if reduction ceed the protracted violence, than by the proceeding here described; for s idle to suppose that the intestine can be protected whilst this splitting ssure is practised. As compared with a carefully conducted operation, ink there should be no hesitation in giving a preference to the latter.

s an advocate for early operation, and of opening the sac in all but excep- al cases, I strongly condemn the (as I think) fallacious principle of *reduc- at all hazards.* I am satisfied that the risks attending an operation for ngulated rupture are very much exaggerated; indeed, as I have pointed out where, experience has satisfied me that the fatality in these cases is not to the operation, but to the hopeless condition of the contents of the sac, often the consequence of delay, or to the prostration of the patient, prior to relief which an operation is designed to afford.

f gentleness and forbearance were more generally adopted in applying the liminary measures undertaken to afford relief, and purgatives and procrasti- ion—often self-inflicted—were eliminated from the treatment, I believe that statistics of the operation for strangulated hernia would present a far more ourable aspect than is at present the case.—*British Medical Journal, April* 1872, *p.* 398.

—ON THE TREATMENT OF ACUTE INTESTINAL STRANGULATION BY PUNCTURE OF THE DISTENDED BOWEL.

By Thomas Bryant, Esq., Surgeon to Guy's Hospital.

do not think the Surgeon should any more hesitate in performing an lominal section than he would hesitate in opening a hernia when symptoms strangulation existed. I cannot help speaking strongly on this point. I do k the two cases are precisely similar. We all know that in a case of ngulated hernia, if relief is not given, and given speedily, death is the inevi- le result; and we as well know that in cases of acute intestinal obstruction, lief is not afforded, death must as inevitably occur. Why, then, should resort to a remedy that proves so successful in the one set of cases, and tate to apply it in the other? The only thing that has hindered the Sur- is a general horror of the operation, and the greater magnitude of it. en you get it associated with the presence of a hernia, we have already your course is clear before you. You are bound to explore that; and, ing nothing there, are bound to go up further. But I am talking now of s where you find nothing of the kind—such cases, I mean, as bands or ts; cases of internal strangulation. Let us consider what means the sur- has at his disposal for their treatment. If you just ask yourselves what angulated bowel really means, you will easily see that other means than e which we have already discussed might be thought of, by which relief d possibly be produced. I will lay before you the line of thought in the in which it presented itself to me. Here is a piece of intestine passing ugh a tight ring. We will assume for the moment that the ring is a solid —one that does not stretch or expand. The intestine beyond the ring mes distended and enlarged from flatus or fæces. The bowel becomes ngulated in its unnatural position by a want of fair relationship between intestine and the ring. It may be an effusion of serum takes place on ount of pressure on the vessels preventing the return of blood; the bowel omes *irreducible and inflamed*—in fact, strangulated—and if left alone would subsequently slough, just as my finger would slough if I tied a piece of string tightly round it. The bowel becomes strangulated by causes acting within the ring in the bowel itself. Now, I think it is not difficult to understand that if we could empty that intestine of its contents the arrest of the flow of blood out of the intestine would cease, the circulation would go on, the strangulation would cease. Now, you see what I am aiming at as regards the possibility of taking away the cause of strangulation. We cannot take away the constriction of the ring, of the neck of the sac without division; but cannot we give relief through the intestine itself? Let us consider this point.

You know the question of the desirability of puncturing the intestine when distended with flatus has been well ventilated of late. Many cases have been recorded in which the operation has been performed with more or less success. I honestly say that my prejudices have been much against it, although there is every reason why it should have been otherwise, because at this institution it has been tried more than at any other. Mr. Stocker, who has more clinical experience than almost any man in London, has always been in favour of it. He always used to say, when a man was suffering from wind in this way, "Tap him; the cow-keeper taps his cows and they recover, and why should we hesitate to tap in similar cases?" I have done it myself on two occasions in cases of ileus, and with immense relief. Of course the great argument against it is this: you tap an intestine that is full of flatus, fluid, or fæces, and in putting your needle in there is the danger that some would escape into the abdominal cavity and set up peritonitis. It is solely that fear that has prevented my doing it except in extreme cases. But I think I can give you a case which will help you to form some idea as to the correctness of these conclusions. It as follows:—

Strangulated Scrotal Hernia (Cæcal)—Herniotomy—Puncture of Intestine—Recovery.—On August 18, 1871, my friend Mr. Kelson Wright, of Kennington, asked me to see with him a case of strangulated hernia in an old, half-childish man, aged 71. He had been the subject of a right scrotal hernia for thirty years, and had worn a truss. He had had occasional difficulty in its reduction after its descent; but Mr. Wright had always succeeded in reducing it. On the present occasion the same effort had failed, and when I saw him vomiting had existed for two days, and a large hernia existed in the right side of the scrotum; one portion of it felt tenser than the other. Chloroform was given, and the taxis employed, but without success; consequently herniotomy was performed, it being necessary to expose the bowel. When this was done the cæcum escaped, dragging down with it some three inches of small intestine covered with peritoneum—the external ring pressing firmly upon it. With some difficulty the bowel was returned, the wound brought together, and the whole carefully bound together by means of a pad and spica bandage of strapping. A morphia suppository was given.

During the night, however, this old man would get out of bed, and in the attempt he tore off all the dressings. As a consequence, the bowel came down again; vomiting returned, with abdominal pain. Mr. Wright was sent for, but all this efforts to return the intestine were fruitless. I was consequently sent for. I found the old man lower than when I saw him before. The hernia was down larger than ever. I gave him chloroform, and attempted reduction, but failed. I then increased the opening at the internal ring; but on doing this more large intestine came down, and no effort of mine could reduce it. I consequently punctured the intestine in four or five places with a grooved needle, and let off the wind; this measure enabling me to do what, under other circumstances, I could not do—reduce the hernia. The wound was then readjusted, and a good pad firmly secured on with strapping, opium being given; and I am pleased to add, no one bad symptom followed these rough measures, and a good recovery ensued.

Mr. Wright tells me the wound united without a drop of pus appearing, the whole uniting by primary union.

It was interesting to note that when the bowel was punctured nothing but *wind escaped*, except in one spot, where the smallest drop of blood oozed out, *evidently from* the congested intestinal walls. None of the contents of the testines escaped even after the rough manipulation to which they were bjected.

Now that case, gentlemen, I think is a text on which we might go on freely lating on the benefit of puncturing the large intestine. I think it has given a great lesson. There was a large piece of intestine entirely exposed, the cending colon and, I believe, the greater part of the transverse colon stended tremendously and black, not from effusion of blood, but from ngestion. I had been pressing and squeezing that intestine in my endeavour get it back for I should not like to say how long, but it must have been a great many minutes. I had been, as the medical man expressed it iterally mauled," and we both felt that the old man would die on the table re and then; but I could not stand by without giving him the chance any ther interference might afford. So I punctured the intestine, and, after a at escape of wind, I was able to return a part of it. In the second puncture, t a little drop (about the size of a large pin's head) of black stuff oozed , clearly blood coming from the congested walls of the intestine. I punced four or five times until I had reduced the whole of it. No fæces escaped atever, although during the whole of this time I was so manipulating the estine that if it was possible anything could come out it would have done

After the patient recovered I asked myself the question: If the large estine, exposed to view as that was, and not supported in any way by the lominal walls and contents, could be tapped without any escape taking ce, surely, where the intestine is supported, the risk of extravasation would greatly diminished by the natural support given to it in the abdominal ity? I think we must draw this conclusion, then: that there are cases in ich you may puncture the intestine freely, and with every prospect of rding great relief. In hernia this case clearly proves that you may resort it, and I believe you had better adopt it if there be much trouble in returning ery large hernia. Had I learned this lesson earlier, I should have tapped g before I did. But might we not employ this treatment much earlier? fter operating, we can reduce a strangulated hernia by pricking, which we ld not reduce without, is it not possible that we might reduce some hernia hout any operation at all? Consider a small enterocele. See the intestine lging beyond the neck of the sac and becoming congested. See how it omes more strangulated and congested if reduction is not effected. If you it you let out all the wind, the whole of the knuckle of intestine collapses, you get only a little flaccid lump. Under these circumstances, what is re to prevent the bowel being replaced by natural agency? I confess my ng for the idea from a surgical point of view. I think it is scientifically rect, and I see nothing to prevent it having the desired effect. I cannot ommend it practically, because I have had no experience of it; but I intend est the value of it when a suitable case comes before me. And what would stitute a suitable case? Not those slight cases where you can return the stine without opening the sac, because we know, as a rule, they do well. t I certainly will try it in those cases of hernia that we not infrequently get hospital, and sometimes even in private practice—large scrotal hernia, and ge umbilical herniæ, the interference with which is nearly always followed death. It is quite exceptional for a large inguinal or umbilical herniæ to over after herniotomy when the sac has been opened. By opening the sac ean not only just at the neck, but complete exposure and manipulation of contents. In both such cases, then, I shall most certainly apply this ctice. Now let us carry this thought a little further. You remember the e I related of a young lady who had strangulation of intestine, evidently m the mere grip of a coil of intestine by a band running from the loin to bladder. The band would not yield, the intestine could not get out, and ry moment rendered the strangulation more intense. Now, supposing I had ped the intestine, and let out the wind, the bowel would have collapsed, and bladder, as it became distended, would have allowed the intestine to escape. en, further, in cases of twist of the intestine you might get much the same ng. When once twisted and distended, it is difficult to untwist, but if the tension is relieved by tapping, you give the bowel a chance of righting itself. nsidering, then, with such examples as we have, that tapping is not attended with much danger, surely we are justified in having resort to it before an more serious operative interference, with every prospect of doing good. Do not, however, go away with the opinion that you should adopt it in all cases. But I think, taking that case I narrated as our text, in large strangulated scrotal hernia, strangulated umbilical hernia, and in all cases of acute internal strangulation due possibly to bands or twists, that we may reasonably look for very good success in its practice.

There is one other point I will just touch upon before we separate to-day, bearing upon the question of umbilical hernia. You heard me say how very fatal the treatment of umbilical hernia by operation generally is, that it is quite exceptional for recovery to take place. In all the years I have been at Guy's the only cases in which recovery has taken place have been those in which the contents of the sac have never been exposed. The stricture has been divided—I cannot say outside the sac—but an opening has been made only just sufficiently large to allow a herniotome to pass. I have had three or four such cases, and they have got well. In all the other cases, where more has been done, death has followed. I would go a little further, and say —Do not attempt to return an umbilical hernia, they are almost always chronic, slow in formation, and exist for many years before strangulation takes place. I think in large herniæ I would puncture; but when you must operate, never be tempted to expose the bowel—simply cut down on to the neck of the sac, divide the structure by a herniotome, and leave the rest to nature. You relieve the strangulation by simply dividing the neck, precisely in the same way as I believe you would relieve the strangulation by tapping the bowel.—*Medical Times and Gazette, April* 20, 1872, *p.* 455.

61.—INTESTINAL WORMS.

By William Date, Esq.

Many are the remedies which have, at one time or other, been recommended for tapeworm. It is only necessary here to refer to two or three of them. *Turpentine* acts with a considerable degree of certainty, but, if given in an effectual dose, is apt to produce disagreeable intoxication and strangury. *Kousso* was at one time a fashionable remedy. It is not easily obtained genuine, and (perhaps for that reason) is uncertain in its action and not to be relied upon. *Kameela*, a powder which consists of minute glands from the capsules of Rottlera tinctoria, has been highly lauded. It is said to be certain and speedy in its effects, and not more unpleasant in action than an ordinary purge. I have tried it in a few cases, and found, when given in effective doses, that patients complained of the severe griping pain which it induced. Dr. Garrod says he has found its active purgative properties rather objectionable. For the last few years I have been accustomed to rely entirely on the etherial tincture of the root or rhizome of the male shieldfern (*Aspidium filix mas*); and, when properly administered—*i. e.*, after due preparation of the patient,—I have found it thoroughly efficient. Sometimes, however, it requires a considerable amount of persuasion to induce the patient to submit to the necessary restrictions, which are certainly by no means pleasant nor always convenient. No food must be taken after breakfast except mutton-broth or tea. In the evening the patient takes a brisk dose of castor oil or compound senna mixture. The object of this is to clear away accumulated mucus, and expose the worm to the full action of the vermifuge. The following morning, early, one drachm and a half of liquid extract of male fern, rubbed up, with half an ounce of mucilage, should be taken in about two ounces of milk. It is better to lie quiet for two or three hours after taking this dose, otherwise it is apt to cause nausea and faintness. At the expiration of that time another aperient dose should, if necessary, be administered, since the male fern itself does not always purge. Milk is the favourite food of the worm, and is therefore the best vehicle for the administration of the medicine. It is commonly recommended to be taken as food the day before. I regard this as a mistake, as the animal becomes satiated with it, and is thus more likely to avoid the poison. Nothing ut broth or tea should be taken until the bowels have been evacuated, unless be a little brandy-and-water in case of faintness. After undergoing this rdeal the patient may be allowed a week's rest. The whole process should ıen be repeated. This time perhaps no fragments of the worm will be expel- d. I have given the male fern in several cases with perfect success after ther remedies had failed. In no instance have I found it necessary to give ore than three doses, although I always give one dose after any joints are ex- elled. Generally the whole of the worm has been killed by the first dose, and ius only two doses have been required. It is seldom the head can be found, ving to its small size, and, perhaps, its maceration in the intestinal juices. he discovery of some of the smaller joints may be regarded as tolerably tisfactory evidence of the destruction of the worm.

Being satisfied as to the entire expulsion of the worm, the next thing is to event the development of others. For this purpose, two indications are to observed:—1. To avoid the introduction of living embryos. 2. To restore e tone of the intestinal mucous membrane, so that it may not offer a favour- le nidus for their development. *No meat should be eaten which has not been roughly subjected to the action of heat.* It is clear that tapeworms cannot iginate *de novo* in the intestine. It may not be possible always to avoid the neasle," but we may always kill it by fire and render it innocuous. To ful- the second indication nothing is more useful than a mixture of nitro-hydro- loric acid with a bitter infusion. If there be much anæmia, as there often a few minims of tincture of perchloride of iron may be added to each dose, in the following mixture:—Dilute nitro-hydrochloric acid, three drachms; ıcture of perchloride of iron, a drachm and a half; syrup of oranges, half an unce; infusion of quassia to eight ounces: take one ounce three times daily. t the same time general hygiene should be well attended to. The diet should plain and nourishing. A daily sponge bath also will be of service.—*Lancet, eb.* 3, 1872, *p.* 146.

.—RECTILINEAR ECRASEUR, FOR THE REMOVAL OF HÆMORRHOIDS.

By Dr. J. C. Nott, New-York.

L. Nélaton objects to the use of an écraseur, for the removal of internal morrhoids, on the grounds that dangerous hemorrhage has been known to llow its use, and that from the amount of tissue embraced, stricture of the us not unfrequently results.]

About twelve months ago I contrived a *rectilinear écraseur*, which I think, operly employed, is free from the above objections, and fulfils all the indica- ons better than any operation yet devised. It is almost entirely free from bsequent pain.

This instrument has two parallel blades coming together like a clamp. One ade has a narrow fenestra running its whole length of about three inches, d the other presents a rough edge (like a fine saw) so constructed as to pass o and fill up the fenestra when the clamp is closed. There is a shoulder ojecting on each side of the blades, for the purpose of crushing more per- tly the tissues operated upon, a little beyond the thin edge of the blade.

This instrument does not completely sever the tissues (as does the écraseur Chassaignac), but crushes them down to a very attenuated pulp. If the morrhoid is within the sphincter, it is safest to tie a ligature in the sulcus ade by the écraseur for fear of some secondary hemorrhage—the tissue is so mpressed that a very small pedicle is left for the ligature, and the vitality of e part being completely destroyed the *ligature causes no pain*, and the mour drops off in a day or two. I cut away with scissors the part of the mour outside of the ligature at the time of the operation.

I removed five internal hemorrhoids, at one sitting, from a patient, a few ys ago, in the presence of Drs. Keyes, Castle, Yale, and Dudley, and after e effect of the anæsthetic passed off, the patient never complained of pain. ıis is a result *which is impossible* after ligatures. The clamp and actual cautery used by some surgeons give good results, but are much more troublesome.

Where the hemorrhoids are external, instead of using a ligature I clip them off with scissors as soon as the clamp is removed.

The principle advantages of this instrument are:—

1st. It is easily and rapidly applied, and requires much less time in its action than that of Chassaignac.

2d. It removes in a *right line* the exact amount of tissue desired, and *is not* followed by pain.

This instrument is applicable to the tongue, cervix uteri, penis, tumours on cervix uteri or vagina, the vagina in operations for procidentia uteri, &c.

I should not omit to mention that after I had worked out in my own brain the above instrument and exhibited it to Prof. Isaac E. Taylor, he showed me one he had invented twelve years previously, identical in principle though differing in shape and some minutiæ of construction; his is curvilinear, mine rectilinear, &c. His instrument, though invented twelve years ago, was never published that I am aware of, has not been exhibited for sale by the instrument makers of New-York, and having only within the last four years been a resident of this city, it is not remarkable that I never heard of it until it was shown to me by him. His instrument, I think, was designed particularly for the removal of uterine growths; but although the principle is excellent, I think the size and shape make it inconvenient and inapplicable in most cases where an écraseur can be used.

I have also had a pair of artery forceps constructed on the same principle, for arresting hemorrhage in arteries. About both these instruments I shall have more to say on a future occasion.—*New-York Medical Record, Oct.* 16, 1871, *p.* 381.

ORGANS OF URINE AND GENERATION.

63.—ON THE DIAGNOSIS AND TREATMENT OF URETHRAL AND VESICAL DISEASES.

By Sir Henry Thompson, Surgeon to University College Hospital.

There are no diseases more painful, and none the relief of which will gain you more gratitude from your patients.

In the matter of diagnosis, however, it is of the greatest importance that it should be a correct one, and not only correct, but rapidly made. I have now to say what I have said to you before, that I interrogate all these patients on the same system, and I advise you to follow this plan. I employ only four questions for urinary patients, and I advise you to use these four questions also, and always in the same order. The first question is, Is there any deviation in the frequency of passing urine? The second is, Is there any pain in the act? The third, Is there any blood in the urine? And the fourth is, Are the characters of the urine altered (quality and quantity)?

We shall see that in all cases of urinary disease these four questions are sufficient, together with the supplementary inquiries which arise out of them; yet we know how often such cases are misunderstood—indeed, the simplest are often mistaken, through not pursuing a systematic method in arriving at a diagnosis. First of all, let us look at the question of *frequency*. Almost every disease of the urinary organs produces some deviation in the natural frequency of passing urine. As a rule, let it be understood that a man in health does not generally rise at night to pass urine, and that he passes it during the day about five or six times; but when there is any degree of inflammatory action in the mucous membrane of the bladder, however slight, frequency of micturition is induced. Now, how does cystitis produce this increased frequency? When the mucous coat of the bladder is inflamed, it cannot bear to be much *extended*; and when the bladder contains five or six ounces of urine, or even ss, the sensitive mucous membrane suggests that it should be emptied: in- ead of comfortably containing fifteen or sixteen ounces, it cannot endure the ctension, and calls on the muscles to contract without delay. There is one, ad one only, of these affections which does not necessarily produce, at first, equent micturition. I speak of stricture: here it always occurs after a time; at a man may have a considerable amount of stricture for years before he is oubled in the way referred to. Calculous diseases produces cystitis, and thus uses an increased frequency in passing urine. Now, as a supplementary uestion, you should next ask, Is the frequency greatest at night or in the ay? If a man have calculus in the bladder, he is not so much disturbed at ight, but in the day he is frequently micturating—all movements make him o so. Now, that extremely common complaint, *hypertrophy of the prostrate*, worse at night than by day, as far as frequency of passing urine is con- erned. Hence, if a man of about sixty years of age says that he has but re- ently had urinary troubles, and these are greatest by night, the case is almost ade out; you may be sure that a very little further inquiry will demon- trate the fact that he is the subject of hypertrophied prostrate.

I come to the second question of *pain*. This question is of greater signifi- ance. Suppose the patient says he feels pain. Where do you feel pain—low lown in the belly? Then there is almost certainly chronic cystitis. Suppose e says that his pain is in the penis or perinæum, you must ask if he feels the ain before, during, or after, passing urine. If the pain be before, that is ecause the mucous membrane is becoming uneasy in consequence of disten- ion. If he find it painful during or after passing urine, and in the end of the enis, he is likely to have stone; and especially, also, if the pain be increased y exercise. The pain is at the end of the penis in stone. It is almost pathog- nomonic of calculus to find the pain near to the end of the penis during and fter micturition. In chronic prostatitis, the pain is also at the end of the enis. This simulates calculus in the bladder more than any other dis- ase.

The third question is, Has *blood* passed? This brings us nearer still to the oint. Blood may be seen in cystitis, but very rarely. The mode and the ircumstances in which the blood has passed, however, determine the nature of he disease. An elderly man, who passes blood intimately mixed with the rine, dark in colour, and not altered much by circumstances, with frequent ather than painful micturition, has probably hypertrophy of the prostate. In alculus of the bladder you find blood: it is as common in calculus as hæmop- ysis is in phthisis. Then a calculous patient will find blood in the urine after drive or a ride, or after hunting, and none if he keep quiet; or he may pass a rop or two with the last expulsive effort at micturition, and with pain at the ime. Such urine is usually rather florid in tint, while, generally speaking, lood passed from the kidney remains long in the bladder, and, from contact ith the urine, becomes brown in colour—it is like porter. This, also may appen when the bleeding is due to hypertrophy of the prostrate.

Lastly, is the *character* of the urine perceptibly changed? A man will often ll you his urine is thick; but he does not discriminate between the thickness f pus or mucus, and that from deposited salts, as lithates. Patients are gene- lly very much disturbed unnecessarily on account of thick urine. In this ld weather, the urine, on cooling, deposits its lithates readily, where none ould be seen in summer; and you may tell him that, if he apply a little heat it, he can see for himself that it will become quite clear again, which is never e case if the thickness be due to organic matters like pus or mucus; and if is be not an habitual appearance, you may make light of it. If, on the other and, a heavy deposit of lithates be constant, you must look into his habits and rrect his digestion—probably restrict some indulgence in diet. If, also, the ine do not become clear with heat, you have an organic compound to deal ith, and you must find out carefully the source of it.

Let me advise you always to make your patient pass his urine into two ves- ls for examination. I should not thank you for an examination of urine assed into one vessel; for whatever a man may happen to have lying in the rethra—a passage which is by no means always clear and sound—passes with

Let him *pass an ounce or two* into one vessel, and examine only what you find in the second vessel. If there be gleety discharge, if there be stricture of the urethra, you will find shreds of pus and mucus and blood-corpuscles in the first glass, but not in the second. In chronic prostatitis, always in hypertrophy of the prostate, sometimes there will be a deposit in the first vessel, which would much mislead you if you imagined it to come from the bladder or kidney. This specimen you must examine for albumen, for sugar, and you must inquire also the quantity passed per diem. Well, then, if a patient have told you that he has frequency of passing urine, increased by exercise; that he has pain at the end of the penis; that he passes blood; and that his urine is changed, you may arrive at a pretty good diagnosis of his case. But you would be very much to blame if you did not further examine the man: you must pass an instrument. It is best to be straightforward with patients and tell them so. People have too much common sense to be dealt with otherwise than plainly in these matters. You need not always sound a man with a stiff rigid metallic instrument at first, who has never had an instrument of any kind in his urethra. It is best to take a soft instrument, pass it gently into the bladder, which produces very little discomfort, and so diminish the patient's fear. You can then, say, pass another instrument (which will give you a little more pain), and ascertain completely what is the matter.—*British Medical Journal, Dec.* 9, 1871, *p.* 662.

64.—ON THE TREATMENT BEST ADAPTED FOR THE PREVENTION OF STONE IN THE BLADDER.

By Sir HENRY THOMPSON, Surgeon-Extraordinary to H.M. the King of the Belgians, &c.

[However perfect the operative means for the removal of stone may have become, there is no one who would not be infinitely well pleased if the formation of stone were prevented, rather than removed, however cleverly you perform the operation.]

Can we do anything to prevent the formation of stone in the urinary passages? It is, in fact, the early stage of this malady that will be the subject of our lecture to-day.

I commence by saying that I think a great deal may be done. But at the outset of the inquiry we naturally ask, What is the kind of stone (for there are several kinds) the formation of which we may hope to do most in preventing? All calculi are of local or of constitutional origin. By "local" I mean formed by disease in the bladder, and not depending upon any constitutional conditions; by "constitutional" I mean formed by some vice in the system. Now, the large majority of stones are of constitutional, and not of local origin. When they are local you know that we cannot prevent their formation except by mechanical means. Calculous matter, the elements of which are produced in the bladder itself, may be washed out, or may be broken and then washed out. But when stones are of constitutional origin—and those are what we are going to refer to entirely to-day—the elements are separated from the blood, and no mechanical means of prevention can be thought of for an instant. Now, from observation we know that nineteen out of twenty of such stones have uric acid for their basis, the remaining one in twenty being oxalate of lime;* and, less commonly still, there are phosphatic stones which are of constitutional origin also. Therefore, practically, to all intents and purposes, the problem before us is how to prevent the formation of uric-acid calculus.

Let us examine the early history of a case of persisting uric-acid deposit? First of all, let me say, going back to the root of the matter, that it is generally more or less hereditary. As a single illustration of this, we have just seen a case of uric-acid calculus in the ward, and learned that his father had "gravel or stone for the last twenty years of his life." And my almost invariable experience is, that either calculus or gout, most commonly the latter, has been observed in the family of the patient who comes to me with one of these forma-

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* *The deposits of oxalate of lime and of uric acid so often replace each other that the consideration of the latter becomes practically generally sufficient for our purpose.*

s in his bladder. I believe it, then, to be very strongly hereditary. We
ak of tubercular disease and of cancer as being transmitted by blood rela-
s, especially the former; but I do not think even it is more certainly heredi-
y than the disposition to uric-acid deposits in one form or another. I make
oint of asking the question of all patients who come to me with this com-
int; and although I cannot at present furnish you with an exact numerical
tement, I do know that in a very large majority, gravel or gout (for I wish
show you the identity as to the origin of the two complaints) has existed in
preceding generation; indeed, it is rare to find it otherwise. This heredi-
y tendency varies in force or strength in different families. You will find
e persons with persisting uric-acid deposits at thirty years of age, others at
ty; others at sixty. Of course, the earlier the time at which it appears, the
nger you will find the hereditary disposition, and the more obstinate, pro-
ly, will be its tendency to persist.

What, then, are the first signs of this condition in the patient. Usually, the
t sign is that the urine deposits pinkish matter, on cooling, at the bottom of
vessel, or the secretion may be merely cloudy when cold. It is passed
te clear, becoming cloudy only when it acquires the ordinary surrounding
perature. This phenomenon, therefore, may appear more frequently in
ter than in summer, because the external temperature is lower. It is
ply a deposit of salts from a hot solution as the liquid grows cooler, all
ng easily dissolved by raising the temperature of the liquid to that at which
as originally passed. This is a condition of urine which very often and
y unnecessarily excites much anxiety on the part of patients, and only the
sistence of which can be looked upon as a sign of what is called "the uric-
l diathesis." Mind, I mean strictly persistence, or at all events frequency
occurrence; for you or I, with no hereditary predisposition, may take a little
e beer than usual, or an extra glass of champagne, or a glass or two of unac-
tomed port, and find next morning a considerable quantity of this pinkish
osit, the urine looking almost like pea-soup, but not so thick; and when the
sel is tilted on one side a tidal mark, so to speak, is seen showing the height
which the liquid stood; all this, as I said before, being redissolved by heat.
t if, without any errors of diet, of which any but a very small allowance of
holic drink is only one, a patient habitually passes this kind of urine—if in
e there is a frequent deposit of uric acid, manifested by the presence of little
stals, looking like particles of cayenne pepper at the bottom of the vessel—
en this occurs rather early in life, say before forty, there is not much doubt
t there is a strong tendency to produce uric acid, either inherited or acquired.
this tendency may to a certain extent be acquired, or a pre-existing habit
y be intensified; but, as I have before said, it is almost always inherited.

We have followed the complaint up to the formation of cayenne-pepper crys-
I now show you a specimen of urine quite cloudy with lithates, although
must be familiar with it in the wards, and how on heating the liquid it again
omes clear, and that in a short time, while we are talking, it again becomes
udy on cooling. Let me once more remind you that this may happen with
most healthy individual; and it is only the persistence of the symptom,
hout errors of diet, which should lead you to suspect a condition that
uires treatment. Of the cayenne-pepper deposit I have some very good
cimens here, which have been collected from patients who passed it habitu-
These consist mainly of the transparent rhomboidal uric-acid crystals,
ich you know to be very beautiful objects under the microscope. They
y be passed almost daily and habitually, or only every month or six weeks,
large quantities, when they usually produce some irritation. At such
iods the patient may experience pains in the back and great discomfort, and
then said to have an attack of sand or gravel. These attacks may occur at
ying intervals, and usually become more frequent or severe, unless the
ient does something to prevent the formation. Afterwards he passes tiny
uli, popularly called "gravel," which seem to be rounded aggregations of
same crystals; and these little bodies tend in time to become larger, some-
es as large as small peas, or even beans; and they are still specimens of the
e product—that is of uric acid, associated more or less with some base,
h as *soda or ammonia.*

Now let me recall for one moment what I said as to the relation of gout to this condition. I have frequently seen these two complaints alternating, comparing one generation with another: gout appearing in one, gravel in the second, and then gout in the third. But the same individual may also have alternating attacks of gout and gravel. I have seen a patient suffering for years from gout, which ceased for several months, when he developed for the first time a uric-acid stone. Lastly, the so-called "chalk-stone," which you have often seen infesting the knuckles and disfiguring the hands of elderly people in advanced stages of gout, are composed of the same material—that is, of uric acid, usually as urate of soda. The identity of the two things, then, is unquestionable; they constitute two different series of phenomena, but both spring from the same root.

Now what is to be done for these cases? What mode of treatment will help to prevent the arrival of at least the advanced condition—namely, that of calculus too large to be voided by the patient? Generally speaking, I think we see such patients in a tolerably early stage, although this is by no means always the case. Some are much alarmed at a very early period, when the urine is only occasionally thick with urates. You will of course disabuse such patients of their false impressions, because numbers of persons mistake such thick urine for highly organised matter. I have known persons to become almost hypochondriac through not knowing that such deposits are of little consequence at first, and can be easily treated. But what are we to do for those who habitually pass the cayenne-pepper crystals of uric acid or small calculi? You will first seek the patient's antecedents, and learn all that he has to tell you of his habits, his diet, and his family history; and your mode of treatment will be determined accordingly. First of all, let me speak of the general principles upon which the treatment should be conducted. A very simple rule—indeed, too simple, I think, is often adopted. When the urine has persistently and habitually thrown down acid deposits, the patient has generally been prescribed alkalies; if, on the contrary, he has had alkaline deposits, he has been treated with acids. That simple mode has too often formed the main portion of the treatment. In the former case he has soda or potash largely administered, or he will be told to drink so many glasses of Vichy water, which is mainly a strong solution of carbonate of soda, only a natural instead of an artificial one. Now it is quite true that with alkalies, provided enough be taken, these deposits will disappear: the uric acid will no longer be deposited; the urine will become less irritating; the annoying symptoms will be diminished or got rid of. And of course the patient is very much pleased with this new condition of clear urine and disappearance of all deposit. And you will say, "What more can be desired?" This: you have merely made his enemy disappear, but he is by no means rid of its presence; you have not checked the acid formation. The uric acid is there as ever; but the uric acid and the urates are soluble in alkali, and you have only made them invisible. You really have the same condition as that of the fabled ostrich, which is said to put its head in the bush when pursued by hunters, and, no longer seeing them, believes itself secure. Just such is the security of the patient with uric acid who trusts solely to alkalies or Vichy water. His surplus deposits have become imperceptible to *his* vision; nothing more. I do not say the alkalies have been absolutely unserviceable as regarding his constitutional state, but they will not improve it to any great extent; and when he leaves them off the acid shows itself again. Diuretics must be regarded in the same light. In those cases which are treated with diuretics, the secretion of water is no doubt increased *quoad* the amount of solids, and the solids are thus dissolved. In both instances what you have chiefly done has been, to stimulate the kidneys, already overworked, to do more. You have by no means cured the patient.

Now let us ask what is the real pathology of these cases, and then I think I shall be able to show you a more efficient remedy. The problem has presented itself to me with great force and frequency, because people, naturally *fearing* they may arrive at the stage of calculus formations, come for advice in *the earliest* stages, and with the strongest desire to avoid the advanced one of *stone in the* bladder. So far from sending them to Vichy or giving them

:s, I believe they can be more effectually dealt with by a different modc ıtment. Let me premise, in broad and simple terms—as our time here, may perhaps add, the extent of our knowledge, will not permit me to be ninute or exact in detail,—that the origin of what we call gouty symp- as well as of a superabundant uric-acid deposit in the urine, is due to ive assimilation on the part of organs associated with or forming the ı viæ. I am quite aware that it is common in practice to speak somewhat ngly of the liver, its actions and its states, although we have still a good o learn about all this. Some years ago we talked and acted as if we thoroughly acquainted with the liver and its functions; but during the fteen or twenty years new light has been thrown upon the subject by rd, Pavy, and other workers in the same field, and we have learned that ore we inquired the less did we certainly know of its natural functions, ess of its action in disease. Thus, if one thing was more settled than er, at least since the time of Abernethy, it was that mercury had a ic influence on that organ; but now we find there are grounds for ing that the action supposed has no existence at all. There were other ı, indeed, which were vaunted to take the place of it, but no one ever ht of disputing the fact that you could augment the bile secretion by istering mercury. I am not here to say whether that is so or not, but ns to have been proved that there are substantial reasons for doubting if ıcient faith in that dogma be tenable. In speaking then, of the "defective . of the liver," or of "torpor of the liver," I merely use provisional , which most understand as indicating more or less distinctly a certain symptoms. Let them be briefly described as mainly consisting of a ınt, or almost constant, deficient excreting function by the bowels, imes, but by no means always, associated with impaired appetite and slow easy digestion; these latter being often absent if the diet is carefully ed, or if the patient lives in the open air and takes much exercise. On :her hand, considerable and multiform symptoms of disturbed digestion ›e frequently present. I cannot positively state whether those phenomena ally due to inactivity of the organ in question; practically, for us to- :his does not signify much, but the current terms are still convenient laries until others can be substituted for describing the condition in on.

v, at the bottom of this tendency to uric-acid production there often lies is thus understood as inactivity of the liver; and the true *rationale* of ıdue formation of the urinary salts appears to be that, the liver or some organ not doing its duty as an excreting organ, the kidneys have more thrown upon them. Thus the solid matters of the urine, or rather some ordinary constituents, are augmented,—not all of them, for urea is not sarily increased, but uric acid is largely produced, and is eliminated not n solution but in crystalline forms. Uric acid is very insoluble in water, ılthough the quantity thrown out may be quite soluble at the natural rature of the urine (100° Fahr.), when this diminishes to 60°, 50°, or 40°, :id is deposited; and when the quantity is still larger, even the ordinary nt of fluid associated with it at a temperature of 100° will not suffice to ve the whole, and solid uric acid is deposited in some part of the urinary ges. This deposit may take place in the kidneys themselves, giving rise, t thrown off, to the formation of calculus, at first renal, but sooner or mostly becoming vesical. Now, if all this be so, the formation of cid gravel is not by any means to be regarded as necessarily disease of idney; on the contrary, it is the result of an active and capable organ ously relieving some other organ which is torpid. The true remedy there- s, not to stimulate the kidneys, already, over-worked, not, to use a fami- imile, to spur that horse of the team which is already doing too much , but you are to seek the cause in that other one of the team which is deficient work, and that is almost invariably the liver, in the sense ly explained.

e treatment, then, which I advise you to pursue is to employ such agents ll stimulate the excretory action by the primæ viæ without depressing power. *No doubt that* a powerful agent for the purpose is mercury; and it is quite unquestionable that the relief of the symptoms above alluded to is to be obtained in a remarkable manner by occasional small doses of that drug. For our purpose, however, it is neither so successful in action, neither can it be onsidered so harmless, as another class of agents, which in these cases I believe to be in every way superior—I mean certain kinds of natural mineral waters. These I also regard as superior, in these maladies, to taraxacum, nitric acid, alkalies, and the other substitutes, as they have been termed, for mercurial remedies, in prompting the function of the liver. The mineral waters which I refer to belong to a group of springs all containing sulphate of soda, and some of them sulphate of magnesia, also, in solution. In studying these waters, I wish you to look with me at the composition of them, and at the same time to dismiss from your minds entirely those views of medicinal doses which you have acquired in the dispensary, and which necessarily belong to it, since small quantities of drugs, as they exist in mineral waters, will act more freely than will those quantities combined after the ordinary pharmaceutical method. You ask me for a demonstration, and I am quite ready to give it you. At the same time, let me caution you against regarding the small doses of mineral waters as having any affinity, either in the matter of quantity or by manner of administration, with what is understood as homœopathic doses. Thus, for example, you know that you may give A an ounce of salts, or B half an ounce, and you purge them; but you may obtain the same effect with one-fifth of those quantities if you give it as prepared in nature's laboratory in the form of mineral water. It is a curious fact, which I give as an ultimate one, and without speculating here on the cause of the difference. As a proof of the superior force of the saline combinations found in natural springs, I may refer you to the following experiment. If you will reduce by careful evaporation, as I have done, such mineral waters to their pharmaceutical condition of crystallised salts, you will find them possessing little if any more power than similar salts as obtained by the ordinary processes, and met with in every chemist's shop. They no longer do their work on the same terms as when administered in the original water before they were separated by evaporation. You will therefore readily understand how essential to our end it is to employ the natural mineral waters; since what are called "artificial waters," however admirably prepared, are simply pharmaceutical products, and are destitute of the very quality which distinguishes the remedies they are designed to imitate.

Here is a table of the waters which I refer to, with a comparative synopsis of their distinguishing saline contents, representing the number of grains (without chloride of sodium and other less active agents) in an English pint. Below these I add two well-known alkaline waters.

	Sulphate of Soda.	Sulphate of Magnesia.	Carbonate of Soda.	
Saline :				
Püllna	154 grs.	116 grs.	—	
Friedrichshalle	58 ,,	49 ,,	—	
Marienbad (Kreuz)	48 ,,	— ,,	9 grs.	Little iron.
Carlsbad (Sprudel)	25 ,,	— ,,	13 ,,	
Franzensbad	30 ,,	— ,,	6 ,,	Little iron.
Alkaline :				
Vichy (Celestins) about	3	— ,,	47 ,,	Little iron.
Vals(Magdeleine) about	—	—	65 ,,	Little iron.

The most powerful of this group is that of Püllna, which contains 154 grains, or nearly 2½ drachms, of sulphate of soda to the English pint, and nearly 2 drachms of sulphate of magnesia. Those quantities would give a tolerably efficient purge to anybody. But you must not give a pint of Püllna; 5 ounces would be a full dose. I do not like Püllna generally for our purpose, because t purges too freely, often gripes, and is very nauseous. Half a drachm of sulhate of soda and half a drachm of sulphate of magnesia in this form is too much for many people. I therefore recommend Friedrichshalle, which contains not a drachm of sulphate of soda in a pint, and little more than three-quarters of a drachm of sulphate of magnesia. Nevertheless, you would not think of giving a pint; eight or nine ounces make an efficient purge; for many persons six or seven suffice. I think I may say that seven ounces is an ordinary average dose; if you take an average dose of Friedrichshalle water an hour before breakfast, and soon after a cup of hot tea or coffee, you will have a full free action of the bowels; perhaps two. That, you see, would be about twenty-five grains of sulphate of soda, and twenty grains of sulphate of magnesia, which, taken in any combination you like out of a druggist's drawer, would have no appreciable action; you might be a little uncomfortable perhaps, but there would be no action of the bowels. To repeat what I have said: if you evaporate a quantity of Friedrichshalle water in a warm water bath, and obtain as perfect a product as a chemist can produce, and administer four times the amount of salt in that form which exists in a dose of the natural water, you would still not have such efficient or such certain results as from the small quantity (in the natural water) named above. So that there is something, which I do not pretend to explain, and certainly shall not speculate about here, which distinguishes the action of mineral waters from the action of salts which are produced pharmaceutically. The next water on my list is Marienbad, which contains no sulphate of magnesia, forty-eight grains of sulphate of soda in the pint, with nine grains of carbonate of soda, and a small quantity of iron. About half a pint produces for most persons an easy motion. If this water is exposed to the air for a day or two, there will be an obvious brown deposit of the iron, and it may be regarded as slightly ferruginous, though that is a secondary character. The next is Carlsbad with its many springs, all of which contain about twenty grains of sulphate of soda and thirteen of carbonate of soda in the pint. Then we come to Franzensbad, which contains thirty grains of sulphate of soda, six of carbonate, and a little iron, which Carlsbad does not. That closes this group of springs.

But now I shall just point out the distinctive characters of the alkaline waters which are so popular in this country. First, Vichy, which contains only three grains of sulphate of soda, but nearly fifty grains of carbonate of soda, in the pint—a powerful solution. Then we come to Vals, which is also from the volcanic district of France, some of the springs of which contain upwards of sixty grains of carbonate of soda, and nothing else worth mentioning. These two waters are extremely famous, and are much resorted to against gout and gravel. Under their use the uric acid deposits disappear—that is, they are dissolved by the alkali. Inasmuch also as this appears to have some beneficial action on the liver, a certain degree of permanent benefit is perhaps also attained. Thus such patients are often better for a time after a visit to Vichy; but, as a rule, are not permanently benefited. I am satisfied, after observation on the spot, and on the effect of the waters here that they only temporarily mitigate the complaint, and do not cure it. Now, the principle upon which the waters of Friedrichshalle and Carlsbad are beneficial is, that they produce activity in all the digestive functions, and thus waste matters which have been hitherto thrown out as uric acid by the kidney are eliminated in some other form. And thus it is that, if it be necessary to send these patients abroad, I prefer very much Carlsbad to Vichy, provided always that the subject of a Carlsbad course must not have become too weak, as for a time it makes a demand on the strength not well supported by a feeble person. Generally, however, this is not at all the condition of those who are passing uric-acid gravel. Happily, also, for most people our purpose can be attained as well at home. I believe that a short course of Friedrichshalle first, followed by Carlsbad, or by a combination of the two, produces the best results in these cases. This method has, at all events, been more successful with me than any other. Such a course should be continued, according to circumstances, for six or eight weeks.

A few words upon the way in which you should give them. If you have a patient *coming to* you whose digestion is not good, complaining of *foul tongue, with deranged* digestion and loss of appetite, it is sometimes, not always, desirable to give first a single dose, no more, say three or four grains of blue pill at night, and the next morning eight or ten ounces of Friedrichshalle water, so as to ensure good action at first. Then commence with Friedrichshalle combined with hot water, one dose every morning an hour before breakfast, diminishing a little the quantity every day, or every few days. One of the characteristics of this water is that the longer it is taken the smaller is the quantity necessary to effect the purpose. If, for example, seven or eight ounces taken in the morning, say with five ounces of hot water, produce one active movement of the bowels immediately after breakfast, the next morning six or seven ounces will do the same, and the morning after probably five or six ounces; and it is very likely that at the end of three weeks the patient will from four ounces experience the same effect produced originally by seven or eight. But after you have given it thus for one, two, or three weeks, according to the nature of the case and the results, you should combine it with Carlsbad, say three or four ounces of the former and five or six of the latter, with three or four of hot water every morning. When Friedrichshalle is given alone, and also when it is mixed with Carlsbad, 20 or 30 per cent. of hot water should be added, that it may resemble somewhat the original condition at the spring. Friedrichshalle is naturally hot, and is evaporated on the spot to a small extent, being regulated and rendered uniform by stopping the process when the water reaches a certain specific gravity. Carlsbad, which at the spring is too hot to drink until cooled, should, when taken alone and unmixed, be raised in temperature to 90° or 100°, by placing the tumbler containing it in a vessel of hot water for a few minutes. After giving this combination of the two waters for two or three weeks, six, seven, or eight ounces of Carlsbad may be taken alone for another fortnight perhaps. The quantities given are considerably less than those administered at the spa itself, where a patient's stay is necessarily limited as to time. I am quite satisfied that the smaller quantity here recommended, and employed for six to nine weeks, instead of the usual three weeks of a foreign course, is better for the majority of the patients we have to deal with. The same quantity of water given there in twenty-one days, producing often notable loss of weight and power, will, if given here in fifty or sixty days, attain the object as certainly and more safely. I by no means dispute that there are other cases which may be benefited by the more heroic plan adopted on the spot. I have largely and systematically employed these agents now for seven or eight years, modifying the quantity and the mode as experience has indicated, and the course thus briefly described is the result of it. The course may be repeated with advantage, if necessary, for most patients after an interval of three or four months. Meantime, as an occasional aperient and a corrector of digestion for these patients, I know nothing at all equal to Friedrichshalle. It leaves the patient as a rule less constipated after discontinuing it than he was before; and, as already said, may be taken habitually without lowering the system. I have no patients continuing its daily use for three or four years, but I do not advise this course unless in exceptional cases. Nevertheless I know a gentleman, nearly eighty years old, who has taken it for five years regularly, a wine glass every morning, and the effect has been for him most admirable. Suffering much formerly from an obstinate constipation, he enjoys perfect regularity and excellent health. Let me say here that what is called "Carlsbad salt" is often used for the same purpose, and is taken also in the belief that it represents Carlsbad water. This it does not do, consisting almost entirely of sulphate of soda taken from the water, and has the same and no more virtue than that salt when obtained from any other source. No doubt that the sulphate of soda, known also as "Glauber's salt," is one of the most admirable medicines we possess, and deserves to be more popular than it is. I constantly order it, with or without a small addition of sulphate of magnesia, for the out-patients as the best *substitute* within my reach for the mineral waters in question.

In the few minutes which remain, I will advert briefly to the subject of *diet, certain* restrictions in which are very important. It used to be said that *when* uric acid is largely deposited, the nitrogenous elements of the food should be considerably diminished. I do not find in practice that a strict application of this rule is advantageous. On the contrary, the diminution of the deposit is more certainly attained by a course which is almost the opposite of that. There are three classes of aliments which must be permitted to the patient very sparingly, in order to attain the end in view—viz., alcohol, saccharine, and fatty matters. First, alcohol: any fermented liquor permitted as an article of diet should be selected in its more diluted and in its most pure form. Believing that form in which it is found existing in natural wines to be usually the best, I would advise a light, sound Bordeaux or a Rhine wine of similar quality, the former, perhaps, agreeing better with most persons here. You will forbid champagne, as for the most part imperfectly constituted, and always bad if containing much liqueur. The stronger wines, as sherry and port, are mostly unsuitable, and strong beer is to be absolutely forbidden. Solutions of pure spirit and water are exceptionally desirable for some few persons with weak digestion. Secondly, sugar in all its forms: at every meal and wherever met with, forbid it altogether. Thirdly, let fatty matters, butter, cream, and the fat of meat, whether simply cooked or in combination to form pastry, be taken very sparingly. I cannot enlarge on the theory on which this advice is given. Suffice it to say that abstinence from the substances named probably lightens considerably the work of the liver, and so lessens the vicarious labour of the kidneys, in accordance with the views already propounded. Let me just advert, moreover, to the dietetic system at Carlsbad. In recognition of some such principle there, no doubt, the use of sugar and of butter is absolutely forbidden during a Carlsbad course; and were you patients there, your purveyor would not supply you with the forbidden food, however much you might demand it. I can only say, as the result of observation, that this system, much more than the elimination of meat from the dietary, will reduce the uric-acid deposit. If you will cut off a portion of alcoholic stimulant when necessary, and it often is so, will forbid everything that contains sugar, and diminish considerably all fatty matter—giving nitrogenous food, in fact, and eliminating hydrocarbons—you will generally accomplish more than by the contrary method. In addition to all this, you will, of course, see that the patient takes daily a fair amount of exercise in the open air, and that he protects his skin and encourages the performance of its functions by habitual ablutions and sufficient clothing. These points I can only name; but they are essential concomitants to the rest.

Well, then, it is this system of diet and regimen, and the occasional systematic employment of the mineral waters named, which mainly constitute the treatment I strongly advise for the purpose of checking calculous disease in its early stages, and so to prevent the formation of stone in the bladder in that considerable majority of cases which are due to uric-acid formation and its consequences.—*Lancet*, *Jan.* 13, 1872, *p.* 85.

65.—REMARKS ON STONE IN THE BLADDER.

By W. F. Teevan, Esq., B.A., Surgeon to the West London Hospital, and to St. Peter's Hospital.

The question of lithotrity *versus* lithotomy in males under sixteen years of age may be considered as settled in favour of the latter operation, for cutting for stone in boys is so very successful an operation that no apparent advantage can be gained by substituting, in them, lithotrity for lithotomy; but the physical development of a patient, rather than his age, ought to determine the question as to which is the earliest period that lithotrity may be admissible in youth. And it must also be borne in mind that the narrow urethra of a boy may preclude the introduction of a lithotrite. The youngest patient in whom I have performed lithotrity was sixteen years of age; but he was large for his age, and his well-developed penis enclosed a capacious urethra.

If possible, lithotrity ought always to be performed in adults in preference to lithotomy; but inasmuch as lithotrity is in reality only applicable to selected cases, it will be seen that in hospital practice a large number of sufferers must always be left for the knife as the only means of cure. As a general rule, it may be said that it is not advisable to crush a stone which is larger than a walnut. In men who are young and strong, a large phosphatic stone may be crushed with safety; but it is not desirable even to crush a moderately-sized calculus in an infirm old man whose health is much broken, or who may be the subject of chronic cystitis, for the increased irritation set up by the operation of lithotrity in a much diseased bladder is likely to prove perilous to life. The state of the bladder in the subject of a contemplated operation has not, I think, received that attention, sometimes, which it deserves. If the viscus be in an irritable state, or be unable to expel its contents without the use of a catheter (not comfortably borne), we shall be exposing the patient to danger if we perform lithotrity; for he will suffer great annoyance either from his ever-recurring desire to micturate, or our washing out his bladder, so that his health will probably break down under the prolonged irritation and confinement. If there are several calculi, it is usually better to cut than to crush; and an oxalate-of-lime stone, unless smaller than a walnut, had better be cut out, rather than crushed.

Then, again, there are certain cases—not many, though—in which no instrument shaped like a lithotrite can be introduced into the bladder, and which must be cut on a very long and curved staff. Thus, therefore, it appears that lithotrity takes out the good cases for itself and leaves the bad ones for lithotomy; the former operation is only applicable in selected cases, the latter embraces all. We may thus at the very outset premise that lithotrity can be performed oftener in private practice than in hospital; for, in the former, patients usually seek advice on the advent of the first symptoms of stone; whilst, in the latter, they very often delay applying to a hospital for relief till forced to do so by the urgency of their sufferings caused by the presence of a calculus of long standing.

Messrs. Mayer and Meltzer have constructed for me, after my own design, a lithotrite which I believe has certain advantages not possessed by any other. It must be a matter of daily observation to every surgeon that a sound or metal bougie glides into the bladder more easily than a hollow metallic catheter; and we shall also find that the heavier the lithotrite the more facile will be its introduction. I consequently have had my lithotrite made with a heavy, square steel handle, which form not only permits the greatest facility of introduction, but also unites firmness of grasp, when held, with extreme delicacy of manipulation. Then, again, I have substituted a vertical slide for a button, which only requires the pressure of the thumb—infinitely more simple than thrusting a button to and fro.

The great secret of success in passing a lithotrite comfortably to the patient, and without any hitch to the manipulator, is simply to keep the handle close to the patient's belly, and to delay the turning movement until it is called for by the fact of the lithotrite not going any further, thus announcing that it has arrived at the turning point. The instrument ought to be oiled from end to end, and the penis pulled well forward on to the lithotrite until the beak has passed the subpubic arch. If the patient's prostate be enlarged, a good stiff bolster under the buttocks will make all the difference between success and failure. No instrument glides into the bladder so easily as a lithotrite; it simply requires, not pushing, but directing: its own weight does away with the necessity of the former action. Any attempt to force a lithotrite into a bladder will be attended with disastrous consequences, especially if the prostate be enlarged. As a rule, the only difficulty in getting hold of a stone occurs when it is small and lying in a cul-de-sac behind a large prostate; and it may be said that the shorter the time that the instrument remains in the bladder the more favourably is the case likely to turn out.

In performing lithotrity, one of two courses is open to us. We can either break up the stone in one or two crushings, or we can subject the patient each week to one very short and partial crushing. If the former procedure be adopted we shall, unless the stone be very small, set up great local and constitutional disturbance and irritation, depriving the patient of rest, and taking away his appetite; if, on the contrary, the latter method be adopted, we shall create but very little annoyance, and the patient will only be confined to his bed the day of his operation, and lose neither rest nor appetite; that method, therefore, must be the better which does not interfere with a patient's rest or appetite. By adhering to the plan of very short crushings, I have lately treated several cases with perfect success as out-patients, and believe they made more rapid recoveries than they would if they had been confined to the hospital; it must, however, be borne in mind that the only exercise they were subjected to was a short occasional walk at a slow pace.

I object to chloroform being given in lithotrity, for I consider the patient's consciousness a most useful adjuvant to the surgeon in this operation, and I should not allow it to be administered in any case of my own unless some grave reason demanded its administration. I am afraid the act of "washing out the bladder" is looked upon very lightly, but it is infinitely more annoying to the patient than lithotrity, and ought never to be employed unless imperatively needed, for much irritation may be set up by it.

[Some time ago Mr. Teevan instituted a series of experimental researches into the state of parts after operation on the dead subject. The following were the results arrived at]:

1. If the tip of the forefinger be introduced into the prostatic urethra, either from the bladder or through the membranous urethra, it will be found that it will only admit the terminal joint of the forefinger without laceration.

2. If the introduction of the forefinger be continued, the mucous membrane splits horizontally as the second joint is passing through. The urethra splits in the roof because the convexity of the joint is pressed against that part. In lateral lithotomy the incision into the prostate prevents laceration of the roof of the prostatic urethra.

3. If a stone, half an inch in diameter, be extracted through a prostate in which no incision has been made, the mucous membrane of the floor of the urethra is lacerated, and the prostate slightly torn; the capsule remains perfect, but the orifices of the ejaculatory ducts are often with difficulty recognised.

4. If a calculus, half an inch in diameter, be extracted through a prostate which has been partially incised, as in lateral lithotomy, the capsule and the orifices of the ejaculatory ducts remain perfect.

5. Stones upwards of half an inch in diameter, when extracted by the median operation, lacerate more or less the prostate and its capsule, and obliterate the orifices of the ejaculatory ducts.

6. Calculi of one inch in diameter and upwards, when extracted through a prostate which has only been partially incised in the lateral operation, lacerate the gland and its capsule completely in a direction downwards and outwards, and obliterate the orifices of the ejaculatory ducts.

7. If a stone be extracted through *an aperture made by cutting, and not by laceration, then the orifices of the ejaculatory ducts can always be distinguished.*

8. When a calculus is extracted from the bladder by means of a limited incision and subsequently so-called dilatation, either in lateral or median lithotomy, there is always more or less eversion of the gland—that is, in such cases the stone has a tendency to enucleate the gland from its capsule in a direction forwards.

Thus, therefore, only a very small stone can be extracted through a partially incised prostate, without completely lacerating the gland and its capsule.

The specimens of bladders and prostates after lithotomy which I have exhibited show that after an ordinary-sized calculus has been extracted by lateral or median lithotomy the prostate is always found split in two, the halves being held together by a fibrous remnant of the capsule about half an inch broad.

Some persons might possibly object to deductions drawn from experiments on the dead body, and say that the results obtained after death must be very different to what happened after lithotomy on the living; but it must be borne in mind that the mechanical properties of the fasciæ are not altered for some time after death, and therefore experiments made a few hours post mortem afford results similar to those that would have ensued in the living. I have also made a personal examination of all the pathological museums in London, and have obtained the following facts:—

1. Out of the many specimens of bladders and prostates after lithotomy, there is no unequivocal specimen which shows that an ordinary sized stone can be extracted through a medium sized prostate by means of the limited incision and subsequent so-called dilatation without complete rupture of the prostate and its capsule.

2. That in extracting ordinary-sized calculi through the prostate, not only are the gland and capsule completely ruptured, but the rent extends into the bladder, as far usually as the orifice of the left ureter.

3. That where there has been much laceration or bruising of parts, the orifices of the duct are no longer to be distinguished.

4. That there are several bladders and prostates of persons who have lived upwards of ten years after lateral lithotomy, and in each specimen the cicatrix can be seen extending into the bladder nearly to the orifice of the left ureter.

5. That a fistula in the bladder communicating with the rectum is not an uncommon occurrence after lithotomy—the result of bruising or laceration,—and would not appear to be of any moment.

6. That in ordinary lithotomy the prostate is completely split into two, the halves being held together by a remnant of the capsule about half an inch broad.

7. That the most frequent cause of death after lithotomy would seem to be the extensive suppuration set up by the bruising and laceration of parts, followed by phlebitis and pyæmia.

8. That infiltration of urine, after lithotomy, must be regarded as a surgical curiosity.

It will thus be seen that whether we examine cases of lithotomy on the living or dead, the same conclusions will be arrived at—that an ordinary-sized stone cannot be extracted through an ordinary-sized prostate by means of a limited incision and subsequent so-called dilatation without complete rupture of the gland and its capsule. I may here remark that Professor Ellis has for a great many years past practically demonstrated to his class that there is no such thing as dilatation of the prostate, and that what surgeons call dilatation is in reality complete laceration. A subject of very great importance, which has not yet received any attention, is the occurrence of impotence after lithotomy. I have seen several such cases. Impotence results from the laceration of the mucous membrane around and lining the orifices of the ducts, and their subsequent plugging; and my objection to the median operation is founded on this ground, for in it the mucous membrane is usually lacerated in the floor of the urethra, and the prostate occasionally split into halves in the paths of the ducts, thus interfering with their integrity.

The practical conclusions at which I have arrived are—firstly, that when lateral lithotomy is performed the stone ought always to be cut out, and not torn out; and, secondly, that the median operation is not justifiable for the extraction of calculi which are upwards of half an inch in diameter, for if such stones be removed by that process obliteration of the orifices of the ejaculatory ducts, and permanent impotence will ensue. Dr. Richardson has stated, from his inquiries, that the median operation is attended with a larger mortality than lateral lithotomy. In boys lateral lithotomy is so successful that no possible advantage can be gained by substituting for it the median operation, inasmuch as that procedure cannot be accomplished in the child without lacerating the very part we ought to avoid—the floor of the prostatic urethra.

Now for a few words regarding lateral lithotomy. (1) The sound ought to have a very short beak, not longer than one inch. The shorter the beak the greater the facility for exploring the bladder. A sound shaped like a catheter will just as often miss a stone as hit one. (2) I use a rectangular staff made after my own design; the vertical part is short, the handle large, and the groove on the horizontal part is continued upwards on to the vertical piece for one inch. The staff ought to be held with both hands; the lower one resting on the pubes. By this method an amount of fixity can be given to the staff unattainable by any other means. I learnt this procedure from Dr. Henry Dick. (3) Some surgeons use a narrower bladed knife when operating on children to what they do in the adult; but this is manifestly wrong, for inasmuch as the ame sized instrument, the left forefinger, is introduced in either case, there ught to be no difference in the size of the knife. Most of the accidents that ive occurred in connexion with lithotomy have arisen from a difficulty tperienced in getting into the bladder in children. This has taken place from sing a very narrow-bladed knife in order to make a small internal incision to the bladder; the result being that the opening made was totally insufficient admit the forefinger, the bladder therefore being pushed back into the pelvis.) In children I never use a forceps, but extract the stone with the forefinger ly. It can be made to slide out of the bladder by simply pressing on it. nother way I have, if I experience any difficulty, is to get the left forefinger ıder the stone, press it firmly against the bladder at the same time that the ;ht hand is placed above the pubes. In this way the left forefinger can ake the stone slip out. For adults I use a simple open polypus forceps. It ll never let a stone escape. (5) The external incision ought to commence low wn; by this means we wound less vascular parts, and there is but little eeding—all-important in infirm old men. (6) The internal incision ought be free, for there is no such thing as dilatation of the prostate. The bladder ust be cut into, not merely pierced. (7) I have never used a tube after hotomy; for the best of all reasons, that I have not seen its usefulness in e practice of others, and I know that a man has lost his life through its nployment.

The above conclusions I have arrived at from carefully watching the practice other surgeons, from my own experiments and investigations, and, lastly, om my own experience, based on thirty-one cases of lithotomy and lithotrity, l of which have recovered except two.—*Lancet, Dec.* 15 *and* 28, 1871, ›. 846, 882.

.—CASES OF STRICTURE TREATED BY A NEW WEDGE DILATOR.

By Berkeley Hill, Esq., University College Hospital.

The accompanying woodcut depicts a modification of the principle adopted : Holt's, Perreve's, and Richardson's stricture dilators, which has been used th success by Mr. Berkeley Hill in University College Hospital. The two lowing cases are selected from a series:—

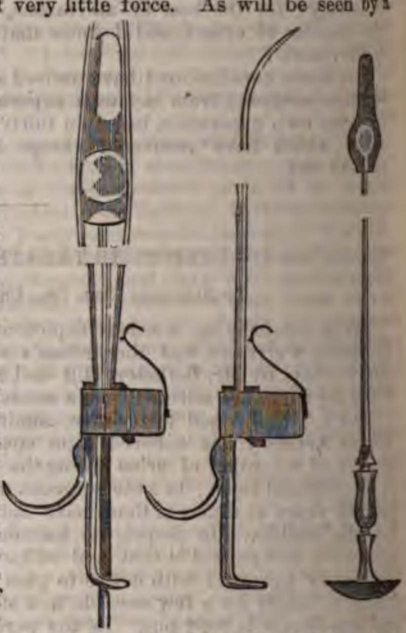
Case 1.—C. J., aged thirty-nine, admitted Sept. 4th, 1871, formerly in the ırine Artillery, has suffered from stricture for many years, and occasional acks of retention of urine during the last ten years. Has been in hospital a provincial town; but the surgeons under whose care he has been for the st six years at different times have not been able to introduce an instrument o the bladder. In despair he has come to London for further advice. On mission, the patient is thin, and looks much older than he really is. He is nstantly agonised with desire to pass urine; this escapes in drops, or, if he rces violently for a few seconds, in a stream. The urine is dark, offensive, d one-fourth is ropy pus. In the perineum a mass as large as the finger, d apparently as hard as wood, occupies the middle line. There is no elling of the superficial parts, and the prostate is not enlarged. During any trials with bougies of various sizes and kinds, and the involuntary plorations of old false passages, on two occasions a fine silkworm gut bougie ıs got through the stricture, to the slight improvement of the flow of urine. iese attempts occupied more than a fortnight, in which time the patient's fferings were treated by warm baths, rest in bed, suppositories, and subtaneous injection of morphia.

On Sept. 22nd, Mr. Hill succeeded in passing a No. 2 silver catheter under loroform. This was tied in, and replaced the next day by a flexible one. a few days, when the irritation had subsided, the patient was again put ıder chloroform and the dilator passed, a No. 13 catheter being immediely introduced, tied in, and the urine drawn off. No reaction took place.

The patient was trained to pass an instrument for himself, and was disarged on October 6th, passing No. 12 easily, his urine being quite clear d free from albumen. He lay all night without passing urine.

Case 2.—A groom, aged twenty-six; admitted Oct. 5th, 1871. Nine years before he had contracted traumatic stricture by falling astride a bar. For the pain, bleeding from the urethra, and inability to pass water, he was treated at Guy's Hospital. Five years later, retention having again occurred, he went to Guy's, and his stricture was again dilated by the daily passage of instruments. After each discharge from hospital he neglected the injunction he had received to pass a bougie from time to time. When admitted to University College Hospital, a large, hard mass, was found in the perineum, and the patient was passing urine every quarter of an hour; urine clear. After some difficulty Mr. Hill passed a No. 1 flexible catheter through a stricture four inches and three-quarters beyond the meatus. This was tied in; and on the 12th the stricture was easily split to No. 14, and a No. 12 catheter tied in for twenty-four hours. In a few days the patient could pass a catheter for himself, make a good stream, and was discharged with the strict injunction not again to neglect the occasional passage of a bougie, No. 10, which was given to him.

These cases have been selected from several others treated by the dilator as samples of copious induration of the urethra giving way instantaneously before the wedge on the exertion of very little force. As will be seen by a glance at the drawing, the instrument consists of a split sound which equals the calibre of a No. 2 or No. 3 catheter. The halves of the split sound can be separated by passing between them a wedge fixed on a slender stem. The wedge is prevented by two dovetail grooves at its hinder part from leaving the sound, which acts as the guide during the passage of the wedge down the urethra. When the instrument is used, the urethra is first explored by an olive-headed bougie, graduated on its slender stem, by which the situation of the stricture is ascertained. The split sound is next passed along the urethra till the beak reaches the bladder. The wedge is then inserted between the blades, and pushed swiftly down till it has passed the stricture. It is then withdrawn, the blades of the sound fall together, and can be removed easily. A full-sized No. 12 or No. 13 catheter is at once passed into the bladder to draw off the urine, and may be taken out or tied in for twenty-four hours. On the third day a No. 12 or No. 13 bougie is passed, and again every other day for a week, while the patient learns to use the instrument himself, and not to let more than a fortnight pass without doing so. The instrument is adapted for the same cases as Mr. Holt's—namely, strictures situated in the bulbous portion, not in the penile part, where the division of a stricture should be a clean cut, in order that a cicatrix may as little as possible block up the erectile tissue and interfere with erection.



The advantages claimed for this instrument are: simplicity of construction; the central guide of Holt's instrument is not needed, hence the split sound can be passed through narrower strictures. Next, and chiefly, diminution of resistance, and, consequently, the more immediate application of the rupturing force—the wedge—to the impediment to be overcome. The force needed to push Holt's dilator is sometimes very great, and the attempt has been abandoned, or the instrument has broken, even in skilful hands, from this cause. Much of the force is expended on the continuous friction outside the tube e split sound, and inside along the guide. In the wedge dilator the surface is reduced to two dovetail grooves, which together do not ex- lf an inch. For this the force required is so small that one hand to overcome the resistance.—*Lancet, April* 6, 1872, *p.* 466.

THE TREATMENT OF RETENTION OF URINE IN IMPERMEABLE STRICTURES OF THE URETHRA.

. A. O'Connell, late Medical Director of the "Ninth Army Corps," U.S. Army, &c.

etimes happens that owing to carelessness, or fear of pain, a man lets a ly contracting stricture go untreated so long, that he suddenly finds unable to void urine at all.]

n the surgeon sees the case the catheter is resorted to of course, but the to pass it fails. Instruments of different sizes and material, perhaps, l, but in vain; and after prolonged, patient, and skilful labour over e, the surgeon finds himself unable to effect an entrance through the e by any means at his disposal. The stricture is an impermeable one; nd such aids to treatment have failed to break up the embargo; the is suffering intensely from the retention of urine; no time is to be lost, at is to be done?

o cases of this kind which have come under my care I have been able ve the patient upon the principle of suction; and the simplicity of hod, as, also, its advantages if upon further trial, it be found to be of l application, prompt me to make it public.

e first case which presented itself the gentleman had suffered from re- of urine on account of stricture before, and, consequently, was not larmed. He was always intending to submit himself to treatment for e of the stricture; and this *in*tention became very determined and posi- ing his attacks of *re*tention; but relief from immediate danger made get his resolution, and he would always postpone action. When I saw wever, the usual means of relief had failed. Baths, opiates, and the r had been resorted to by a medical gentleman of skill to whom he had for assistance, but all in vain, and his sufferings were very severe. I areful attempts to pass instruments, but without success. The other of relief that suggested themselves to my mind had failed already, e sufferings of the patient required that something should be done ately.

g upon my office table an india-rubber hand-syringe, consisting simply bber pouch or ball, with a hard rubber stem to it, that I generally a part of Politzer's apparatus for inflating the inner ear, it occurred hat it might be made use of as an exhauster—a suction instrument,— at by this means, perhaps, the stream of water could be started. upon the idea, I took a catheter of medium size, made a perforation xtreme end, and passed it *down to* the stricture; then squeezing the pouch so as to drive out the air, I connected it by means of a short f india-rubber tubing with the catheter already in the urethra, and g it to expand gently, instructing the patient at the same time to gentle effort, *and only a gentle effort*, to pass his water, I had the tion of learning that the experiment had become a success, and that n was relieved.

is was merely a transient patient whom I had not seen before and did again, I do not know what became of him afterwards. He stated, n agony, that he would return on the following day to place him- der treatment for his stricture, but, as in the previous instances he ld of, no doubt relief brought with it carelessness, and I saw him e.

he second case in which I tried this method, the man was a reck- eless *liver; also* a procrastinator, for whose procrastination, however, fear of anything bearing the name of "operation," perhaps also a chronic scarcity of money, acted, to a great extent, as cause.

The use of the india-rubber bulb, exhausted and then fixed to the catheter after its introduction *as far as the stricture*, succeeded in this case also. Once afterwards, when the passage had become closed again, and his experienced efforts to relieve himself had failed, so that the case presented apparently the same necessity for the use of the suction apparatus as before, percussing the bladder after all other efforts had failed (this was done for the purpose of ascertaining the extent of the distension) alone started the flow, and he became relieved without any further operation. This latter occurrence suggested the probability of the retention having resulted from some slight mechanical obstruction inside the stricture which became dislodged by the percussion, and thus allowed the water to pass. Whether such a condition of things would interfere with the success of the method of treatment by the suction conducing rather to retain the obstruction *in situ*, must be the subject of further experience.

The attention of surgeons is called to this method of relieving retention of urine from impermeable strictures, when other methods have failed, with the hope that further careful experiment will develop its value fully.

No expensive apparatus is required, a simple catheter perforated at its extreme end (although a hollow tube without side openings through which the lining of the urethra can be acted upon would be better), and a hollow india-rubber ball that may be connected with the catheter by means of a short piece of india-rubber tubing, is all the paraphernalia required; and I doubt not that its use will relieve much suffering, and give time for subsequent calm and deliberate treatment for the permanent cure of the stricture itself.

Introduce the tube, or catheter, as far as the stricture; let the suction be gentle and continuous, and let the patient assist, by a very easy effort, to void urine during the operation.

It is possible that a common syringe of any kind might be used for this same purpose in place of the india-rubber bulb, but as more is expected from the persistence of the force working continually in the right direction than from its violence, there seems to be good reason for adhering to the original method. A common syringe, used with rashness, is capable of doing injury in cases of this kind.—*Lancet, March* 2, 1872, *p.* 286.

68.—VERTEBRATED PROSTATIC CATHETER.

By Dr. T. H. Squire, Elmira, New York.

[Formerly a rigid metallic instrument of large curve was the favourite instrument in cases of prostatic disease. There are, however, many cases in which only men of the greatest skill can introduce such an instrument; and we believe of late years that a gum-elastic, or india-rubber catheter is far more frequently employed.]

In the consideration of the subject of catheterism, in prostatic retention, one fact occurs, possessed of the utmost significance, namely: *the seat of embarrassment and failure is, always, in the deeper part of the canal, very near to the bladder.* Why is it that the rigid catheter invariably passes easily through the anterior three-fourths of the canal, and so frequently meets with impediment or failure in the posterior or vesical fourth? The reason is: The anterior part of the urethra being perfectly flexible, adapts itself to the curve and direction of the instrument, and there is no resistance, whilst the deeper part of the canal, being rigid and fixed, cannot adapt itself to the form and direction of the catheter, and as soon as the axis of the instrument ceases to be in correspondence with the axis of the urethra, that moment advancement is difficult or impossible. If the discrepancy be considerable, and too much force be used, a *perforation* of the canal is sure to be the result. The gum-elastic is better than [illegible] for, being somewhat flexible, it has a limited power of adaptation of the rigid urethra. But both instruments are faulty n this, that they cannot accommodate themselves to the direction of that part of the canal which is rigid and often tortuous in its course. In this connection the following question arises: If a rigid instrument will go with ease through a perfectly flexible canal, why will not a *perfectly flexible* instrument pass with ease through a rigid canal? Inasmuch as the canal, in the one case, readily conforms to the instrument, the instrument, in the other case, ought readily to conform to the canal. When the correctness of this theory had become firmly established in my mind, I determined to construct an instrument, that, with every other essential requisite for a prostatic catheter, should in its vesical extremity possess this quality of *perfect flexibility*. What, then, are essential requisites for a prostatic catheter? They are as follows:—

1. It should have a good calibre for the flow of urine.
2. It should have a smooth external surface, that it may glide in the canal without friction or pain.
3. The vesical portion should be perfectly flexible and floating, that the beak may readily follow the curves of the rigid portion of the urethra.
4. It should have linear or longitudinal stability, that the propulsion of the hand at the pavilion may be transmitted without loss to the beak.
5. It should possess transverse stability, that its calibre may be preserved.
6. It should have such strength as to make it secure against accidental breaking in the canal.
7. The flexibility of the vesical portion, perfect in degree, should be limited in extent, that the instrument cannot double on itself in the canal.

In order to secure all these requisites, the instrument should be made of silver or some other hard material, and the flexibility of the vesical portion obtained by means of a series of perfect joints or articulations, giving it a vertebrated appearance.

The rigid portion, which may be called the shaft of the instrument, is from eight to nine inches in length, and number ten in size. At its outer extremity it resembles an ordinary silver catheter. The opposite extremity is ovoid in form, and has a central opening reduced to number five in diameter. The latter extremity or floating part of the catheter, is composed of from twelve to sixteen vertebræ, or joints, including the beak. Each vertebra is one-fourth of an inch in length, and bears some resemblance to a common tea-cup, having an open mouth surrounded by a thin, smooth, well-rounded lip, and an opposite extremity which is precisely like the ovoid extremity of the shaft. The beak has an open mouth like each of the vertebræ, and in other respects is similar to the beak of an ordinary catheter. The shaft and the various sections of the floating part are held in loose apposition by means of an internal chain and rod —chain in the floating part, and rod in the shaft—the two forming a continuous ligament of connection. The end of the chain is secured within the beak, and the rod terminates with an eye at the pavilion, and through this eye passes a small screw-bolt, the thumb-piece of which is one of the rings or ears, at this extremity of the instrument. Or the rod may be secured in the following way: The rod can be made to terminate in a right angle with a ball at its end, the latter is then passed through a fine slot very near the end of the tube, and the short arm given a quarter turn and secured by a cap having a bayonet slot in the side, which cap is slipped on the end of the tube. This cap should have a slot in the opposite side to admit the neck of a stationary ball.

The exact length or tension of this internal ligament is a matter of the utmost importance, for upon this depends the essential virtue of the instrument. There is but one degree of tension that is appropriate. If the tension be too great, the flexibility of the floating part is seriously abridged; if it be too slack, the tubal integrity of the instrument is lost. In order to be exactly right, the chain should be as lax as may be, and not allow the ovoid extremity of any vertebra to escape from the mouth of its fellow. In this condition perfect and delicate flexibility is secured, and, at the same time, the integrity of the tube is preserved.

Another feature in the construction of the instrument requires special comment, namely: the precise form of the ovoid extremity of the shaft and of the vertebræ. If the contraction of this extremity be too abrupt, approaching a spheroid *form, it is objection*able; and on the other hand, if it be not sufficiently abrupt, if it be too conical, it is equally at fault. The smaller extremity of an ordinary hen's egg supplies an appropriate illustration of the form required.

About one year ago, I had some experimental instruments made, and with the aid of quite a number of professional friends, began to submit the catheter to practical testing in typical cases of prostatic retention, and, although the information thus sought has been slow and difficult of acquirement, nevertheless, the results, thus far, have been exceedingly gratifying, as the following cases will show :—

Case 1.—Reported by Dr. Caro, of New York. M., æt. 60, corpulent, average weight 300. Temperament sanguine, temperate and healthy. Six months ago he commenced to have difficulty in passing urine. Upon examination, I found he was troubled with prostatic enlargement. I tried a No. 8 elastic catheter, but could not pass it. A common silver catheter of the same size was passed with great difficulty, producing laceration of the prostate, causing considerable hemorrhage, distress, and delay in using any other instrument. I obtained from Messrs. Stohlman and Co., one of your vertebrated catheters No. 8.

After four days I attempted a second catheterism. To my great satisfaction, and with very little difficulty, I succeeded in passing it to the bladder, affording immediate relief to my patient by drawing two quarts of bloody urine. Four days of practice made him acquainted with the manipulation of it, and ever since he has been enabled to relieve himself from the distress of want of free micturition.

Case 2.—Reported by Dr. Walter Booth, of Boonville, New York. Was called four miles to see a man, aged 55, and found him suffering from retention of urine. There was great distension of the bladder and very severe pain. At once attempted to introduce the common silver catheter, but failed to reach the bladder. Returned for the vertebrated instrument, and within one hour was by the side of the patient again. The instrument passed at once and without the least difficulty, and, of course, relieved him of his sufferings. I continued the use of the new instrument five or six days, when the patient was discharged cured.

Case 3.—Reported by Dr. J. L. Stewart, of Erie, Pa. The vertebrated catheter was used with great satisfaction; succeeded in a case where others had failed with the common instrument.

Case 4.—Reported by Dr. Wm. E. Johnson, of Waverley, New York. Mr. ——, æt. 56. After a long ride on the cars, had retention of urine, severe pain, tenesmus, &c. Employed the silver catheter, which with difficulty was introduced and with very unsatisfactory results; but little urine escaped, and upon the withdrawal of the instrument, as well as during its presence in the canal, blood escaped.

In this dilemma the vertebrated catheter was used, and between two and three pints of urine drawn off with no pain, and very little blood following.

Next day used the catheter three times, and in each instance with ease and satisfaction. Patient then used the instrument himself, till the power to urinate returned.

Case 5.—Reported by Dr. F. Abbott, of Elmira, New York. Was called to a patient, aged 60 years, suffering from an attack of prostatic retention of urine. Failing after repeated efforts with the usual gum-elastic and the rigid catheters, recourse was had to the vertebrated prostatic catheter, which entered the bladder with perfect ease, and three pints of urine flowed away. Patient afterwards used the instrument himself, till the natural function was restored.

Case 6.—Reported by Dr. J. K. Stanchfield, of Elmira, New York. Retention of urine in a man 57 years of age. Unable to pass a common silver catheter, vertebrated catheter readily, and without pain, entered the bladder as if by it own gravity.

Case 7.—Reported by Dr. William Woodward, of Big Flats, New York. W. R., farmer, æt. 66 ; after a long ride was seized with complete retention of urine He was speedily relieved by the use of the common silver catheter; but, requiring the operation to be repeated more than twice a day, the neck of the bladder became inflamed, and the introduction of the catheter was attended wit

n. On the sixth day of his illness the vertebrated catheter was used, and s passed with much less difficulty and pain than attended the use of the nmon catheter. On the tenth day the patient began to use it himself, and continued so to do as long as an instrument was required.

Case 8.—Reported by Dr. H. Lyle Smith, of Hudson, New York. Patient years of age, with prostatic retention of urine. The common gum-elastic heter was used without difficulty by the surgeon; but the patient, in empts to introduce it himself, could not succeed. At length the vertebrated heter was substituted (Sept., 1870), and its introduction was attended with little pain, that the patient used it himself for a long time with the greatest isfaction. Death finally occurred from gradual exhaustion. The new heter was employed as long as any instrument was called for.

Case 9.—Reported by Dr. Hutchison, of Brooklyn, New York. Dr. H., in uary, 1870, was called as counsel to a case of retention of urine from arged prostate, and being unable to go, sent his assistant Dr. Wilson. The o physicians who had charge of the case, one a gentleman of large experience, d tried ineffectually for an hour or two to enter the bladder. Dr. W. introced a No. 7 or 8 catheter, with but little difficulty, and subsequently the tient tried to introduce the catheter, but was unable to do so. He (the tient) did, however, introduce the vertebrated catheter with the greatest e, and continued to do so three times a day as long as it was required.

Case 10.—Reported by Dr. Hutchison, of Brooklyn, New York. Mr. ——, . 60, had prostatic retention, which the attending physician was unable to ieve by the catheter after two hours' effort. Dr. Wilson then saw the case n. 1871, for Dr. Hutchison, and introduced quite readily a No. 8 silver heter. On the following day Dr. H. saw the patient, and introduced a No. readily, and twenty-four hours later he requested the patient to try the verrated instrument. The patient was timid and agitated, but passed the cathe- to the prostate, where it stopped; the *slightest* push, however, by Dr. H. sed it to enter the bladder. It was subsequently introduced by a son-in-law the patient without any difficulty.

Case 11.—Reported by Dr. Caro, of New York. A healthy man, æt. 81, ighing 250, had retention of urine from enlarged prostate gland. Tried a . 8 silver catheter to relieve him of the accumulated urine in the bladder, d probably by force, could have passed it, but finding an obstacle at the ostate, and the patient suffering great pain, the instrument was withdrawn, d a No. 8 vertebrated catheter was passed without the slightest inconvenience, ieving him of about three pints of brown-coloured urine. Afterwards the se accomplished the operation regularly, and always with ease and great isfaction to the patient.

Case 12.—Reported by Dr. May, of Corning, New York. Young man suffer- from distended bladder, with inflammation of the bowels. Attempted to ieve the retention with No. 9 silver catheter also with No. 6; but did not ceed with either. With the vertebrated instrument, No. 10, had not the st difficulty in reaching the bladder. Nearly two quarts of acrid urine were wn to the great relief of the patient. The instrument was used once afterds, and then the natural function was restored.

Case 13.—Reported by Dr. May, of Corning, New York. Having occasion tap a man for ascites, and desiring, as a precautionary measure, first to cuate the bladder, the vertebrated prostatic catheter was used, which ched water with the greatest facility, inflicting no pain whatever upon the ient.

Case 14.—Reported by Dr. Bates, of New Lebanon, New York. A mechanic from a building, was badly injured, kidneys wounded, and bladder filled h urine and blood. Tried the ordinary catheters, but could not get into bladder. Then tried the vertebrated instrument with perfect success. Had use it many times. Patient recovered. Attributed the life of the patient to use of this catheter.

Case 15.—Reported by Dr. Smith, of Hornby, New York. Was called to O., of Orange, N. Y., May 5th, at 4 o'clock p.m., who was suffering from retion of urine of twenty-two hours' standing. Had suffered much pain for ieteen hours. The case was so evident, and pain so severe, I concluded to lose no time with fomentations, &c. Passed a gum-elastic catheter, No. 8, deep into the urethra, but met with resistance near the prostate. The instrument would invariably turn to the right, showing the channel to be tortuous and uneven. Carried my index finger in the rectum, and found prostate gland very much enlarged, heat of parts much above natural standard, and a good deal of soreness. I pronounced the case acute prostatitis with hypertrophy. By retaining my finger against the membranous portion of the urethra, or nearest portion of prostate gland, I succeeded with much difficulty in reaching the bladder, and drawing off about twenty-four ounces of high-coloured urine, which of course relieved the patient; gave opium.

May 6th. Called at M., and found the patient suffering from retention as on the day previous, but not so severely. Introduced catheter, but with a little less trouble, and drew sixteen ounces of urine.

7th. Was called very early in the morning with message that patient was suffering very much. I saw him at seven o'clock a.m., and attempted to introduce catheter as before, but failed; heat in the rectum intense. Gave an opiate, and sent for an experienced and skilful physician from Monterey, who reached the patient at 10½ a.m. He made a persevering effort to introduce various instruments, but failed to reach the bladder. Ordered nauseating medicines, morphia, hip-bath, &c. At 2 o'clock p.m., I succeeded in reaching the bladder with a No. 8 silver catheter, with a long, deep-sweeping curve, and drew 1¼ pint of urine.

8th, 9th, 10th, and 11th. I introduced gum-elastic catheter with much difficulty, generally without the stilet. Could pass the gum catheter with less difficulty than the silver; although the silver instrument was a remarkably good one for the occasion, as it was long and deeply curved. The patient, at every attempt to pass the catheter, became very much exhausted, and I could see he bore each successive operation with more difficulty, as he grew more and more exhausted, his strength constantly failing.

12th. Called on Dr. May, of Corning, and through his kindness obtained a vertebrated silver catheter. On exploring the urethra, found the same zigzag course of the canal. Could with some difficulty reach the prostatic portion, but then came the obstruction as before, and, as it worried my patient so much, I threw the old instruments all aside and attempted the introduction of the new catheter. It being about three sizes larger than those I had before used, I used but little force, giving the passage sufficient time to dilate, but, in less than one-half the time required before, I had the satisfaction of reaching the bladder, followed by a copious flow of urine. Here was success I had hardly dared to expect. At every attempt to introduce the other instruments more or less blood was sure to flow. But in introducing this new instrument no hemorrhage whatever followed. The patient was extravagant in delight. In no case afterwards could I induce him to let me pass any other instrument, I introduced this twice a day, for three or four weeks. Since that time to the present date, he has been able to pass urine with more or less difficulty without the aid of any instrument whatever.

Occasionally when using the catheter I could detect small calculous deposits. Since he has been able to void urine naturally, more or less gravel has escaped with the water.

Case 16.—Reported by Dr. Gere, of Chemung, New York. G. C., æt. 70. In the month of March he was taken with complete retention of urine. A physician was called, and the common catheter was used with considerable difficulty and pain. The vertebrated catheter was afterwards tried, which proved so easy of introduction that the patient himself has since used it regularly, and is still obliged to use it twice daily. The prostrate gland is enormously enlarged, almost entirely filling the pelvic cavity, and seriously obstructing the passages from the bowels.

Case 17.—Reported by Dr. Purdy, of Elmira, New York. Was called in counsel to see Mr. E., æt. 70; found him in a state of extreme suffering from prostatic retention. Having the vertebrated catheter with me it was used at once, with the utmost facility and freedom from pain. A relation of the patient afterwards used the instrument, till the man was able to urinate.

The testimony of these cases confirms the correctness of the theory or principle embodied in this new catheter. It settles the question that, in the introduction of a catheter for the relief of prostatic retention, the *urethral walls*, and not the surgeon, should guide the beak of the instrument. I do not hesitate, therefore, in pronouncing this new catheter *a real and very important improvement* in the art of modern surgery.

My very grateful acknowledgments are due to those members of the profession who have kindly assisted me in testing the practical merits of this new catheter.

The vertebrated prostatic catheter may be obtained from J. H. Gemrig, No. 109 South Eighth Street, Philadelphia.—*American Journal of Medical Science*, *Oct.* 1871, *p.* 393.

69.—CATHETERISM; SYNCOPE; EMBOLISM; DEATH.

By Francis D. Lys, Esq., Bere Regis, Blandford.

The following case, though happily rare, shows the necessity for increased caution even in the slightest surgical procedure upon those patients in whom we have reason to suspect any cardiac lesion.

J. S., a very stout man, aged sixty-two, had always enjoyed good health till within the last few months, during which he has suffered from atonic dyspeptic symptoms, for which he consulted me on October 5th, and again on the morning of the 8th, when he was somewhat better, though his pulse was weak, and he complained of shortness of breath when walking up-hill. While driving home he wanted to pass urine, but was prevented by the presence of a female. On his arrival he had lost the power to relieve himself, and came to me again in the evening.

I passed a No. 10 catheter for him whilst in the recumbent position, and, having done so, he sat up in order that he might the better empty his bladder, which contained about fifteen ounces—certainly not more. He now complained of feeling faint, which induced me to lay him down again and to give him stimulants. In spite of this, he remained almost pulseless for more than an hour, and it was quite two hours before he sufficiently rallied to admit of his removal from my surgery.

Oct. 9th. He has passed urine twice without difficulty. He has been sick several times. His pulse is very weak and intermittent. I gave him milk, beef-tea, and brandy mixture, and ordered a sinapism to the epigastrium.

10th. Pulse scarcely perceptible. He had several convulsive attacks, and died in the evening.

I regret that no post-mortem examination was practicable. I believe, however, that it would have revealed that the man had a fatty heart, and that during the syncope caused by the shock of passing the catheter a clot formed either in the heart or aorta, which prevented his recovery. Nobody accompanied him except a little boy to drive him; it is therefore fortunate that he rallied sufficiently to be removed home.

From this case, it seems to me that when there is reason to suspect any weakness in the heart, it is advisable to enjoin the recumbent posture till after the removal of the catheter, if not for a short time longer.—*Lancet*, *Nov.* 25, 1871, *p.* 746.

DISEASES OF THE EYE AND EAR.

70.—ON THE TREATMENT AND ORIGIN OF PURULENT OPHTHALMIA IN NEW-BORN CHILDREN.

By R. Liebreich, Esq., Ophthalmic Surgeon to St. Thomas's Hospital.

[The treatment recommended by Mr. Liebreich, consists]
First, in careful cleansing; secondly, in the local application of cold; and, thirdly, in cauterisations with mitigated nitrate of silver (one part of nitrate of silver and two parts of nitrate of potassium fused together). Allow me now to speak of this in a somewhat elementary and detailed manner. As regards the cleansing, it is above all necessary to explain to the attendants the importance of it, for ordinarily, the fear of injuring the child prevents them from properly opening its eyes in order to remove the secretion. I do not recommend syringes, so generally used, for cleansing—first, because they are dangerous to the attendants, who, in using them, may easily have some of the contagious matter spattered into their eyes; secondly, because by this method the secretion is not completely removed, even after pouring much water over the child. A fine sponge, if you can rely upon it being kept clean, or if not, small pieces of moistened linen rag, are preferable for effectually cleansing the conjunctiva.

The application of cold, if made in a careful and suitable manner, is of great assistance in the treatment. In the mildest form of the disease, this application alone may even effect a cure in a few days. It is then only necessary to apply, for several hours a day, small linen rags, moistened by being dipped in cold water, changing them constantly. In the more serious cases, on the contrary, when there is much swelling, redness, and heat in the eyelids, and a copious discharge of thick yellow purulent secretion, it is necessary to apply, day and night, without intermission, small rags, previously placed upon ice, and to renew them continually. Later, when the elevated temperature begins to fall, the applications may be discontinued during the night, and gradually reduced, according to the course of the disease. In order to prevent the child from taking cold, it is necessary that the rags should be of a size merely to cover the eyelids without touching the bridge of the nose, and not to make them too wet.

We now come to the real curative treatment—that is, the cauterisation. It should only be done with mitigated nitrate of silver. The eyelids must be reversed one after the other, and, after being carefully cleansed, touched with the caustic, which must be passed over all the swollen and red part of the mucous membrane. Before placing the eyelids in their natural position, it is necessary to neutralize the free nitrate of silver by a drop of salt water. For the first few days only of the disease, we may restrict the treatment to the application of cold, and then commence the cauterisations; repeat them once a day, never more frequently, and, after an evident reduction of the disease, once every two or three days. It is important not to repeat the cauterisation until the scar of the previous one has disappeared.

To drop weak solutions of nitrate of silver into the eye is not advisable, even in the mildest forms; for the graver forms it is completely insufficient. Cauterisation with pure nitrate of silver ought never to be used, neither in this nor in any other disease of the conjunctiva, as it is impossible to limit its effects. The slightest touch with pure nitrate of silver, in fact, produces a strong cauterisation, not only not limited to the surface of the mucous membrane, but attacking the subjacent connective tissue. The cicatrisation which is the result of such cauterisations produces a permanent irritation of the eye, which cannot be removed by any possible means. None of the other known caustics can *replace* the mitigated nitrate of silver in the treatment of purulent ophthalmia. *Let nobody* avoid the trouble of preparing a pencil of nitrate of silver himself *if he has not* a suitable one already prepared at his disposal. The sole diffiulty lies in procuring an iron mould, into which the mixture of one part of itrate of silver and two parts of nitrate of potash is to be poured after having een melted over the fire. This difficulty could be avoided by immersing an ron wire repeatedly with one end into the melted mass until, on cooling, a ufficiently thick layer of caustic adheres to it. That is the procedure which I ormerly recommended for caustic probes in cases in which it is desirable to auterise with a very fine point or in a very narrow canal (lachrymal canals, achrymal sac, ciliary roots, &c.).

In the most difficult cases, cleanliness, cold, and cauterisation are sufficient, nd enable you to form a good prognosis. The case is, however, different hen children come under your treatment, after a more or less great part of he cornea has already been destroyed, the iris projecting, the capsule of the ns injured; here the prognosis depends entirely, in the individual case, on he existing destructions. Ulcers of the cornea of no great extent, and not too ear to perforation, allow of a still favourable prognosis; but it becomes more nfavourable already, when perforation has taken place, and the iris projects. here is a decidedly bad prognosis if almost the greater part of the cornea had een destroyed before the child came under your treatment.

In such grave complications, it is sometimes desirable to cut off the projectg iris, or to let out the lens. But I advise you generally to abstain from ose operations, which require considerable experience and practice. Direct our attention, then, exclusively to the quick cure of the affection of the ucous membrane, whatever the complication may be which neglect and false eatment have caused. You will be astonished to see how quickly even the ravest complications come to a comparatively favourable cure, as soon as the ossibility of it has been given by the removal of the affection of the mucous embrane. I have shown you an instance of it in a child, in one of our preious meetings; it was brought as an out-patient, after it had suffered for ore than three weeks from a very high degree of purulent ophthalmia, and ad been treated during all that time with warm milk.

If you inquire into the cause why the use of warm milk or of moist heat in eneral, in spite of its evident mischief, and in spite of the repeated warnings f ophthalmologists, could have gained ground in all countries, amongst the ublic as well as amongst general practitioners, you will find that it originated an erroneous supposition on the nature and origin of the affection.

It has always struck me with what haste relatives or nurses declare, as soon s they bring a case of purulent ophthalmia for treatment, that the affection is ue to having caught cold. The same will be told you if you have to examine rown-up people, where you will find the results of an ophthalmia from which e individual has suffered immediately after his birth. In addition to the rroneous supposition of having caught cold in the eye, comes the wrong priniple of treating that cold by warmth; and these errors have preserved themelves for generations, in spite of all the opposition against them. Let us, erefore, first of all, understand the pathogenesis, of which, in my opinion, ere is not the slightest doubt.

You may accept it as proved that any pathological secretion of any mucous embrane, be it catarrhal or purulent, brought into the conjunctival sac, will ause a pathological condition of the conjunctiva, which will, on the one hand, ccording to the nature of the secretion, and on the other hand, according to e peculiar disposition of the individual, assume a variety of appearances. On oing through the maternal passages, the eyes of the child are exposed to inoulations from two quite different sources. They are—first, gonorrhœal secreons from the urethra of the mother (this causes the most formidable cases of urulent ophthalmia, but only an extremely small percentage of the cases are ue to that cause); by far the greater majority of cases are due to inoculation rom the second of the two sources—namely the secretion due to the increased unctional activity in which the maternal passages are in the last month of regnancy. According to the nature the secretion had taken in a given case, nd according to the quantity of it introduced into the eye of the child, we hall see a series of affections ranging from simple conjunctivitis up to those orms of purulent ophthalmia which in no way are different from those caused y *gonorrhœic secretion.*

Whatever cause may have produced the disease, and whatever degree it may have reached, let me repeat to you it must be cured without leaving any serious pathological changes by a rational treatment, if early directed against it. Let us hope that rational principles in regard to this disease will soon be so generally spread that blind asylums will cease to be filled, as is now the case, by individuals who have lost their sight by purulent ophthalmia.—*Medical Times and Gazette, December* 23, 1871, *p.* 763.

71.—ON STRUMOUS OR PHLYCTENULAR OPHTHALMIA.

By Dr. H. R. Swanzy, Ophthalmic Surgeon to the Adelaide Hospital, Dublin, and Assistant to the late Prof. Von Graefe.

This disease is called strumous, because of the condition of the general health which it commonly attends; and phlyctenular, because the little vesicles, which form on the conjunctiva or cornea, as the case may be, are termed phlyctenulæ. It is a very common disease—a large proportion of the out-patients of this department of the Hospital labouring under it. Its most common victims are children between the ages of three or four, and eleven or twelve; but it is by no means uncommon in still later periods of life. In many of the text-books you will find phlyctenular ophthalmia divided under the two heads of "Diseases of the Cornea," and "Diseases of the Conjunctiva." The arrangement is very absurd, for the affection is the same whether it appear on the cornea or conjunctiva, it is treated in a very similar way in either case, and, moreover, it most frequently attacks both tissues simultaneously, or passes from one to the other. The vesicles, which make their appearance in the disease, vary from the size of a pin's head, or somewhat larger, to very small specks like grains of fine sand, which are distinguished by the naked eye with difficulty, unless when a great number of them occur close together, as is indeed usually the case.

Perhaps the most common form of phlyctenular ophthalmia is that in which the conjunctiva, close to the margin of the cornea, becomes swollen at on place, and there small points like grains of sand, make their appearance. There is a good deal of conjunctival and sub-conjunctival injection of vessels in the neighbourhood, and the remainder of the conjunctiva participates more or less in the hyperæmia. At this stage there is often a good deal of tearing and photophobia. The disease may cease here, or, after a few days, we may find a number of very fine phlyctenulæ in the adjacent part of the cornea, giving it the appearance as if sprinkled with powder. If the cornea become affected, the tearing and the photophobia increase.

Another common form of the disease is the solitary phlyctenula, which appears on the conjunctiva as a rather large vesicle; it may be situated at some distance from the cornea, or close to its margin. No matter how many of these may be present, they never become confluent. This form of the affection frequently produces but little irritation of the eye, so long as it is confined to the conjunctiva: when, however, one or two of these phlyctenulæ are situated close to the cornea, circumscribed opacities are apt to be formed in the cornea itself, and we thus have one of the most frequent and severe forms of phlyctenular corneitis, attended with distressing symptoms.

A third form of strumous ophthalmia is the broad phlyctenula, situated on the conjunctiva, usually at some distance from the cornea. It is uneven on its surface, appearing to be formed of an aggregation of small infiltrations. It is much rarer than the two forms I have already described to you, and it is less frequently associated with corneitis; but, on the other hand, it is more apt to strike deep, and produce inflammation of the sclerotic.

Still another form of the disease is termed "fascicular corneitis." Although I have seen almost as many cases of this in Germany as of any of the other forms, still I have not, as yet, met with one in this country, and I shall not [illegible] by a description of it—my great object being to mak

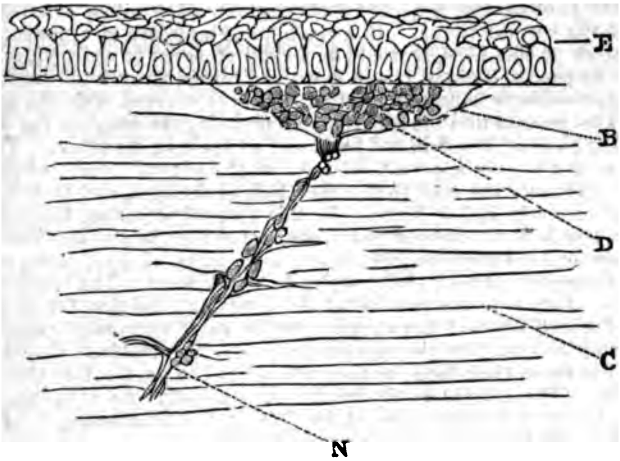
acquainted with disease of the eye as you are most likely to come
s it.

hink you will find all cases of phlyctenular ophthalmia referable to one or
of the forms I have described. They will, of course, present different
res according to their different degrees, according to the way in which
have been treated or mistreated, and according to the extent to which they
been neglected. Injudicious treatment may alter the natural appearance
y disease, so as to make the diagnosis difficult enough. A common error,
st which you must guard, is that of confusing pustular with phlyctenular
ıalmia. The former is most frequently met with as a sequel to small-pox,
s one of the most destructive affections to which the eye is liable, differ-
thus, widely from phlyctenular ophthalmia in its prognosis. Its treat-
also is quite different, and pathology will, doubtless, sometime demon-
, that the two diseases are distinct in their nature.

e greatest danger in phlyctenular ophthalmia is, that when an infiltration
ə cornea ulcerates, the process may lead to perforation of that membrane.
is, however, of comparatively rare occurrence. What we commonly have
ır, are opacities of the cornea, left at the places where infiltrations and
s were situated. Even mild cases of phlyctenular corneitis, frequently
opacities behind, which may be recognised with the aid of the oblique
ination. The more severe cases, however, accompanied with intense infil-
ns, particularly if these had assumed a purulent character, are apt to
white opacities behind, which are very disfiguring, and very detrimental
ion.

rish now to explain to you what is known of the pathology of phlyctenu-
phthalmia, before we consider its treatment. It is only lately, indeed,
anything has been made out on this point, for, as you may suppose, it is
asy to get preparations of such a disease, in order to examine it micro-
cally. Professor Iwanoff, of Kiew, in Russia, had the good fortune to ob-
three specimens of phlyctenular ophthalmia, *post-mortem*, of which he has

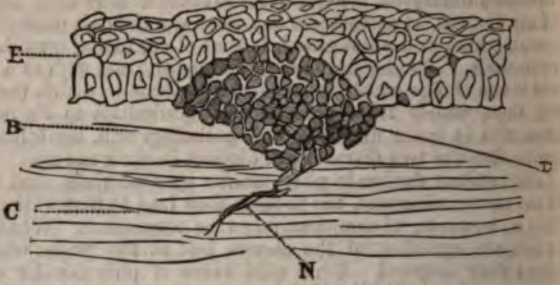
FIG. I.



ı me the microscopical preparations. These demonstrate that the phlyc-
e are collections of small, round cells, lying immediately under the epithe-
of the cornea or conjunctiva. In cases of small phlyctenulæ, the epithe-
covering them remains normal; in the larger ones it is usually destroyed
, commencing ulceration). The membrane of Bowman, on which the
tenulæ in the cornea lie, was always found normal. You may perceive
se drawings the remarkable path by which the cells arrive between Bow-
membrane and the corneal epithelium, namely, along the course of nerve
ents, *which pass from* the deeper parts of the cornea, and penetrate the

membrane of Bowman to reach the epithelium. You are, of course, aware that the cornea, and especially its superficial epithelium, is richly supplied with nerves. This course of the cells along the nerves is interesting, as it explains, by pressure on these filaments, the blepharospasm so frequently present, and which is often referred altogether to photophobia, attendant on the affection. If the distressing sensations of the patient were merely owing to the fact that the light hurts the eye, they ought surely to disappear when he is placed in a dark room. This is, however, not the case, for the blepharospasm is then alleviated only, but does not cease altogether. The blepharospasm must be regarded much rather as a reflex action, caused by irritation of the corneal nerves, in a manner similar to that by which an ordinary nictitation is produced. Iwanoff did not make out from whence the cells originated. In appearance they are similar to lymph corpuscles.

Fig. II.—(After Iwanoff.)



E. Corneal epithelium
B. Membrane of Bowman
D. Collection of small round cells (Phlyctenula)
C. True cornea
N. Nerve

You will find little difficulty in the treatment of mild cases of phlyctenular ophthalmia; the instillation of a few drops of atropine, with the insufflation of a little calomel, and protection of the affected eye from exposure, will in a few days suffice for the cure. Not so, however, those cases which are attended with severe photophobia, pain, and lachrymation, all denoting extensive infiltration of the corneal tissue. If I could paint a picture of a patient with a severe attack of this kind, it would be a child of seven or eight years of age, led in by its parent, its head bowed down and turned away from the light, the eyelids spasmodically squeezed together, the eyes covered with the hands, or the knuckles pressed into the sockets, and probably, to complete the drawing, a half-eaten piece of bread in one hand and an apple in the other. The diagnosis of such a case is apparent, as soon as the patient comes within sight. Nothing will induce the child to give us a look at the eyes, and in order to get it, recourse must be had to force. In this state of irritation, the only treatment admissible is an antiphlogistic one, consisting in the instillation of a dozen drops or so of atropine (sol. sulph. atrop. gr. iv. ad ℥ j.), in the course of the day, purges of calomel, and restraint in a dark room. The blepharospasm is a serious obstacle to the removal of this irritable condition, for it not only impedes the instillation of the atropine, but in itself encourages congestion of the eye, and thus sustains the morbid process. The children should not be permitted to lie on their faces, as they are inclined to do, for this also induces congestion. The antiphlogistic treatment, especially the atropine, and an attentive nurse, will soon succeed in abating this distressing condition of things; but still there are some cases which will resist.

In these obstinate cases, a seton in the temple is a favourite remedy with some of the London ophthalmic surgeons. For my own part, I have no experience of the seton in phlyctenular ophthalmia. I can quite imagine that it is a very effectual mode of treatment, but I cannot help thinking that it is unnecessarily severe. I have had many cases of the worst phlyctenular ophthalmia under my care, but I have not yet come across one which demanded such a treatment; nor, unless indeed the phylctenular ophthalmia which occurs in London, exceed in virulence anything which exists in Germany or here, can I believe that such cases present themselves. When the disease resists the treatment I have described to you, you will commonly find, as I have already mentioned, that the blepharospasm is the principal enemy with which you have to contend. The infiltrations in the cornea give rise to it, and it then causes congestion of the eye, which again, in its turn, encourages the formation of fresh infiltrations, and so you have a vicious circle, in which it is necessary to break a link before you can make any progress. We have a very simple means for the correction of this excessive blepharospasm, which was always adopted by my lamented master, Von Graefe, and which I have never known to fail, if carried out properly and systematically, namely, the submersion of the patient's *face* in a basin of cold water. The child is raised up by two nurses, one supporting the trunk and hands, the other holding the legs, while the surgeon takes charge of the head, and dips the face under water, holding it there for about ten seconds; the patient is then allowed to take a breath, and the same manœuvre is repeated several times. The effect of the proceeding is magical; the blepharospasm disappears, the child lies on its back, allows the eyes to be easily examined, atropine to be instilled, and becomes in every way as docile as could be wished. The process may require to be repeated after some hours in consequence of a return of the spasm, but the latter will soon be permanently conquered. When, then, you have reduced the irritation of the eye, the question of a more active topical treatment arises, and there is one application which I can confidently recommend to you, as being little short of a specific in this disease, namely, Pagenstecher's yellow ointment.[*] About the size of a pea of this ointment is to be inserted between the eyelids with a camel-hair pencil, and, after five minutes, what still remains of it in the eye is to be carefully washed out with a soft sponge. The application is to be repeated daily, and the beneficial effect of it will be soon observable—the infiltrations becoming absorbed and the ulcers filling up. An important point in the use of this ointment is, that it be not employed until the irritative stage is quite passed. If this be not attended to, the irritation is increased and all the symptoms aggravated. The effect of this first application should be well noted, and if the eye appear more irritated next day, the further use of the ointment should be postponed for a little longer. Calomel dusted into the eye is a favourite remedy with some; but I use it only in the after-treatment, unless in mild cases. The instillation of atropine must be continued until all irritation has disappeared. In order to prevent recurrence of the ophthalmia, treatment, either with the ointment or with calomel, must be persisted in long after the affection is apparently cured; for, the most common cause of these recurrences is the too early cessation of the treatment. So long as the eyes are red and injected upon the patient's awaking in the morning, the cure is incomplete. A complication which may retard the cure and cause recurrence of the affection, would be the existence of a stricture of the nasal duct; you must, therefore, be on your guard against it.

There is no doubt that this form of ophthalmia is most frequently found in children of a strumous habit, indicated, in well-marked cases, by enlarged glands in the neck, a red and bulbous nose, and a swollen and overhanging upper lip. I am inclined to think that in the Profession, the influence of the constitution upon local diseases in general is often over-estimated, and that the result of this is to encourage the error among us of our consoling ourselves for our inability to cure certain diseases, because they are supposed to depend upon an abnormal condition of the constitution; whereas, in fact, perhaps, the fault lies in the unsuitability of the local treatment employed, in the mode in which it is applied, or in some local complication which should be corrected. I believe that the great thing for the cure of phlyctenular ophthalmia, and for the prevention of its recurrence, is the local treatment, and I do not think (I confess I have never seen it tried) that any constitutional treatment would alone effect the cure. At the same time, it is most important that you should attend to the general health of these patients. You must look after the digestive organs and regulate the diet. Above all things, the constant chewing of apples, cakes and bits of bread must be forbidden. It is a habit which is almost universal with these children, and one which is often encouraged by the parents, who think it a sign of robust health. The meals must be given at stated hours and be of a plain and wholesome kind; but between times the stomach must have leisure to digest its contents and to rest, and must not be kept in a state of irritation by having fresh work thrust upon it every now and then. Cod-liver oil, internally, will suggest itself to you all as specially indicated. Von Graefe was very fond of prescribing what he called Plummer's powders, containing calomel and golden sulphuret of antimony in equal parts.

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[*] I have only been able to have this ointment properly prepared in Dublin by Mr. C. R. C. Tichborne, of the Apothecaries' Hall of Ireland. He has given me the following note of the process:—"The precipitated mercuric oxide was made by me in the following manner,—Corrosive sublimate was dissolved in hot water, and this solution was precipitated with a solution of potash. The latter should only be added until all the oxide is precipitated. The mercuric oxide falls as a yellow powder. It must be carefully washed with distilled water, and dried at 100° C. The ointment is prepared by rubbing up one part of this precipitated oxide with fifteen of unscented cold cream. If properly prepared, this is just the right consistency. It should, however, be genuine cold cream, and not what is ordinarily sold as such, which is only an ointment. *That is to say, it should* contain water, and be made with unbleached almond oil."

In conclusion, I must warn you against the terrible mistake which is prevalent, that the subjects of phlyctenular ophthalmia "grow out of it," and that it is therefore unnecessary to take any measures for its cure. And so, indeed, they do "grow out of it;" but with their corneæ in so nebulous a condition that their future prospects are often seriously affected in consequence.—*Medical Press and Circular*, *Nov.* 29, 1871, *p.* 476.

72.—TREATMENT OF CONICAL CORNEA BY REMOVAL OF THE TOP OF THE CONE.

By C. Bader, Esq., Ophthalmic Assistant-Surgeon, Guy's Hospital.

[In 1863 the first case was operated on in this manner, and up to the present time nine cases altogether have been thus treated. The operation consists in removal of the top of the cone, and the results have been favourable.]

1. Place the patient on a bed as for extraction of cataract, and bring him thoroughly under chloroform.

2. Keep the eyelids open with a stop-speculum, so as to press upon the eyeball as little as possible, and fix the eyeball with the forceps.

3. Immediately before commencing the operation, ascertain the position of the top of the cone by turning the eye to be operated on in different directions, while strong light is thrown upon the cornea with a two-inch convex lens.

4. The subsequent steps of the operation, however they be taken, should tend to remove the top of the cone (the entire thickness of the cornea), so as to cut an opening through the cornea into the aqueous chamber. This opening, somewhat oval-shaped, is from one-twelfth to one-sixteenth of an inch in its longest diameter; it was measured immediately after removal of the apex of the cone, after escape of the aqueous humour, the cornea having collapsed and being in contact with the iris. The pupil, in all cases, was opposite the opening in the cornea; the surface of the lens could be seen bulging into the area of the pupil. In the cases in which the thread was used, the latter caused an indentation in the crystalline lens while passing across the area of the pupil.

5. *Removal of the top of the cone.*—A small, curved needle, armed with fine white or black silk, or with silver wire (or a gilded, small, sharp hook), is thrust through the cornea in its horizontal diameter, close to the portion of cornea we wish to remove. The point of the needle, after piercing the cornea, is carried horizontally across the aqueous chamber to a spot opposite the point of entrance, and again thrust through the cornea close to the portion of cornea we intend to remove. The aqueous humour escapes before or after passing the point of the needle through the cornea the second time. The needle or sharp hook is left in the cornea until the top of the cone has been removed; it helps to protect and keep back the crystalline lens. The portion of cornea (the top of the cone) situated in front of the needle is then removed as best we can. *This part of* the operation is somewhat difficult, the cornea being transparent *or nearly so,* extremely thin and flaccid, and the iris and lens being in contact *with the cornea.* The head of the needle is held in one hand, and with a

nife, or with a sharp, narrow, lancet-shaped knife, an incision is made
ıe cornea (a small flap incision, as in flap extraction for cataract).
ade the incision, the needle is let go, the small flap seized with an
s, and the rest of the cornea (of the cone) removed with the knife or
ors.

ure of the wound (opening) in the cornea.—If a sharp hook or a needle
ıread has been used for transfixing the cone, it is withdrawn after
f the cone, and the wound left open. The eyelids of both eyes are
d, kept bound up and cool with wet lint, and the patient kept in bed
days; on the third day the use of the eye not operated on is per-
hile the eye operated on is kept bound up until all redness has sub-
a needle, armed with silk thread, has been used, the needle, after
f the cone, is drawn out gently by the second opening in the cornea;
e thread, while passing across the aqueous chamber, the surface of
upil, and crystalline lens irritates those parts as little as possible.
ng in the cornea is closed by tying the thread, as is done when uniting
ıs of a wound by a suture. The suture is tied tightly. If it should
.e before completing the knot, it does not signify. The cornea, by
.e suture, is thrown into numerous folds. This folded condition
in an extreme degree in one case for nearly four weeks. Having
wound, one end of the thread is cut off close, the others left about a
an inch long, so as to assist when removing the suture. Both eyes
ool and bound up with wet lint. The patient remains in bed. The
removed on the appearance of slight chemosis and swelling of the
(See cases.)

oval of the suture.—The patient being rendered insensible (by methy-
lids are kept open without pressing upon the eyeball, the surface of
ı is well cleared from mucus, &c., and the long end of the suture
ay from the cornea, so as to stretch the suture, and the latter cut
nd withdrawn. After this, the lids of both eyes are again carefully
d up with wet lint. The use of the eye not operated on is permitted
rd day after removal of the suture. The operated eye is kept bound
ıe lint cool, until all redness of the eyeball has subsided, or nearly
ın artificial pupil is made.

other mode of destroying the apex of the cone in conical cornea—
ce, by the galvanic cautery—would, I believe, answer as well as
of the cone.

ntages of the Operation.—An opaque spot in the cornea, from where
ıad been removed. The patients were told that there would, after
ion, remain a small white speck on the eye, but that nothing except
on could improve sight. None of the patients complained of the
ent, and all were pleased with the amount of vision obtained.
essity of giving an anæsthetic repeatedly. For removal of the suture,
e artificial pupil, methylene is quite sufficient.

ıges of the Operation.—In extreme cases of conical cornea a greater
ənt of sight is obtained than by any of the known modes of treatment.
ward accident occurred in any of the cases.
ır-treatment, after removal of the suture, is very simple. The patient
be seen at great intervals.
ical cornea in all cases completely disappeared, and gave way to an
y flat cornea.

-O. M., aged twenty-four, female, delicate, thin. Conical cornea in
; cone of the left eye opaque. Iridesis had been performed in left
2, and iridectomy on both in 1864, without result. Left eye unable
ıe letters, or to guide herself. Lately attacks of ophthalmia, photo-
creased prominence of the cone. Sept. 28th, 1864: Chloroform.
removal of the apex of the cone: closure of the wound by a silver-
e. Oct. 4th: Suppuration of the cornea round the wire suture;
the suture; the corneitis soon subsided. Nov. 5th: Both eyes
d up for one week, and the operated eye for six weeks. Left eye,
al corneal opacity, of about the size of a pupil of medium dilatation;
f cone; iridectomy inwards and downwards. Feb. 9th, 1865: No

trace of cone; tells time on watch at eight inches. August, 1871: No trace of cone; the left eye, as regards vision, is now the best of the two.

Case 2.—M. M., aged thirty-four, female, thin, delicate. Conical cornea in both eyes; opacity near the apex of left cone; right near-sighted; left the same, with amblyopia; recognises No. 40 of Snellen with difficulty at one inch. A large crescentic atrophy is seen in the choroid at the outer side of the left optic disc. Feb. 6th, 1868: Chloroform. Left eye—removed the apex of the cone (cornea extremely thin). 29th: Some ciliary redness, with slight intolerance of light, and watering; central corneal opacity; no trace of cone; left the hospital. April 24th: Iridectomy inwards and downwards. Oct. 3rd, 1871: Left eye—central corneal opacity; no trace of cone; cornea very flat; time on watch at 3″; 200 at 5′.

Case 3.—G. C., aged thirty-seven, male, thin, delicate. Always slightly near-sighted; the last few years the left eye had been failing more. March 21st, 1870: Both corneæ conical and transparent, the left most; with left eye recognises time on watch at about one inch. April 25th: Von Graefe's operation performed on the left cornea; touching with caustic was discontinued, the patient being unable to attend regularly. May 9th, 1871: Removal of apex of cone of the left cornea; one suture inserted. July 18th: Left eye, iridectomy inwards. November 17th: Right eye, — 7; recognises No. 70 of Snellen at 5′. Left eye, + 10; recognises 50 of Snellen at 5′. For work and reading gave spectacles with — 7 for the right, and + 10 for the left, eye. In this case an extreme degree of myopia has been changed into one of hypermetropia of $\frac{1}{16}$.

Case 4.—Y. D., aged thirty-five, male, delicate, thin; sixteen years ago had some form of inflammation in left eye; sight of this eye failing since then. Right eye more near-sighted the last twelve months. May 18th, 1870: Left eye—cornea transparent, conical; a slight opacity is seen at the apex of the cone when examined with a convex lens. Recognises 200 (Snellen) at 1′. Right eye—slight conical cornea; 70 of Snellen at 20′. Left optic disc appears very distorted. Left eye not improved practically by calabar, atropia, or similar remedies. 24th: Cut off apex of cone (under chloroform) from left cornea; one suture inserted. 27th: Suture removed; cornea quite clear, but thrown into folds; immediately after removal of suture cornea assumed a smooth good curve, with an uneven loss of substance, resembling an ulcer, at the spot from which the cone had been removed. 29th: Slightly bulging uneven opacity and ulcer from where the cone had been removed; slight redness, no pain, and good aqueous chamber. Patient left London, keeping the operated eye bound up and using the fellow eye for work. Aug. 12th: Left eye—central corneal opacity of the size of a moderately dilated pupil; no trace of conical cornea; pupil dilates well under atropia; iridectomy inwards and downwards. Aug. 23rd, 1871: Left eye—no trace of conical cornea; small central corneal opacity; the optic disc of natural shape and well seen; reads 70 (Snellen) at 20′; does the same with a convex lens of 24 inches; the sight of this eye is still improving. In this case we see an extreme degree of myopia with amblyopia, owing to conical cornea, changed into a hypermetropia of one-twenty-fourth.

Case 5.—J. R., aged thirty-seven, thin, delicate, both eyes failed at the age of nineteen; does all his work with the right eye. August 4th, 1870: Right eye recognises 200 of Snellen at 20′; left eye does not recognise 200 at 1′. Both optic discs are visible, but appear very distorted; both corneæ are conical, with a small opacity at the apex of the left cornea. June 3rd, 1871: Chloroform; removal of apex of cone of left cornea; an opening of about the size of a small pupil was thus made into the cornea; it was closed by one suture; this caused the cornea to be thrown into large folds, which diverged from the suture. Very sick all day after the operation. 8th: Much pain and swelling of eyelids; cornea clear and much folded; iris in contact with cornea; chloroform; suture removed. 26th: Slight chemosis and swelling of lids; painless; cornea clear, except where suture had been; there is an opacity; the cornea is still very much thrown into folds; iris in contact with cornea. Aug. 24th: Curve of cornea good; large central opacity; good aqueous chamber; a small piece of the suture, which had been left in the cornea, hidden by pus, was removed. 27th: Left eye—iridectomy inwards; bleeding into aqueous chamber. Oct. 27th: The optic disc, well visible through the iridectomy pupil, appears much distorted when viewed through some, regular when seen through other parts of the cornea. Patient states that never before the operation has the sight for distance been as good with the operated eye as it is now. The patient is still under observation. It will be remarked that the folded state of the cornea, and the contact of the iris with the cornea, persisted for more than six weeks.

Case 6.—S. P., aged sixteen, female, delicate, thin; became near-sighted five years ago, left most, iridectomy upwards performed elsewhere without result; right eye recognises No. 50 of Snellen at 70′, left eye No. 200 at 8′. April 7th, 1871: Chloroform; apex of cone removed from left cornea. 21st: Opaque matter occupies the spot where the apex of the cone had been. 28th: Much intolerance and watering; opaque matter disappeared; slightly bulging vesicle in wound; good aqueous chamber. Aug. 12th: The bulging vesicle has become opaque; left hospital. Sept. 11th: Iridectomy inwards and downwards. Oct. 2nd: Left eye recognises 200 of Snellen at 8′; with 8 convex recognises 70 of Snellen at 12′, and letters of IV. at 1′. The cornea is very flat, with a central opacity. Thus an extreme myopia has been changed into a hypermetropia of one-third. One is inclined from the above to infer a peculiar shape of the crystalline lens.

Case 7.—G. M., aged twenty-two, female, fair, stout; three years ago some kind of general illness (fever), with ophthalmia in both eyes; this was followed by great pain in the head and by impaired sight, especially of the left eye. Feb. 6th, 1871: Both corneæ transparent, conical, left most, with a large shadow thrown by the cone; both optic discs as to colour appear healthy; right eye with — 20 recognises 70 of Snellen at 20′; left eye does not even recognise 200 of Snellen close to the eye. June 8th: Left eye—chloroform; removal of the apex of the cone. Oct. 8th: Iridectomy inwards and downwards. Nov. 9th: Left eye—no trace of cone; cornea very flat; central opacity; vision improving; 200 at 20′ with + 10, time on watch at 6″.

Case 8.—W. S., aged twenty-four, female, delicate, thin; near-sighted as long as she remembers; extreme conical cornea in both eyes; the cones transparent; tells time on watch with difficulty at 2″; is not able to work; no concave lenses are of use. Aug. 4th, 1871: Chloroform; removal of apex of right cornea; an opening the size of a pin's head cut into the cornea; *no suture;* bandage over the right eye. 28th: Left cornea operated on in the same manner; *no suture;* bandage for five weeks, then shade. Oct. 16th; Both eyes no trace of cone; small faint central corneal opacity; central round clear pupil; tells time on watch at sixteen inches.—*Lancet*, *Jan.* 20, 1872, *p.* 78.

73.—THE TREATMENT OF CHRONIC CONJUNCTIVITIS.

Mr. Liebreich relies almost exclusively on nitrate of silver as a stimulant application in the treatment of chronic inflammatory affections of the lids and the superficial structures of the globe. The patients mostly attend daily and receive the application in the form of a solution, of the strength of ten grains to the ounce, from his own hand. All the cases of corneitis were treated either with this solution or with atropine.—*Lancet*, *Dec.* 2, 1871, *p.* 778.

74.—ON A NEW METHOD FOR THE EXTRACTION OF CATARACT.

By R. Liebreich, Esq., Ophthalmic Surgeon to St. Thomas's Hospital.

[The original method of flap extraction of cataract was abandoned by Von Graefe on account of the large per centage of suppurations which occurred. He came to London in 1862, and was surprised at the great success which Bowman and *Critchett* had achieved in the operation by the invention of better instruments (broad lancet-shaped knife, Bowman and Critchett's spoon). Thi caused Graefe to reconsider the subject, and he soon afterwards brought ou his operation by linear extraction. This operation, although adopted by mo ophthalmic surgeons, did not satisfy Mr. Liebreich, for reasons stated in th paper, and he now brings before the profession an easier and more certa method of procedure.]

For the better understanding of what follows, I deem it essential to mention here the principal points of the comparative considerations.

Let us first compare the incisions in regard to their position, form, and execution. Originally Graefe's incision was made as peripherical as possible in order to avoid the tissue of the cornea supposed to be unfavourable for union. Later he approached more to the margin of the cornea, the external surface of the wound, however, remaining always above and beyond it. By its linear form, as nearly straight as it can be on the surface of a globe, it contrasts most markedly with the flap incision. The latter almost semicircular, situated everywhere within the cornea itself (not to speak of a modern modification), does, indeed, offer quite different conditions for the succeeding steps of the operation. Whilst the linear wound remains closed even in eccentric movements of the eye, and can only be made to gape by pressure, the flap wound has so great a tendency to open, that a brisk movement of the eye, or a slight pressure with the finger on the edges of the wound causes them to be widely separated. On this point chiefly rests the difference in the mechanism of the removal of the lens. In the flap extraction, the capsule being opened and the wound made to gape, the lens rotates so that the point of its equator most distant from the centre of the wound serves as centre of rotation, whilst the margin of the lens towards the wound moves forward. In this movement the lens passes over the posterior surface of the iris, like the fœtal head over the perinæum, and protrudes the iris, in order to place itself in the wound, having first overcome the resistance of the sphincter pupillæ. This mechanism cannot take place in Graefe's incision. If after his incision the capsule be opened and the wound made to gape, and the lens to advance further forwards, it would press against the whole posterior surface of the iris, and could never pass the pupil; it is, therefore, in this method, a mechanical necessity to cut out that part of the iris directed towards the wound up to the sphincter, or, as Taylor proposed, to incise it at the periphery. But even after excision of the iris, after the opening of the capsule, the making of the wound gape cannot force out the lens; consequently, traction instruments (such as Bowman and Critchett's spoon, Graefe's crotchet), were originally used. Later, the introduction of these instruments was replaced by external manipulations. With a spoon of caoutchouc (Graefe) or tortoiseshell (Soelberg Wells), the wound was made to gape and the lens thrust upwards. The latter is done by pressing from below upwards upon the cornea.

In the greater tendency of the flap of the cornea to gape rests also (and as it appears to me exclusively) the difference in the healing, in the necessary precautions and in the results which are to be obtained by each of these two methods.

That the more favourable results of Graefe's operation are not due to a greater tendency of the tissue of the sclerotic to heal than that of the cornea (as has been supposed for a long time), there can now be no doubt, since it has been shown that even in these incisions, made according to the original plan, the superficial part only belongs to the sclerotic, the deeper must always be in the cornea. In regard to the peripheral position of Graefe's incision I am fully persuaded that it not only does not impart to his operation any specially favourable influence, but that, on the contrary, it is the principal objection to it. I shall have occasion to recur to this point.

Thus the linear form of the wound and its lesser tendency to gape alone remain the cardinal points, to which we owe, in Graefe's method, the lesser percentage of total suppuration than in flap extraction, the diminished necessity of precautions and restrictions for the patients; also that, even in cases of very bad general constitution, weak and decrepid individuals with thin and flabby cornea, the prognosis is not so unfavourable as in flap extraction.

Even the latter fact does not appear to me to be due to the unfavourable nutrition of the flap, but rather to the more mechanical condition that the linear wound of the cornea, even where from want of elasticity it folds itself up and sinks in, immediately closes, so soon as the aqueous humour begins to reaccumulate, which is not always the case in flap wounds.

No wonder that in consequence of these advantages of Graefe's method the flap extraction became soon more and more abandoned. To me, however, it appeared that the mechanism of Graefe's method was still too complicated, and too violent, that prolapse of the vitreous body, and hemorrhage into the anterior chamber were too frequent during the operation, iritis, and strangulation of the iris in the corners of the wound too frequent after it, and that the most favourable results, compared with the most favourable results in flap extraction, were not perfect enough. If these inconveniences are carefully inquired into, it is found that they can all be brought back to one, and the same principal cause; namely, the peripheric position of the incision. This peripheric position explains why:

1. It is impossible to remove the lens without iridectomy.
2. The excision of the iris must be very extensive, else the iris tends to prolapse.
3. It is necessary to perform the operation above in order that the enlarged part of the pupil may be covered by the upper eyelid; the removal of the lens upwards is far more difficult on account of the tendency of the eye to roll upwards; and consequently,
4. During the whole operation the eye has to be kept open by the speculum, and to be drawn downwards by the forceps. This is not only painful and injurious to the eye itself, but causes
5. Not unfrequently prolapse of the vitreous body to which a peripheral incision itself already tends. Prolapse of the vitreous body, and hemorrhage into the anterior chamber, are the chief impediments to a careful removal of all the *debris* of the cortex and cause
6. Those grave forms of iritis which are kept up by the permanent irritation caused by the tumefied remains of the lens behind the iris.

Of these disadvantages I was perfectly aware, after I had followed for a short time Graefe's original plan, and I proposed some modifications, therefore, in 1867, in an article on cataract which I wrote for the 'Nouveau Dictionnaire de Médecine et de Chirurgie,' Paris, Baillière. They, however, were but a first step, and during the last four years I have by a large series of systematic experiments arrived at a method, which now, after more than 300 operations performed by it, I consider definitely settled.

Gradually I removed my incision more and more from beyond the margin towards the centre of the cornea, so that it is now entirely situated within the cornea, with the exception of the points of puncture and contra-puncture, which are placed about one millimetre beyond it in the sclerotic[*]—the whole remaining incision passing with a very slight curve through the cornea, so that the centre of it is about 1½—2 millimetres within the margin of the cornea, and consequently nearly in that place towards which the equator of the lens is turned, when by a slight pressure on the margin of the wound the lens is brought into that rotation of which I have spoken above in flap extraction. In this form and position of the wound a broad excision of the iris could be avoided and consequently I cut off smaller and smaller pieces of the iris, until at last I entirely gave up iridectomy. I now saw that without actual formation of a flap that mechanism can be brought about, by means of which the advancing equator of the lens overcomes the obstacles of the iris and of the sphincter pupillæ in order to enter the wound. Avoiding iridectomy, I could do without elevators and forceps, and thus change the whole operation into a less violent and almost painless one. Made with Graefe's knife, the most perfect regularity can be very easily obtained without fixation. My operation is now made in the following way:

The patient lies on his back; chloroform is administered only on his express desire; the pupil having been dilated with atropine the day previously, if possible. For the right eye the operator stands behind the head of the patient, for the left at his left side. An assistant is not necessary. All the instruments required are two, namely, the smallest Graefe's knife possible, and a cystotome which has a common Daviel's spoon at the other end. The whole may even be united in one instrument, which has at one end Graefe's knife and at the other end Daviel's spoon, and within the handle a cystotome which admits of being pushed in and out. Supposing the right eye is to be operated upon. In that case the operator takes hold of the upper eyelid with the index finger of his left hand, whilst he slightly presses the middle finger against the inner canthus of the eye. The knife held in the right hand with its back horizontal and backwards, the plane of the blade making with the horizontal meridian of the eye an angle of about 45°, enters the sclerotic with its point about one millimetre externally to the margin of the cornea. *Without altering the direction*, the knife passes through the anterior chamber in order to make the contra-puncture on the opposite side, so that the point of the knife becomes visible in the sclerotic about one millimetre (or less) distant from the cornea. The knife is now pushed forward, so that its retraction finishes the incision. As soon as the incision is made, the eyelid is to be dropped.

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[*] Of course only as regards the outside of the wound. As regards the inside, all the wound, even the puncture, is situated in the cornea, the peripheral part of which cannot be reached by a knife introduced in the indicated position without previously passing through a small portion *of the sclerotic.*

The second part of the operation consists in the careful opening of the capsule.

In the third part Daviel's spoon is slightly pressed against the inferior margin of the cornea, and the index finger of the left hand, which holds the upper eyelid, through it exerts a very slight pressure on the highest point of the cornea. Thus the lens is made to rotate a little, its lower margin presses in the manner already described against the posterior surface of the iris, pushes the iris forward, passes along it to the margin of the pupil, overcomes the obstacle and places itself freely in the wound, which is made to gape by Daviel's spoon pressing against it. A slight pressing movement of the index finger of the left hand, by means of which the upper eyelid is shifted from above downwards over the cornea, serves to expel the lens. Similar movements of the lids are employed for the purpose of forcing out any remaining *débris* of the cortical substance, after pushing them from behind the iris towards the pupil, by gently rubbing the shut eyelids. Should the pupil then not appear round, but its margin drawn towards the wound, it regains its normal position by an outward shifting of the lower lid; or, if that be not sufficient by the introduction of Daviel's spoon. Immediately afterwards I put some atropine into the eye, and close it by my compressive bandage.

What are the advantages of my method of operating?

1. It is undoubtedly of all methods the simplest and the least painful.

2. It is unconditionally the easiest to perform, and requires the least practice; it may, therefore, be performed by those who have only occasional opportunities of operating; and those patients will profit by it who are unable to reach a central point in order to place themselves in more practised hands. On account of the greater facility of operating, the last pretext for reclination of cataract is removed, which, though almost universally and justly condemned, is still here and there performed.

3. It is preferable to the flap extraction, on account of the safer and constantly regular incision. The flap incision scarcely ever acquires the regularity which may theoretically be demanded, even if made by the most practised operator with the best assistance, the most enduring patient, or under chloroform by the use of the elevator and fixation instruments. At one time its height or breadth is not what it is intended to be; at another time its position is incorrect or the wound irregular. Indeed, part of the irregularity is due to the difficult form of the incision, but by far the greater part, according to my conviction, is due to the mechanism by which the cuneiform cataract knife makes the incision. A small Graefe's knife would make a flap safer and more regularly than any wedge-shaped cataract knife. The incision I designed can easily be made, in every case, exactly in the desired form and position, even if the patient is very restless, without assistance, without elevator or fixation. This mainly depends on the facility with which the place of the contrapuncture *can be* chosen, the knife drawn back and made to pierce at another point, if a *mistake* is made in the selection of the place for contrapuncture, and in the

eedom with which in terminating the incision the inclination of the knife can changed, if necessary.

A little practice will enable every operator to avoid those corrections, and make the contrapuncture, as well as the whole incision, correctly, according his original plan.

4. Compared with Graefe's method, it has the advantage of a more favoura-e position of the field for the operation, and avoids through it all the incon-niences to which I have referred as arising out of the peripheral position of e wound.

5. In regard to the mode of healing, it favourably contrasts, like Graefe's ethod, with the flap extraction, on account of the diminished influences which e, constitution, general state of health, season, and other causes exert; also account of the less demand made upon the patient to remain quiet after the eration, and, above all, on account of the lesser tendency to suppuration of e cornea.

6. The advantages of my method over that of Graefe are shown by the timate results obtained—by not furnishing a greater percentage of total sup-uration than Graefe's method my best results are in regard to optical and f I may use the term) anatomical perfection, identical with the best results tained in flap extraction.

The method is well adapted for the different cataracts, with the exception f—

1. Those laminellar cataracts, which need only be treated by iridectomy.
2. Cataracts which in earliest childhood have to be operated upon by re-eated division.
3. Perfectly liquid cataracts, (division with a broad needle).
4. Partial cataracts, without a nucleus, already absorbed to a great extent, nd therefore chiefly traumatic cataracts, for which also division suffices.

In all other cases of cataract my proceeding can be adopted, only the breadth f the incision and its obliquity may vary very slightly, according to the size f the nucleus, the consistency of the cortex, and especially to the proportion hich the size of the cataract bears to that of the cornea. In cases of equally oftened cortex, with a smaller nucleus and sufficiently large cornea, puncture nd contra-puncture may be made low down and closer to the margin of the ornea, and thus by slighter inclination of the blade, the incision obtains a trictly linear form. With a large and hard nucleus and tough immature ortex it will be necessary, especially if the cornea is exceptionally small, to ake the wound somewhat larger and a little more curved. For that purpose uncture and contra-puncture must be made a little closer to the horizontal eridian of the eye, and the blade somewhat more inclined (a little more than 5° from the horizontal meridian).

In this way it is in every case possible to make the wound sufficiently large or the easy passage of the most voluminous cataract, without incurring the endency to gape. Even for the removal of the lens with the capsule (without pening the latter), the wound is sufficient.

If Pagenstecher's procedure is followed, my incision must then be combined ith iridectomy. Delgado's procedure can be followed out, even without idectomy. Delgado introduces a small instrument into the anterior chamber, efore he makes his incision, and presses with it on the lens, in order to free from its natural connections and to remove it, together with the capsule, fter the incision has been made.

I intend to propose another method of extracting the lens together with its apsule; but a more extended series of experiments is required before I can efinitely decide the question, whether and in what cases this procedure can e usefully combined with my incision.

The question, if and in what cases my procedure is to be combined with ridectomy of more or less extent, can be solved only after a larger experience f my operation without iridectomy than I yet possess. I have performed everal hundred operations with iridectomy, but as yet scarcely 100 without.

I hope that I shall not be left alone to work out this question, but that other urgeons will accept my proposal, which, to recapitulate shortly, is as ollows:—

To avoid the disadvantages in Graefe's operation arising out of the peripher position of the wound, and the disadvantages in flap extraction arising out the height of the flap, I propose a new method of extraction, which is to made in the following manner:

Puncture and contra-puncture are to be made in the sclerotic about 1 mm. beyond the cornea; the whole remaining incision is to pass, with a very slight curve, through the cornea, so that the centre of it is about 2 mm. distant from its margin. This incision may be made upwards or downwards, with or without iridectomy, and the lens may be removed through it, with or without its capsule.

If, as I now operate, the extraction is made downwards without iridectomy, the whole proceeding is reduced to the greatest simplicity, and does not require assistance, elevator, fixation, or narcotism, and only two instruments.

In this latter form my procedure has the advantage of being on the one hand, the least dangerous mode of extraction; and on the other, of being able to obtain the most perfect results, viz. that condition in which the eye operated upon differs from the normal eye only by the absence of the lens, and a cicatrix of the cornea difficult to be seen.—*St. Thomas' Hospital Reports*, 1871, *p.* 259.

75.—ON EXTRACTION OF CATARACT BY A PERIPHERAL SECTION OF THE IRIS WITHOUT INJURING THE PUPIL.

By DR. CHARLES BELL TAYLOR, Surgeon to the Nottingham and Midland Eye Infirmary.

[Dr. Taylor has found the operation here described very successful, not more than four or five unfavourable cases having occurred to the hundred. The advantage claimed for it is a central and moveable pupil.]

Prolapse of the iris has always been the bugbear of extraction wherever iridectomy has not formed a part of that operation; and yet the extreme beauty and superexcellence of the results, when no accident has occurred and when all has gone well, after one of the old flap operations, is such that we constantly find ophthalmic surgeons abandoning the modern methods to revert to the old and necessarily much more dangerous flap extraction. When I was last in Utrecht I found that Professor Snellen had, in spite of the good results to be obtained by Von Graefe's and other modern methods, returned to the old flap operation; and the same is true of many other continental surgeons as well as of some of the most eminent in this country. Those, for instance, who have had the good fortune to study Mr. Bader's practice at Guy's Hospital must have noticed how frequently and with what excellent results he resorts to the old flap operation. I have myself on several occasions, actuated by a similar desire to save the iris and attain the maximum of success, extracted by my own method without excising any portion of the iris, merely enlarging the wound in a lateral direction sufficiently to permit of the exit of the lens. Dr. Macnamara, of the Calcutta Hospital, has also endeavoured to effect the same object by the use of the spoon. In my own experience, however, prolapse has occurred from time to time, and whenever I have left the iris untouched I could by no means say, as I was in the constant habit of doing when every step of my process had been fully carried out, that my anxiety had ceased with the operation, and that scarcely any subsequent treatment was necessary.

Latterly I have been endeavouring to ascertain whether it were not possible to combine the prevention of a prolapse and the other advantages attendant upon an associated iridectomy with the central and movable pupil so much to be desired in all operations of extraction for cataract, and having attained that desirable consummation, I am anxious to lay before the profession the steps of the process, which are briefly as follows.

As a rule I administer a mild aperient the day before the operation, and extract the following morning before breakfast. Chloroform is much more pleasantly given under these circumstances. The risk of vomiting is to a great extent obviated, and that of all other accidents dependent upon anæsthesia diminished.

The instruments I employ are a pair of sharp forceps that pierce the sclerotic; a very light speculum (a modification of Von Graefe's); and two knives, a line in width, and bent at an angle similar to the ordinary iridectomy knife—one with a sharp point, the other with a blunt or bulbous extremity.

Having separated the lids with the speculum, the eye should be gently turned downwards with a pair of ordinary forceps in the operator's right hand. Having got the globe into a favourable position, it should be fixed by the sharp forceps at about the junction of the upper with the middle third of the cornea; the pointed knife is then entered in the cornea-sclerotic junction one or two lines from the forceps at the summit of the cornea, pushed well into the anterior chamber, and then, with a gentle sawing motion, carried along the summit until about one-third of the cornea has been incised. The capsule is then carefully divided with Von Graefe's cystotome, having been previously rendered tense, and the eyeball fixed with a pair of ordinary forceps. (It is better to open the capsule at this stage, because bleeding from the wounded iris—and conjunctiva also—at a later period is apt to fill the chamber and render this part of the operation obscure and difficult.) The upper segment of the iris is then seized, and a small piece of the periphery only excised, the pupillary margin and portion of iris attached to it being left untouched and free in the anterior chamber; the lens is then extruded through the gap in the ordinary way, gliding behind the pupil, so that there is no stretching of the sphincter.

In this way I believe that I have secured all the advantages, in the way of safety and certainty, of an associated iridectomy (which I have already detailed), and at the same time attained that great desideratum—a central and movable pupil.—*Lancet, Nov.* 4, 1871, *p.* 634.

76.—ON CATARACT.

By Dr. H. R. Swanzey, Ophthalmic Surgeon to the Adelaide Hospital, Dublin.

The cataract of old age or senile cataract, is the most common form of this disease. It usually attacks persons over fifty years of age. In it the central portion or nucleus of the crystalline lens is the most opaque and hardest part, while the cortical substance is much more translucent, even when the cataract has become quite ripe. This depends upon the physiological changes which take place in every crystalline lens with advancing years. In the youthful crystalline the central part is not more consistent than the cortical layers; but, before middle life is attained, a distinct nucleus of a firmer texture is found, which becomes more marked at later periods. In old age, too, the lens, and especially its nucleus, obtains an amber tint, which is sometimes of so deep a shade that you must be careful not to mistake it for a cataract. Senile cataract, indeed, appears to be one of those conditions which stand on the boundary of what is pathological and what is physiological. We may yet, no doubt, have much to learn as to the cause of the affection, but, so far, we know that cataract frequently makes its appearance in persons otherwise perfectly hale, and, therefore, when you find it in some who have suffered much from gout or rheumatism, it would be irrational to seek its cause in the latter or any other concomitant constitutional disease. Nor has the microscope been as yet successful in demonstrating changes in the choroid or vitreous humour, which would account for the alterations in the lens on the supposition of a locally impaired nutrition. We may therefore be not far astray in regarding senile cataract as an exaggerated physiological condition, a consideration which does not take it out of the category of diseases, interfering as it does with the functions of the organ. Similarly, a certain amount of chronic enlargement of the prostate gland is extremely common in old age, but is not regarded by surgeons as a disease until it attains such a degree as to impede the functions of neighbouring parts.

Senile cataract may commence in the nucleus, or in the cortical portion of the lens, or simultaneously in both. These forms, however, which commence with fine stripes in the edge of the lens, while the nucleus remains clear, ar usually very slow in their progress, so much so, indeed, that if your patien be of an advanced age, it may be more judicious to refrain from mention of th condition of the eye, for he will be very apt to outlive this cataract. Bro stripes in the corticalis with accompanying opacities of the nucleus indicate rapidly advancing cataract. In every elderly person who comes to you complaining of his vision, you must carefully examine the crystalline lens. You must do so by daylight, by the reflected light of a lamp with the mirror of your ophthalmoscope, and by means of oblique illumination. According as the opacities in question, if they be present, are more or less intense, you will discover them best by one or other method. In using the reflected light to discover incipient cataract, it is a mistake to have it too brilliant, for then the more translucent opacities may escape detection. To examine the periphery of the lens which is concealed by the iris, you must direct the patient to look strongly to one side, or upwards or downwards, while you look into the eye from the opposite direction. When a senile cataract is quite ripe, the entire of the cortical portion has become opaque, and with the oblique light a yellow reflector is obtained out of the centre of the lens from the hard nucleus.

This is the most favourable form of cataract for operation, fully ninety-five per cent. of such eyes being restored to vision by means of Graefe's peripheral linear extraction. By means of this method, which is you see practised here, the most infirm person may be operated on. In the days of the flap extraction many bodily infirmities afforded contra-indications to its performance, as it entailed confinement to bed in a horizontal position for a lengthened period. If the condition of the patient renders it impossible or undesirable that he should be confined to bed, he need barely lie down for more than a few hours after the extraction by Graefe's method; and in no case which runs a favourable course, need the patient remain in bed for more than three or four days. I do not now intend to take up time by a description of the various steps of the operation which you have seen performed, and which you will have another opportunity of witnessing in this case. To have a successful result, it is quite as necessary, in my mind, that the after treatment of the case be properly conducted, as that the operation itself be correctly performed. Graefe not only developed this method of extraction, but also the treatment best adapted to it; a circumstance which is very generally ignored.

Immediately after the operation the eye is carefully padded with fine charpie or French wadding, in such a way that no part of the globe is pressed on more than another; then, three turns of a narrow flannel roller are made with a gentle, but firm and equal pressure over the eye and round the head, in the manner with which you are acquainted. The patient is then comfortably settled in bed, and the room darkened. The pain of the wound should not be very severe, and should not continue longer than four or five hours; in either event you should administer about the eighth of a grain of morphine hypodermically in the corresponding temple. Pain, in all affections of the eye, is a symptom which, as a general rule, you must combat, for in itself it gives rise to irritation. In eight hours, or thereabouts, after the operation the bandage should be changed with the object of removing the slight secretions, blood clots, &c., which are apt to collect between the edges of the lids, and which, if left there might, by their decomposition, afford the starting point for suppuration of the wound. It is not advisable, however, to open the eye or expose the wound at this early period. Between the twelfth and twenty-fourth hours is the period when you must be on the look out for anomalies in the process of healing. During this period when the course is normal, no pain or disagreeable sensation should be experienced by the patient, or at most, only an occasional and momentary "pricking." Any continued pain at this time, no matter how slight, must be regarded as a bad symptom, and be at once attended to. The bandage must be removed, any collection of tears under the lids giving egress to, and if there be any swelling of the lids as counter-irritation, they should be lightly touched externally with mitigated lapis, which is then immediately neutralized with a solution of salt. If the pain still continue, a hypodermic injection of morphine should be given, and if this does not attain the desired end, then in case your patient's strength admit of it you should employ a venesection from the arm to the amount of four or five ounces; this should be followed by a calomel purge, and—if the interval since the previous one is long enough—another morphine injection, a very firm and exact bandage being applied. When the patient is not healthy enough to admit of a venesection, you must confine yourselves to the other means I have mentioned. With this aid you will, in a vast majority of cases, succeed in cutting short the suppurative process, which is just about being set up in the wound. But I must guard you against the fallacy of supposing that some delay in their employment is of no consequence. If you are to effect anything you must resort to them early, before suppuration has set in, for once it has done so your skill will be of little avail. You will not find that this treatment is one much adopted by ophthalmic surgeons; but it was laid down upon the basis of a most extensive field of practice by Graefe, who was endowed with remarkable powers of clinical observation, and I can testify to its value myself. That which most surgeons would be likely to take exception at, is the changing of the bandage so soon after the operation, and yet Graefe considered this one of the most important points. After the flap operation the bandage used not to be altered for some days. The venesection, too, is likely to be regarded with disfavour by many, in consequence of its being now so out of fashion. In this particular instance, I have no doubt of its frequent efficacy; and further, I believe you will see a reaction in favour of venesection in general medicine and surgery before many years, but with more restricted and precise indications than formerly. Among the many advantages afforded by the peripheral linear extraction, is the short period during which the surgeon has to be on his guard against suppuration of the wound; from the twelfth to the twenty-fourth or thirtieth hour being the time when it occurs, and never any later. With the flap operation, the eye could not be pronounced safe from suppurative reaction for several days.

Previous to extracting a cataract, you should endeavour to ascertain whether the condition of the retina, &c., is normal; in senile cataract it will generally be found so. Complications, however, which are more common in these cases are conjunctivitis, and obstructions in the lachrymal canals and nasal ducts. These you should in every case look for, and, if present, you must endeavour to restore the normal condition, or something approaching it, before extracting the cataract; otherwise, the muco-purulent secretion collecting under the bandage is very apt to produce serious anomalies in the healing process.

I have told you that old age is the most frequent cause of cataract. The disease, however, is by no means rare at other periods of life. You will often find persons of thirty-five and forty afflicted with it, and you will not seldom observe that its occurrence at this age depends upon an hereditary tendency, some of the patient's forefathers having some fluid from the same cause, although perhaps later in life. The cataract then is generally of a softer consistence, with a small nucleus. Its removal cannot be effected with as good a prognosis as the true senile cataract.

In diabetes, as I suppose most of you know, the lens often becomes cataractous. There is nothing in the appearance of a cataract dependent upon diabetes to distinguish it from any other, and it is consequently a good general rule to examine the urine of every cataract patient for sugar. Diabetes cataract may be removed with success, and it will always be your duty to remove it unless a fatal termination to the general disease appear imminent. We know how long patients sometimes linger under this disease, and it surely would be inhuman not to afford them the boon of vision for the last few months of their existence, when it may be given them with so little pain. My own experience of extraction in diabetes cataract is not sufficient to form from it an estimate as to the percentage of successful cases. It happens that I have not yet seen an unsuccessful one. Graefe found that in twenty cases of cataracta diabetica, sixteen recovered their vision by extraction.

Cataract may be produced traumatically; the most common injury being that caused by the entry of a foreign body into the eye through the cornea, and then into the lens itself. When the capsule of the lens is opened, the action of the aqueous humour on the fibres of the lens is sufficient to make *them swell and become* opaque. The foreign body may have been a large one, which was again withdrawn from the eye, or it may have been a small atom of metal which continues in the eye. These are often most delicate points to decide, and they are of the utmost importance, as in case of a foreign body lodging in the eye, the ultimate prognosis for the organ is very serious indeed. You will arrive at a conclusion by the history of the case and by an examination of the eye itself. Besides penetrating wounds, blows on the eye are sufficient to cause cataract, whether given by the fist or by the cork of a soda water or champagne bottle, or by the end of a billiard cue, the patient standing behind the player while he is making his stroke. It is probable that in these cases the capsule of the lens is ruptured in some place by the concussion, as otherwise it would be difficult to understand how such injuries could cause opacities of the lens.

Cataract may depend upon deep-seated disease in the eye, *e.g.*, detachment of the retina, certain forms of choroiditis, retinitis, and irido-choroiditis. In most of these cases the external appearance of the eye gives no clue to the condition of things lying behind the cataract. The opthalmoscope of course is of no use, in consequence of the opacity of the lens; and it is by the state of the functions alone that we can determine whether the cataract be complicated or not. If you hold a muffed milk glass close before your eye you will be able to realise the disturbance to vision caused by a fine uncomplicated cataract, and at from one to two feet you will be able to distinguish the light of a gas flame turned down to the lowest, and to see the motion of the hand in a favourable light at a foot or so from the eye; in a dark room you will also be able to tell from what direction the light from the lamp comes; this latter we term the "power of projection." If in any of these particulars a cataractous eye be not up to the mark, you must be very suspicious of the presence of complications, especially so if the projection be incorrect or defective, for this shows that that part of the retina upon which the light falls is probably diseased. Under such circumstances you would either not extract the cataract at all, or if you thought the disease in the fundus oculi to be of limited extent, you might do it, having prepared your patient for an imperfect result.

The patient before you is aged eight-and-twenty. In the right eye there is a cataract which is shrivelled with a thickened anterior capsule. The power of perception of light in this eye is very insufficient, and the power of projection is wanting in every direction; there can be no doubt, therefore, that there are extensive complications in the fundus, of what nature I am not prepared to say. There is the history of an injury to the eye some years ago, and, very possibly, both the cataract and the deeper complications depend upon it; indeed, if it were not for the condition of the other eye, I should almost certainly be of that opinion. In the left eye she has a mature soft cataract of some months' standing, and, as well as we can make out by a functional examination, the fundus is normal. This is very remarkable in so young a subject, and is rendered still more so if the other eye be taken into consideration. The urine is normal, and there is no hereditary tendency. It is therefore indicated to operate upon the left eye, but not on the right.

Before I conclude I may mention two other forms of cataract which you will meet with—namely, lamellar or zonular, and congenital. The first affects only certain layers of the lens, the others being left quite intact. The layers affected are generally those around the nucleus, so that the latter and the cortical substance remain clear. If the opacity be not very extensive the patient may have very fair vision still, particularly with his back to the window, or when he shades his eyes with his hand, so that, the pupil dilating, he sees through the clear edge of the lens. This too suggests one mode of improving the vision of these persons, namely, by forming an artificial pupil in the nasal portion of the iris, through which the patient may now see in any light. But when the opacity is extensive an artificial pupil would do no good, and then you will generally find the best proceeding to be that called division or breaking up of the cataract. A fine needle is pressed through the cornea into the capsule, and the latter torn to a slight extent; the anterior layer of the lens is broken up a little with the needle. In this way the aqueous humour is admitted to the substance of the lens, which gradually softens and becomes absorbed. This operation requires to be repeated frequently before the entire lens disap-

, and the entire process, which occupied several months, is sometimes at- d with dangers of which I shall speak to you when next I have a case of ind under treatment. Zonular cataract most commonly appears after the dentition, and it is a curious fact that the subjects of it have usually ed from convulsions during teething. The affection may also be con- l.

ıgenital cataract is not uncommon. It is a soft cortical cataract, and it is ful to what circumstances it owes its formation. Generally speaking are favourable cases for operation by division, provided the patient be not ld, but by the sixth or seventh year the retina, from never having been becomes rather inert. It is advisable therefore to operate early, if pos- before the first dentition. Much mischief is done by postponing the tion until the tenth or even fifth year. It is a remarkable fact, showing necessary practice is for the development of the functions of a part, that en years of age a cataract is produced by trauma, you may obtain very rision for the patient if you operate even forty years afterwards, but in nital cataract the result will be but very wretched in the fifth year after

s is a short sketch of some of the principal points in connection with the t of cataract.—*Medical Press and Circular, Jan.* 17, 1872, *p.* 49.

77.—ON GLAUCOMA, PRIMARY AND SECONDARY.

By T. Shadford Walker, Esq., Liverpool.

riously to the invention of the opthalmascope by Helmholtz, in 1851, the e now known as glaucoma was only recognised as such when it had ar- at its last, and most complete stage, that namely, in which vision was so ected that objects could only be seen dimly in one position of the eye— neral field of vision having become greatly contracted. Nothing beyond rception of light still remained, the pupil was seen dilated and fixed, the luced to a mere ring, its colour altered, and its fibres muddy and indis- the lens semi-opaque, and the area of the pupil showing a greenish re- a (from which the complaint obtained its name), and the eyeball itself, touched, being of a stony hardness. Several observers, especially , Lawrence and MacKenzie, had noted and pointed out the importance increased tension of the globe; but the early stages of glaucoma were nded with the symptoms of amaurosis, and passed under that name. however, after the opthalmascope allowed the interior of the eye to be ned, Prof. Jaeger, of Vienna, observed, and carefully described, the ap- ce of the optic disc in glaucoma, drawing special attention to the typical tion, which, from the peculiar shading of the cup, was supposed to be an on instead of a depression. Descriptions of the opthalmascopic appear- presented by isolated cases, were also published by other observers, and creased tension of the eyeball in glaucoma was remarked upon; but it ot until the late Prof. Von Graefe, of Berlin, combining the observations ers with his own, demonstrated the connection between increased globe n and the excavation of the optic disc (the true nature of which he was st to discover), and the consequent pulsation of the central artery, that al pathology of glaucoma was made clear. Von Graefe, moreover, ing from the success he had obtained by performing iridectomy in ulcera- nd progressive staphyloma of the cornea (which he explained by the of the tension undoubtedly produced in these cases) to the great proba- of a similar result in glaucoma, at length in 1856, had the satisfaction of ncing the complete success following iridectomy in all those cases of ma where delay in resorting to it had not occasioned such alterations in ucture and nutrition of the retina and optic nerve as to render operative rence of any kind of no avail. He had thus the double glory of being st *adequately to recognise* the essential character of a very serious disease, and to point out a most valuable remedy, the simplicity and good effect which have been acknowledged by the foremost oculists of all nations.

Mr. Walker having remarked that glaucoma was chiefly a disease of advanced life, and that it resembled gout, phthisis, and other constitutional disorders, in the tendencies common to them of transmission from parent to offspring, gave a careful description of acute inflammatory glaucoma. A premonitory stage was recognised, in which very mild and transitory attacks passed off without apparently doing the eye much damage, but eventually, these became very frequent, leaving intervals of only a few days, the sufferer began to pass restless nights, and found no relief in his symptoms on awakening in the morning; his sight became affected, and contraction of the field of vision was observed. Then, usually at night and accompanied by violent neuralgic pain in the forehead, the attack suddenly came on which led to the title given to this form of glaucoma of acute inflammatory. Rapidly increasing dimness of vision, marked dilatation of the pupil, a narrowed muddy condition of the iris, more or less turbidity of the humours, and great increase in the tension of the eyeball soon follow, and are accompanied by great constitutional disturbance. One form, coming on suddenly, and actually destroying sight within a few hours, was noticed by Von Graefe, and termed by him "Glaucoma fulminaris." Fortunately, it is of extremely rare occurrence. A case of this kind occurring in a working man who came to the Eye and Ear Infirmary, was described by Mr. Walker. This patient stated that two years previously, at bedtime, without any warning, the left eye was attacked; this was ushered in by excruciating pain, and in the morning vision was gone, not even a bright light held close to the eye being seen. In a similar manner, six weeks ago, the sight of the right eye had entirely disappeared within two days. No operation had been performed, and although he still suffered from attacks of dreadful pain in the eyes, he would not allow any operation to be performed.

The second variety of primary glaucoma, known as chronic inflammatory glaucoma, was next described. It is distinguished from its predecessor mainly by having, instead of intermissions, remissions and exacerbations; rather than by the occurrence of fresh symptoms on a previously quiescent condition. Again, the cornea is much more affected in this variety; after a time it becomes flattened, and loses its sensibility to a remarkable extent, so that in many instances it may be touched or rubbed without producing any signs of distress.

A third variety exists under the name of glaucoma simplex, and is really only a very insidious form of the last; in which the inflammatory symptoms are masked, or are so slight and transitory, that the patient does not notice them. So quietly does the disease advance, that the sight of the one eye may be completely lost, while that of the other eye begins to fail in a similar manner, and, what was ascribed to a natural failure of sight, due to increasing years, is discovered to be disease requiring prompt measures to arrest its progress.

Having completed his sketch of the three varieties of primary glaucoma, the author made a few remarks on secondary glaucoma, which, as its name implies, consists of the grafting on a previous disease of the glaucomatous condition. Speaking generally, it may be said that those inflammatory diseases of the eye which, in their course, occasion an increase in the tension of the globe, and which, on subsiding, leave behind deposits of organised lymph, binding down and agglutinating the tissues, are very apt to occasion glaucoma.

In examining these classes of cases three modes of investigation must be pursued:—(1.) A careful enquiry is made into the history of the disease, and into the patient's symptoms. (2.) An ophthalmoscopic examination is made, and this, in a typical case, reveals the optic disc of a muddy, reddened, or yellowish tint, instead of showing a flat surface, being deeply excavated or cupped, the arteries smaller than natural, and exhibiting, either spontaneously or on very slight pressure, a distinctly visible pulsation, the retinal veins swollen or knotted, and very tortuous, the retina itself deprived of its transparency, becoming blurred, indistinct, and muddy, or being atrophied, and showing the choroidal pigment through its coats. (3.) Examination by the finger to determine the tension of the globe. In connection with this subject, the new ophthalmotonometer, invented by Professor Doe, of Berne, was described. It consists of a hollow ivory cylinder, containing a smaller solid ivory cylinder, the latter movable, and connected at its upper end with two upright needles, which, when pressed upon by the solid cylinder, move like the hands of a barometer along a flat metallic indicator divided into equal parts, so that when touched or pressed upon, they move until the pressure ceases, when they remain stationary, registering the pressure, which is reckoned in grammes and millimeters; the patient being laid down and the eyelids separated, the operator, by means of a silk thread, suspends the instrument by his teeth, and allows it gently to stand with its own weight on the outer side of the anterior surface of the eyeball, steadying it, but not holding it, by placing a finger against the side. The solid cylinder is allowed to project two millimeters below the level of the hollow one before being placed on the eyeball. So soon as the end of the projecting cylinder touches the globe, pressure is exerted, and the needles begin to register.

Proceeding to consider the cause of glaucoma, Mr. Walker, after mentioning the different views that had been held at various times, remarked that most probably the correct one was that held by Von Graefe, viz., that in all persons predisposed to glaucoma a rigidity and non-distensibility of the sclerotic exists, that this natural condition is increased and confirmed at the approach of old age so that the ciliary nerves become pressed upon, their functions are interfered with, and the nutritive absorbent action of the parts they supply becomes affected, so that when slight causes of irritation arise the fluid contents of the eyeball no longer can be changed. The result is, that on the occurrence of inflammatory action no yielding of the sclerotic can take place, the products of inflammation are not absorbed, and increased tension follows. The most promising of the plans for relief at first employed, viz., paracentesis corneæ and intra-ocular myotomy, have been gradually abandoned after repeated trials, and, in spite of strenuous opposition, the practice of iridectomy, first proposed by Von Graefe, has received the sanction of the foremost oculists of all countries.

The remainder of the paper was occupied with a review of the theories as to the rationale of the relief afforded by iridectomy. These all, however, tend to support the view that this operation acts by diminishing intra-ocular pressure, thus affording the restitution of conditions favourable to nutrition and circulation. Finally, Mr. Walker urged the early performance of the operation, as soon as a glaucomatous condition is fairly recognised.—*Liverpool Medical and Surgical Reports, Oct.* 1871, *p.* 141.

78.—UNRECOGNISED ACUTE GLAUCOMA—A FERTILE SOURCE OF BLINDNESS.

By Ernest Hart, Esq., late Ophthalmic Surgeon to St. Mary's Hospital.

Perhaps the most serious class of cases in which I find on my note books indications of serious mischief having arisen for want of definite practical hints as to diagnosis, are the cases of acute glaucoma. Early diagnosis of this rapidly destructive disease appears, indeed, except in the hands of practised ophthalmic surgeons or of those who have attained special acquaintance with modern ophthalmic practice, to be still the exception rather than the rule. Yet there is no disease of the eye in which prompt action is so rapidly called for—none in which delay leads to more irretrievable injury. Of sixty-seven cases of acute glaucoma of which, during the last decade, I have noted details, which had been under treatment before I saw them, fifty-two did not reach my hands until they had suffered serious injury from delay; and in thirty-nine the true nature of the disease had not been suspected, and the treatment had been either useless or injurious in its character. This had not been due to any want of general capacity and skill, but to the fact that the attention of the professional attendants had not been directed to this subject, and that they were not in possession of the few practical points which in this disease, as in others, suffice to establish *prima facie* evidence of the nature of the case, such as would at once have suggested the necessity of a more careful investigation of the facts. One or two instances will illustrate the subject, and I will choose some of the most striking. Five years since, I was asked by my friend Mr. Barnett to see a lady who had been suffering from an affection supposed to be connected with constitutional weakness and debility, for which she had been under treatment for some time. She had "inflammation" of the left eye, with loss of vision. When I saw her the conjunctiva was injected with blood; the cornea had lost its polish; the iris was ovally dilated; and, on closing the sound eye, it was found that in the inflamed eye vision was abolished. The ophthalmoscopic examination showed marked excavation of the disc, but great clearness of the media. The eyeball was hard and unyielding as marble. It was a typical case of acute glaucoma, with total loss of vision from extreme intraocular pressure. There had been abundant premonitory symptoms—considerable increase of "far-sightedness" (*hypermetropia*), haloes seen around the lights in the evening, and pains in the eyeball. It was a great shock to those around when it was found that the operation of iridectomy afforded the only chance of recovery of sight, and that this must be performed without any delay, if it were to do any considerable amount of good. That opinion was, however, confirmed by Mr. Bowman; and on the same day I practised a full iridectomy. The sight was only partially restored, the other eye remaining good. The patient was well satisfied with the result, but did not cultivate the vision of the operated eye. Last January I was called again to see the same patient. She had been selecting silks in a draper's shop three weeks before, when she felt a severe pain in the eye, and the sight failed her suddenly. She returned home and was placed under treatment. Once more she had calomel and opium, warm fomentations, and local antiphlogistic treatment, with general support by port wine, for she was very weak. Meantime, she was kept in darkness and in hope. When I saw her in consultation, I found sight totally abolished, as it had been during the whole three weeks; the conjunctiva red; the eyeball of stony hardness; the pupil ovally dilated; the cornea roughened; the media so extremely clouded that ophthalmoscopic inspection of the fundus was entirely obstructed. The neuralgic pains of the face and side of the head had been throughout and were still intense. Here was once more an attack of severe acute glaucoma; and this time it had been allowed to run on, unchecked by iridectomy, so long that the case was practically hopeless. Iridectomy afforded the only chance. I informed her medical attendant of the true state of the case, and we once more had to make the best of a very awkward position. He assured her of recovery; and attributed the affection to ordinary inflammation with effusion, which would clear away. At this stage of the disease, even iridectomy held out but little chance of restoration of vision, and it could only be suggested as offering the one chance which remained. The result in this eye was not so good as in the other, where so much time had not been lost, and where the nervous structures still retained a power of recovery. I doubt whether this patient will ever have any useful vision with it; but the vision of the left eye, aided by stenopæic glasses, suffices for most of the ordinary purposes of life. I have seen the patient this week; she still has useful vision in the eye first operated on.

This case is perhaps calculated to dwell in the memory, and to fix there the lessons which it teaches, from the remarkable fact that in both eyes the attack was alike; that its true nature was similarly misinterpreted in both; and that the patient escaped the one error with partial vision in the eye affected, and the other with barely a remnant.

Not to be tedious, I will mention only one other case, which is less painful, but not less instructive. I was asked lately to see an aged gentleman, whom his attendant described to me as having been suffering for a little time from what seemed to be a troublesome conjunctivitis. There was in this case little or no pain—only some uneasiness of a very slight character. The patient insisted, and continued to insist, that he had a habit of sleeping with his face resting on his hand, and that he had unduly pressed upon the eye. His eye "felt a little bruised," but not enough to induce him to complain of it voluntarily. It was sensitive to light, but not enough to prevent him from writing comfortably when it was partially shaded, or even from pushing the shade off ye when writing; and he was but little disturbed about it. He was a hale entleman, but considerably more than eighty years old, and therefore dy suffering from some infirmities of the prostate. This insidious and ess conjunctivitis was then an almost painless but marked glaucoma, and ging pathologically, if not in all its clinical characters, to the subacute . The conjunctiva was little congested; but the pupil was dilated, the ll of great hardness and tension, the cornea rough, the media dull, the papule cupped. On closing the sound eye, he discovered, much to his mfiture, that he could not count fingers. Had I seen this old gentleman e outset of his disorder, I should have counselled iridectomy, which alone l effect a cure. At his great age, and in the existing condition, in which tomy could not be relied upon to give more than partial results, I thought st not to urge any operation, but to lay plainly before his friends the facts the sight was seriously compromised, that iridectomy might be expected ally to restore it, and that no other known treatment offered any such ect; but that against this must be weighed the objections to any sort of tion involving confinement to a room and the inhalation of an anæsthetic aged man of a peculiar temperament. Ultimately it was decided, with oncurrence of his relatives, who included members of our profession, and r. White Cooper, who kindly came into consultation, not to operate. ide of potassium, and nervine tonics and sedatives of various kinds, were ully administered, and a soothing local treatment; but the disease was, as t be expected, unchecked, and the sight is lost in that eye.

is not necessary, of course, here to speak of the usefulness of iridectomy aucoma. That belongs to the acquired facts of science and practice. ssful iridectomies in glaucoma are among the most striking, brilliant, beneficent achievements of surgery; and iridectomy in glaucoma ranks g the greatest surgical discoveries of the age. Here I wish only to point rom the results of my own experience, which has not varied in this respect g the decade, that the sphere of usefulness of iridectomy is narrowed by elays which commonly occur in general practice in recognising acute oma, and the omission to apply at once the one and only remedy—tomy.

e ophthalmoscopic test affords the crucial evidence of the disease, but it not remain unsuspected for want of a few simple rules. 1. *Wherever are superficial inflammation of the eye, spontaneous dilatation of the (generally ovoid) hardness of the eyeball, pain, and diminution of vision, glaucoma may shrewdly be suspected.* Wherever there is *spontaneous tion of the pupil,* accompanying slight redness of the eye, with *hardness e ball and affection of the vision, subacute glaucoma may be suspected.* previous history generally includes the following subjective symptoms. ly increasing farsightedness, haloes seen around lights at night, and ional pains in the eyeball. I leave out of view any ophthalmoscopic or of the optical tests of circumscription of the field of vision, for experts lready be on their guard. The external characters to which I refer in short rules have been more or less typically conjoined in all the cases have come under my notice. I am satisfied that it will be very useful hey should, in the minds of all practitioners, be so associated with glau- (to which they belong) as always to suggest by their occurrence the ble nature of the disease, and to lead to decisive investigation, and, if sary, timely action. More eyes have been, and apparently still are, lost e neglect or misapprehension of glaucomatous affections, than from any single cause.—*British Medical Journal, Feb.* 3, 1872, *p.* 122.

79.—ON GLAUCOMA.

By Dr. H. R. Swanzy, Ophthalmic Surgeon to the Adelaide Hospital, Dublin, and late Assistant to Professor Graefe.

[The great importance of being able to recognise glaucoma depends upon the fact that if left to itself it is certain to lead to absolute blindness, but if proper treatment be applied in good time, nine out of ten cases can be saved.]

In what does this disease consist? It consists in an increased tension within the chamber of the vitreous humour, possibly a hypersecretion of the vitreous itself. What it is which gives rise to this hypersecretion, if such it be, has not yet been discovered, although numbers of experiments have been made on the subject, and theories without end propounded. The simple known fact, however, is that glaucoma consists in an increased tension within the chamber of the vitreous humour, or, as we more commonly say, an increased intra-ocular tension.

You may recognise the increased tension in the present case, as in all others of the disease, by palpation of the globe of the eye with the tips of your two index fingers, when you will find the affected eye much *harder* than the normal globe. In making this examination, I think it is well to support the 2nd, 3rd, and 4th fingers of each hand on the patient's brow over the eye, desiring him to look down at the same time. The pressure you apply to the globe must of course be very gentle, and it will be well for you at first to practice palpation of the globe on yourselves and your friends, so as to form an estimate both of the amount of pressure which you should exert, and of the average tension of normal eyes.

There are two chief forms of glaucoma, namely, simple and inflammatory. Our patient labours under the latter form of the disease.

Simple glaucoma destroys vision very slowly indeed, such a case often lasting two or three years. It is unattended by pain, and the only signs of the disease on the exterior of the eye are usually a few rather engorged episclerotic veins, a shallow anterior chamber (in consequence of the pressure in it being less than in the vitreous humour), and a sluggish pupil (owing to paresis of the ciliary nerves from direct pressure on them).

Patients then, labouring under this form of the disease, will come to you complaining merely of failing vision, you investigate the intra-ocular tension and find it more or less above the normal tension. You then examine the fundus oculi with the ophthalmoscope (a proceeding unattended with difficulty in this form of the disease, as the ocular media remain clear). The surface of the disc, or optic entrance, instead of corresponding almost exactly with the surface of the retina, as it does in the normal eye, is pushed back, so that the appearance of the excavated or cupped disc is presented. The surface of the disc is often driven further back than the external surface of the sclerotic itself. This cupping of the disc results from the fact, that it is the least resisting point of the coats of the eyeball, inasmuch as here the sclerotic is perforated by the filaments of the optic nerve as they enter the eye. In accordance with hydrostatic laws, the pressure within the eyeball must be equal on every part of its walls, and of course the weakest point of these will be the first to give way to an abnormally high pressure. In commencing glaucoma, we may find no cupping of the disc, as it requires the tension to have reached a certain height, and to have lasted a certain time before this effect of it is produced. The high pressure upon the disc and retina paralyses these tissues, and so produces disturbance of vision. The defects of sight display themselves in central vision, so that the patient cannot read so well, or distinguish small objects with his former precision; but, what is of much greater importance for the diagnosis, and in the long run for the patient too, are defects in the eccentric portions of the field of vision, which are apt to be present. They should be looked for, *by lamplight*, in the following way: —directing the patient to close one eye, you cause him to steadily fix the other one (to be examined) on some object at about two feet removed from it (e.g., your hand, or face); you then move a whitish object (your hand) slowly round in the periphery of his field of vision, and about one foot removed from its centre, and observe whether there be any position where the motion of the object cannot be seen. If the glaucoma be somewhat advanced, you will almost constantly find a defect in the inner and upper region. At a still later period this defect may extend all round the periphery of the field of vision, and encroach also towards its centre, so that then the patient finds himself in a condition, which any of you may realise to yourselves, by closing one eye and looking with the other through a roll of music or other small bored cylinder. At last this little remaining glimpse of the world is swallowed up by the ever advancing enemy, and the patient is left in irremediable darkness. Or, the defect in the field may merely advance towards the central point (point of fixation, macula lutia), and having attacked it go no further; such a patient would only be able to see objects held to one side of his field of vision.

Inflammatory glaucoma, of which you have here an example, has a very different course. It rarely extends over a term of more than a year, and usually but a few months. The tension is not a steadily increasing one, but advances in fits and starts, called "attacks of glaucoma." Each attack is attended by dimness of vision (often described by the patient as smoke in the room), by the appearance of rainbows round the candle or lamp, by severe ciliary neuralgia, which extends down the side of the nose, into the malar bone, and into the corresponding brow and side of the head, there is also much tearing and sensitiveness to light, and frequently vomiting. If then, you examine the eye during an attack of this nature you will find it very hard, the anterior chamber shallow, a bright injection surrounding the cornea, and much engorgement of the episclerotic veins. In consequence of pressure upon the ciliary nerves, the sensation of the cornea becomes greatly diminished, as you may discover by touching it with a small shred of paper. You may observe that in the case before us to-day, there is complete anæsthesia of the cornea. In these attacks of glaucoma, you will often find the aqueous and vitreous humours so opaque, that, even if the photophobia allow of the examination, the fundus oculi can scarcely be seen with the ophthalmoscope, the optic disc appearing but indistinctly, somewhat like the sun when seen through a thick fog. Such attacks vary of course in their intensity, from something which the patient himself hardly observes, to one like that I have described. An attack of glaucoma frequently lasts several hours. When it has subsided the media become clear again, the vision becomes almost as distinct as before, and the tension returns nearly to its former condition. Every attack, however, adds something to the permanent intra-ocular tension, takes something from the acuteness of vision, makes another advance towards cupping of the disc, and towards atrophy of the nervous tissues. As the disease gains a footing, the attacks become more frequent. Instead of returning once a fortnight they come once a week, then perhaps once every second day, or every day. At last an attack comes which has no complete intermission, although it may diminish in severity for a few hours, and when this happens we say that the glaucoma itself has arrived. The last moment for the exercise of your art will then also have arrived, for a few days would otherwise suffice to extinguish vision. If the disease be still let run its own course, although sight be completely lost, yet the most violent ciliary neuralgia may continue for months. The eye is gradually disorganised. Cataract forms, and the iris becomes atrophied. Such is the condition of our patient's eye. Still later, the glaucomatous process leads to ecstacy of the sclerotic, purulent inflammation may then come on, and end in phthisis of the eyeball.

Glaucoma, of both forms, is most commonly double sided. Both eyes may be attacked about the same time, but more usually there is an interval of weeks or months, or even longer. In this man the second eye still continues healthy.

Until within the last fifteen years a successful treatment for this terrible disease was unknown. Patients suffering from it, and applying to a surgeon for aid, were simply allowed to go home and get blind. It is to Von Graefe that humanity is indebted for the great discovery that abcision of a portion of the iris (iridectomy) would relieve increased intra-ocular tension. In 1857 he published his first great monograph on the subject; and thousands are the eyes which have been saved since then by the operation. Graefe arrived at this discovery by a remarkable process of clinical observation, of which it would be difficult to convey to you an adequate idea. We are still ignorant as to how iridectomy has the effect of reducing increased intro-ocular tension. Doubtless, one day, this too will be discovered, and I think most likely then, when the cause of the increased pressure itself becomes known. In the meantime, we may be well contented to practise the treatment without being acquainted with its mode of action. How many medicines are administered internally quite as empirically?

In order to perform the iridectomy with a successful result, it is important you should attend to two points. 1. The portion of iris must be excised to its very periphery. 2. It must be a sufficiently wide piece. The first point is attained by making your incision into the anterior chamber, with the little iridectomy knife as far back as possible, i.e., in the corneo-sclerotic border. The width of the portion of iris excised will depend upon the length of the wound, because on withdrawing the knife, the aqueous humour flows out and carries with it a section of iris corresponding to the extent of the wound. This, then, is seized with fine forceps, gently drawn forward a little, and snipped off with the scissors close to its base.

The earlier in the disease iridectomy is performed the more perfect the result obtained, for, in general, we may expect by the operation to retain the degree of vision which exists at the time, but we can rarely calculate upon restoring what is lost. Eccentric defects of the field of vision do not recover after the operation; at best central vision may improve somewhat. You will find the results of the iridectomy in cases of inflammatory glaucoma almost uniformly satisfactory. In cases of simple glaucoma, on the other hand, the operation sometimes appears to arrest the disease only temporarily, and you may afterwards have to employ a second iridectomy in order to complete the cure, and there are sometimes cases in which even that does not produce the desired effect, and where in spite of all your exertions the eyesight will be lost.

Glaucoma is a disease of old age, seldom making its appearance in persons under forty-five.

Before I conclude, I must mention another form of glaucoma, which fortunately is very rare, but for which you should be prepared, namely, glaucoma fulminans. It resembles inflammatory glaucoma, except that it has no premonitory stage, it gives no notice of its approach, but suddenly attacks an eye, which has apparently been until then, perfectly healthy. It comes on with great violence, and frequently destroys vision in a few hours. It consequently admits of no delay in the performance of the iridectomy.

I shall now proceed to operate on the case before you, not with any expectation of restoring a spark of vision to the eye, for the time for this is long since past; but in the hope of relieving the frightful neuralgia from which the patient is, and has been suffering for some months. Even in this I may not succeed; for, when the iris has become so atrophied as you see it here, we cannot speak of the result of the iridectomy with as great certainty as we otherwise could do. In case the pain be not allayed by the operation, I shall be obliged to resort to enucleation of the eye-ball.—*Medical Press and Circular, Nov.* 22, 1871, *p.* 453.

80.—CASE OF AMBLYOPIA.

By Dr. Donald Fraser, Paisley.

[The following is a case of white atrophy of the optic nerve, treated with the continuous galvanic current. It possesses a wider interest than pertains merely to the treatment of an obscure case of eye disease, for, as iritis and its treatment becomes the medium through which important lessons in the therapeutics of mercury are conveyed, so by means of the ophthalmoscope the effect of galvanism on certain affections of the optic nerve may ultimately throw much light on the treatment of nerve affections generally.]

I will now proceed to narrate the case to which I consider these remarks a necessary introduction.

William O., aged 59 years, consulted me on the 8th Sept., 1871, for failure f sight. He was, up till three years ago, a weaver. He had been always a ealthy man, and temperate. For the last ten years he smoked tobacco to the xtent of two-and-a-half ounces per week. At the age of 44 he began to use pectacles for presbyopia. For the last 9 years of his work as a weaver, he xed his eyes severely at pattern weaving, working most of the day, during e greater part of the year, in gaslight. For the last 5 years his sight had een gradually failing. It was not, however, until the beginning of last year at he began to be alarmed at the rapid increase of this failure. He now ecome dyspeptic, low spirited, and weak; all this due, he considered, to the epressing effects of some family troubles. At this time, during the day, a ist came before his eyes, which passed away at twilight, so that by gaslight e could read, for a few minutes at a time the largest type of the newspaper rith No. 6 convex glass. About nine months ago even this became impossile.

When he called upon me he complained of a mist being constantly before is eyes, so that he was unable to recognise his most intimate friends above a ard off. I found that he could read slowly, and with effort, No. 20 of Snelen's test types at 4 inches from his eyes with the right eye, at 8 inches with he left, and at 6 inches with both. On examination with the ophthalmoscope, he outer two-thirds of the optic disc in both eyes was found to be pearly white tint and glistening, and the inner third hyperæmic, the retinal veins were enrged and tortuous, and the arteries diminished in number and calibre. longside some of the vessels were to be seen the white lines said to be characristic of *neuritis descendens*. There was here then a markedly atrophic conition of the optic nerve, most probably primary; there being no certain evience either by the ophthalmoscope, or otherwise, of intra-ocular causes suffient to produce such atrophy. I dismissed the idea of the disease being due tobacco poisoning, although, in some respects the condition of the disc seeml closely allied to what is usually described as due to excess in smoking. At e same time there was not, and never had been, any symptom of cerebral afection. Still I have been strongly impressed with the idea that this was a ase of primary degeneration of the optic nerve, a degeneration which may find llustration in cases of so-called tobacco amaurosis.

Knowing how useless drugs were in such cases, it occurred to me that galvansm might be of use. But taking a serious view of the case, before using uch an agent I advised my patient to consult Dr. Thomas Reid, of Glasgow. r. Reid confirmed my diagnosis, and while considering the case a very unromising one, recommended the use of the bichloride of mercury in combinaon with iodide of potassium. My patient began this treatment on the 10th ptember last, and continued it for a month. During this month he was a od deal at the coast, and came back to town much improved in general health. s sight, however, was scarcely, if at all, improved; although he could read 20 at 7½ inches. At this visit, I passed a current from six cells of a Stöhs battery through the temples for about twenty seconds. On testing his t immediately afterwards, I found that he could read No. 20 at 9½ inches; improvement of two inches within a period of as many minutes. I advised however, to go on with the bichloride mixture for another week or so. I him again on the 8th October, when I found, as I expected, his temporary had left him, he being only able to see No. 20 at 7½ inches as before. I here mention that in reading, my patient always sought to make the best is case, so that the limit of clear definition would be more correctly stated at uch less than the above figure. I again tested him with the current, the lt being an improvement of three inches. Two days afterwards he could d at 8½ inches. I again applied the current, and immediately afterwards he d at 10 inches. Feeling satisfied now that the galvanic treatment would yield d results, I asked him to cease taking the mixture, and to call upon me y morning at 10 a.m. In order to insure accuracy in the results, I kept him he same hour, position as to light, &c. The days at this season and at the ve hour, were usually dark and foggy; conditions, by the way, in which he ld see best. On clear sunny days, he described the mist as being particularly thick and dark before his eyes. About a week after I began the galvan treatment he improved in this respect, being able to see best on a clear day.

I continued the galvanization daily for a month, then every second day, sometimes every third day, and again every day as I thought fit. I sought to avoid the dangers of over-stimulation by the weakness of the current—six cells—and the shortness of the application, which never exceeded thirty seconds. I applied the electrodes at first to the temples, and to the long axis of the head, at each break of the current producing the flash indicative of retinal irritation. Latterly I applied one to the forehead, the other to the tongue, with alternations. Occasionally, and experimentally, I applied a current from eight cells to the cervical sympathetic,—the results, however, never seemed so good as by the other methods.

The treatment was continued for three months; during which time his progress was a matter of daily observation. At the end of that period he could read No. 5½ Snellen with as much ease as at the beginning he could read No. 20. The improvement in his health and spirits due to this progressive recovery of sight has been very marked. It may be here observed that every day there was a distinct improvement of one inch in his ability to read the test-types. This improvement, which affected both eyes, was usually to the extent of two and three inches after the application of the current, though the permanent effect was only one inch.

I have demonstrated a certain amount of improvement by means of the test-types. The evidence of the ophthalmoscope may now be added. On the 19th November, 1871, I fancied, on examining the fundus, that the condition of the disc was, if anything, slightly improved. On the 11th January, 1872, I made a very careful examination, and was satisfied that while the atrophic condition of the disc was still very marked, there was an improvement. This was particularly so in the case of the retinal circulation, the arteries being distinctly larger in calibre, while the veins were smaller and much less tortuous. As it was an important point that there should be no doubt as to this improvement, I again asked my patient to see Dr. Reid, who substantially agreed with me.

The question now is, how far this improvement will go. I do not expect that much more can effected in the case, although I mean to continue the treatment twice a week or so for some time to come. A more important question is, how far will the results be permanent? My impression is, that with care on the part of the patient, his present condition may be maintained for a considerable length of time, if not permanently.

But whether this be so or not, it is evident from the ophthalmoscopic examinations, that we have not simply stimulated the patient's retina, but that we have distinctly improved its circulation, as well as the nutrition of the optic nerve, and that this has been done in a disease in which ordinary medical treatment is practically useless.

It seems likely that in this case the galvanism acted as a direct stimulant to the optic nerve, and that the improvement in the retinal circulation followed secondarily, in great part as the result of the improved nutrition of nerve, and partly from the direct action of the current upon the vessels of the retina. The idea that the influence has been mainly exerted upon the nerve is impressed upon me by the results of similar treatment in a case of so-called atrophy of the optic disc, depending distinctly upon intra-ocular changes, probably choroiditis. The patient was a delicate lad, eighteen years of age, who had suffered from some severe inflammatory affection of the eyes in childhood. In this case not the slightest improvement was observed (or indeed scarcely expected) from a very careful galvanization, conducted as in William O.'s case, and continued for many weeks. Now, if the retinal circulation had been affected in this case, as it is found to be in galvanization of the cervical sympathetic, some improvement might reasonably have been expected. It is possible that in cases of this kind, with a continuous course of galvanization of the sympathetic, in conjunction with other treatment, good results may yet be obtained. But for those purely nervous degenerations of which W. O.'s case is a type, I am firmly persuaded that the continuous galvanic current will e the appropriate remedy of the future.—*Glasgow Medical Journal, Feb.* 872, *p.* 168.

2.—ON THE FORM OF AMAUROSIS SUPPOSED TO BE DUE TO TOBACCO.

By Jonathan Hutchinson, Esq.

Mr. Hutchinson has twice before published his experience on this important ubject. One of his former papers will be found in *Retrospect*, vol. 1, p. 274.]

Any one who may take the trouble to compare the cases given in these reports, ill be struck by their similarity, and by the remarkable confirmation which he later papers give to the conclusions of the first. A very great disproportion the numbers of the two sexes who become the subjects of "idiopathic amausis," was one of the facts brought out (for the first time) by my first report. n it the numbers were 37 men and 3 women, and in the second they were 34 en and 5 women. In the present one they will be found to be 28 men and nly one woman. There can, therefore, be no reasonable doubt that we have to eal with a formidable type of symmetrical amaurosis which is almost excluvely confined to the male sex. For myself, I may briefly avow that I have rupulously investigated other possible causes, and that I feel no hesitation in elieving that in most of these cases tobacco is the real one.

The facts as regards the one woman whose case is included in the present eries, are extremely interesting. Her case in every feature resembled those hich in males I attribute to smoking, and upon this point I had made special omment to those who were attending my class. After I had done so, she inrmed me that one of her sons had formerly been my patient, and that a nehew had been under the care of Mr. Hulke. I referred to my notes and Mr. ulke was kind enough also to refer to his, and we each found that the cases ad been diagnosed as tobacco amaurosis. In each the patient was a young an and a smoker. Now the lesson of these facts seems to me to support the pinion I have long held, that when tobacco causes blindness it does so in virue of an idiosyncracy. It is by no means improbable that such idiosyncracy ill be found occasionally in several members of the same family, and further hat it may involve liability to suffer from other influences besides tobacco moking. Thus, then, we have two young men, cousins, attacked by amaurosis t an early age in virtue of an idiosyncracy rendering them liable to special oisoning from tobacco; and we have a woman, who had never smoked, the other of one and aunt of the other, at a much later period of life becoming the ubject of a similar form of nerve disorder in virtue of the same peculiarity of iathesis acted upon by a different exciting cause. In her, not improbably, he exciting cause was the disturbance consequent upon the cessation of mentruation.

Holding the opinion that there must be some pre-existing peculiarity in the ervous systems of those who become the subjects of tobacco amaurosis, I have een very anxious to discover if possible whether it reveals itself by any other igns. The only points in this direction to which I feel at present inclined to ttach any importance are the following. Not unfrequently it will be found hat these patients have had unusual difficulty in learning to smoke, and have hroughout life displayed special susceptibility to its influence, and that also hey have often been beyond the average liable to suffer from sea sickness. In he tabular statement some information will in many cases be found given on hese points. My attention to the matter of sea sickness was first given in conequence of the statements of some patients who were sailors, and who specially eferred to it.

We have, during the last few years, made the observation, that those who uffer are, almost invariably, smokers of *shag*, the most deleterious form of toacco. As regards the results from disuse of tobacco my impression is strong, hat almost invariably when the disuse is real and complete, the state of vision mproves. I cannot exaggerate the expression of my conviction as to the duty f urging *immediate* and *complete* abstinence in the early stages of this most serious malady. Many of my patients came too late, having continued to smoke until the disease was far advanced. I have never seen a case in the early stage in which the disease went on to blindness if the patient had strength of will to give up the habit. In a large majority of those in whom, according to the patient's account, the habit was wholly abandoned, I had reason to suspect that it was only reduced. My experience on this point fully confirms that of Dr. Mackenzie, that there are those "who would rather smoke than see." It is very few, however, who have the honesty to admit their inability to give up the habit, and hence a very annoying source of fallacy in our inferences as to the effect of treatment.—*Ophthalmic Hospital Reports, Nov.* 1871, *p.* 169.

83.—AMAUROSIS SUCCESSFULLY TREATED WITH HYPODERMIC INJECTIONS OF STRYCHNIA.

By A. S. G. Jayakar, Esq.

P. H., a Rajpoot, aged about 35, was admitted into Hutteesingh's Hospital, on March 21, 1871, with total amaurosis of both eyes. He had an amaurotic look, and his vision was very dim, being perfectly unable to distinguish light from darkness. He constantly turned the eye-balls upwards, almost under the upper lids, evidently for the purpose of attempting to bring the remaining small portion of the retina in axis with the object seen.

History.—The history of the case was rather unsatisfactory, the patient being far from intelligent. The only account he could give of his blindness was that it first commenced thirteen months ago with a sensation of burning in both the eyes, followed by a gradual loss of sight. He then suffered from fever for about ten months, and had suffered from total amaurosis for nine months before his admission.

Ophthalmoscopic Examination.—Right eye: image natural; media dull; disc anæmic; no venous congestion; shape of blood-vessels undefined; arteries atrophied; retina pale and anæmic. Left eye, image natural; media dull; disc and retina anæmic, the same as in the other eye; arteries atrophied.

There was no photophobia or pain of any kind in either eye.

On March 31 one twenty-fourth of a grain of strychnia was injected into the back of the neck.

April 1. The patient expressed himself slightly relieved. He could count fingers and distinguish colours with the left eye. With the right he could only distinguish light from darkness.

4th. There was some more improvement in the right eye. He could count fingers and tell different colours with it; but there was no progress with the left eye. He was ordered to again have one twenty-fourth of a grain of strychnia injected.

5th. Left eye considerably improved; so also the right, to a certain extent. Could tell the shapes of different objects and count dots.

9th. One twenty-fourth of a grain of strychnia was again injected.

10th. There was a marked and decided improvement in the left eye, the right also having improved to some extent. With the left he could tell minute objects.

18th. Another injection administered.

19th. The patient could count dots No. II. (arranged on Snellen's test-type scale). With the right he could only count No. XX.

May 21st. An opthalmoscopic examination revealed an improved state of both the disc and retina in the left eye; whilst the same structures in the right eye were in much about the same condition as on his admission.

7th. Hypodermic injection of strychnia was given in the right temple.

12th. The injection in the right temple was repeated.

22nd. No change having been noticed in the right eye since the last note, the injection was again repeated, with this beneficial effect: that he could see *No. VI.* easily with the right eye, the sight in the left eye remaining stationary.

By June 6 (the day on which he was discharged from the Hospital, being very anxious to return home) the physical condition of both the disc and retina in the right eye had greatly improved; and, instead of atrophic arteries, there were well-defined bloodvessels in both the eyes. Vision of right eye could count No. VI.; with left eye could count No II.

Remarks.—This was evidently a case of asthenia of the retina and discs, accompanied with exhaustion of the optic nervous apparatus, and a case, therefore, best suited for treatment with hypodermic injections of strychnia, so strongly advocated in these cases by Professor Nagel, of Tübingen. The patient had altogether seven injections between March 31 and May 22, the quantity of strychnia used being about one-third of a grain; and, on the whole, the progress of the case had been satisfactory. Perhaps, had a larger quantity been injected each time, a more rapid improvement might have been noticed; and, if the line of treatment had been persevered with for a few weeks more, I have no doubt but that his right eye would have eventually come to the same state as the left, so as to be able to count No. II., and even some further improvement may have been reasonably expected in the left.—*Med. Times and Gazette, April* 6, 1872, *p.* 398.

84.—SUBSTITUTE FOR THE OPHTHALMOSCOPIC MIRROR.

By Dr. J. S. Torrop, Heywood.

The ophthalmoscope having come into general use in medicine, it may save considerable embarrassment in choosing from among the vast variety of instruments offered, to point out that we all possess one already in our watch-pockets—viz., a watch-glass.

Any transparent reflecting body is an ophthalmoscope, and, with the ordinary lamp, quite enough light is returned from the concavity of a watch-glass to display all the details of the fund of the eye. It is used in the same manner as the mirror, but possesses this advantage that (in viewing the inverted image), it may be held at any convenient distance from the observer's eye, so long as he looks through it. Therefore, if more light be desired, the watch-glass may be approximated to the patient's eye till all its reflection is concentrated on his pupil. Atropine is not required except in special cases, as there is no glare to provoke contraction of the pupil.

Any watch-glass will do provided the glass be (as it is in almost all of them) equally thick throughout, so as not to distort the image seen through it. Improved forms will, of course, at once occur to the reader—e.g., with a central convexity to enlarge the image or concavity to correct myopia; but to discuss these would be foreign to my purpose, which is merely to point out the use of what we have already got; the employment of which, I may add, was suggested to me by the reports of some of Mr. Carter's admirable experiments.—*Lancet, March* 2, 1872, *p.* 309.

85.—ON POLYPUS OF THE EAR.

By Dr. W. B. Dalby, Lecturer on Aural Surgery at St. George's Hospital.

[It will generally be found that polypi of the ear have been preceded by disease of the middle ear, which has resulted in a perforation of the tympanic membrane, purulent catarrh with scarlet fever being the commonest of all forms. This rule is, however, not invariable, for they are sometimes found after death as a commencing growth in the cavity of the tympanum, and sometimes they appear in the external auditory meatus independently of any tympanic disease.]

They are at times very vascular, and when they are so there is a good deal of bleeding after their removal. When they do not attain any considerable size, a very common appearance for them to present is a red, fleshy, globular mass projecting through a perforation in the membrane. In such a case they grow from the cavity of the tympanum, and from their situation, by preventing the escape of discharge, sometimes thus become the indirect cause of death in those fatal cases where the lateral sinus and brain become involved.

The reasons for which their removal generally is demanded are the continuous offensive discharge from the ear, and deafness, both of which symptoms are their invariable accompaniments. The most usual position from which aural polypi arise is some part of the walls or roof of the tympanum; they also spring from the external meatus and from the tympanic membrane itself. After one polypus has been removed from the cavity of the tympanum, it not unfrequently happens that one or even two more are brought into view. In looking down a speculum where the membrane has been completely destroyed, it is not always easy to decide at first sight whether the red mass at the bottom of the meatus is a small polypus, or simply granulations on the lining membrane of tympanum. This, however, is ascertained by examination with a probe, when a polypus will be found to move under the touch. Indeed, this should always be practised so as to determine, as nearly as possible, the point from which the growth arises.

The indications for treatment of aural polypi are threefold. Firstly, to remove them; secondly, to apply caustics to their root regularly and for a sufficiently long time, to ensure against their reproduction; and, thirdly, when they exist with a perforation, to keep the tympanum clear of secretion, and to induce a more healthy condition of the lining membrane of this cavity and the Eustachian tube; in short, in addition to directing attention to the polypus, the case must be treated as an ordinary perforation of the tympanic membrane.

It is most desirable that the tediousness of these cases should be recognised. Although, after removal, a few applications of some caustic will occasionally eradicate them, and this especially when they arise from the external meatus, the treatment will often extend over several weeks, or even months; the time occupied depending upon the tendency to reproduction which the growth manifests.

Judging from the cases which present themselves where polypi were taken away some years ago and have recurred, only to be again taken away and again to reappear, my conviction is that until late years the perseverance necessary for the successful termination of this class of cases was but imperfectly estimated.

With regard to removing the polypus. If it be very large, the best instrument is a Wilde's snare armed with fine gimp. If of moderate size or small, simple rectangular forceps, meeting at the points by two rings, are the most convenient. The lever ring forceps of Toynbee are not often necessary, and at the best will only grasp a very small growth. While using any kind, or in applying caustics, the meatus should be illuminated with diffused daylight through the largest speculum that it will hold, the reflector being worn on the forehead and fastened with a band round the head; both hands will thus be quite free. The most effective caustic I find to be chloro-acetic acid, applied on a very small camels'-hair brush. In using this, care should be taken not to touch the meatus; and provided this is done, very little pain is felt; but if this should happen, it is relieved at once by syringing with warm water. Potassa fusa and chloride of zinc are not so manageable, and the former is liable to spread on to more parts than it is necessary should be touched. Nitrate of silver is not sufficiently powerful, but it is a very useful caustic to apply to granulations on the membrane or on the surface of the cavity of the tympanum. The liquor plumbi is useful for the same purpose, and may be applied with a camels'-hair brush, or in the form of a piece of cotton-wool soaked in it and worn for a few hours every day pressed on to the part. It is well, before any of these applications, to dry the part carefully with cotton wool.

A plan for cleansing the middle ear of secretion and applying solutions to the whole extent of the lining membrane in cases of perforation, by forcing them through the tympanum and Eustachian tube by means of a syringe with the nozzle guarded by india-rubber, and fitting the external meatus, was described by me in the Lancet in April and August, 1870; and in all cases where there is a perforation of the membrane and a pervious Eustachian tube, it is most useful. The patient may also be instructed to practise a plan which I first saw Dr. Joseph Gruber at Vienna teach at his clinique to patients with perforations—viz., to incline the head to the affected side; and, after a drachm or so of the lotion to be used has been passed into the inferior nasal meatus of the same side with a small glass syringe, to close the nostrils firmly with the fingers, and the mouth also being closed, to blow vigorously, when the fluid will run through the Eustachian tube, and out of the perforation; thus cleansing the lining membrane in its course. When neither of these plans succeed—and occasionally they fail on account of some temporary obstruction—the Eustachian cathether may be employed with an india-rubber bag fitting into its open end; and thus fluids may be made to pass directly through the Eustachian tube into the tympanum, and through the perforation. Speaking generally, however, I should like to be understood to imply that no pains should be spared to keep the cavity of the tympanum thoroughly cleansed; and, in case of an adult, no trouble which has this in view should be grudged by the patient. He should be provided with a syringe which he can use without fear of hurting himself, and with this he should wash the ear twice a day. Lotions (a very good one is sulphate of zinc and solution of opium, four grains and ten minims respectively to the ounce) should be used in the following way:—The meatus is filled with the lotion, previously warmed, and with the affected side uppermost and horizontal, let the patient blow through the perforation; this will cause the fluid to bubble in the meatus, and, on ceasing to blow, some little of it will pass into the throat, and thus it will come in contact with all parts of the lining membrane of the middle ear. Observation teaches me to be more hopeful as to the results of treatment in cases of perforation when they are accompanied by polypi than when they are not. I cannot in any way account for this, any more than I am able to do—judging simply by appearances as regards size, shape, &c.—for the immense variations in improvements to hearing that are met with after treatment in all cases of perforation. When the lining membrane of the tympana and Eustachian tubes becomes more healthy, the improvements that take place, with regard to the hearing power, are with some very great indeed, while with others they are scarcely perceptible. With these latter a careful examination with the tuning-fork, the history, and some subjective symptoms—tinnitus, worse hearing after fatigue, &c.—will frequently detect a nervous lesion accompanying the tympanic disease.—*Lancet*, *Dec.* 9, 1872, *p.* 809.

DISEASES OF THE SKIN.

86.—RINGWORM IN SCHOOLS.

By Dr. Tilbury Fox, Physician to the Department for Diseases of the Skin, University College Hospital.

[The management of ringworm in private practice, is by no means a simple matter, and in large schools it is of course far more difficult and involves a good deal of responsibility, as a mistake may involve the principal in serious annoyance and pecuniary loss. Dr. Fox's attention has lately been drawn especially to this subject by an outbreak in a large school near London, in which 300 cases of the disease occurred.]

Origin and Dissemination.—Firstly, I do not understand that a school is properly managed unless every child admitted is shown to be free from ringworm of the head (tinea tonsurans) and ringworm of the body (tinea circinata), either as certified by a medical practitioner, or by a careful examination at the time of admission by some competent person. A matron or good nurse can have no difficulty in preventing the introduction of ringworm as the rule. Every child with ringworm (tinea tonsurans) has certain "scurfy patches" or *spots where the hair looks shrivelled* or unhealthy. Such appearances and red scurfy patches on the body are readily detected by any one who has a pair of eyes, and they should suffice to excite suspicion and to lead to medical examination. Secondly, every week at least a careful inspection of heads should be made in schools. The heads of girls should especially be searched on account of their long hair. In this way the earliest signs of disease must be detected. Ringworm of the body should be recognised more decidedly than it is as the frequent source of ringworm of the head, and dealt with accordingly.

Well, once introduced into schools, how is ringworm spread? (*a*) By neglect, of course; (*b*) by actual contact of the healthy with the diseased; (*c*) by the common use of towels and brushes by the diseased and healthy; (*d*) by the air of the institution, which, under certain circumstances, is loaded with the germs of the fungus—trichophyton tonsurans.

I want specially to notice the last point. When I came to collect the dust deposited from the air in the wards of the institution in which the outbreak of ringworm before referred to occurred, I found that it contained fungus elements in abundance. This observation I believe to be a novel one. The achorion has been detected in the air passing over children affected with favus, I know; but I speak of the existence of the trichophyton in the air when no artificial means have been adopted to disseminate it there.

[This abundance of the fungus of ringworm no doubt arose from the large number of children collected together—of whom there were 121 in one room. The dust contained abundant spores of trichophyton, which were seen to develope into mycelium two days subsequently.]

Treatment.—As regards the actually diseased.—Isolation at all hazards is the first thing to do. I will only say on this point that cases of ringworm of the body must be isolated. I think this of essential importance in the case of schools. It is not, however, thought of any moment as the rule. Where a number of cases occur, it is better to separate instances of very bad and extensive diseases again from slight *new* cases and convalescents, for the simple reason that active treatment may at once annihilate the disease in the former, and in the new cases and convalescents fresh implantations over the, in the main, healthy area of the scalp may be taking place from contact with bad cases of tinea. I would, of course, only adopt this plan where the cases of disease are very numerous—say thirty, forty, and fifty or more.

There are, next, certainly general considerations to be taken account of. Attention to the dietary is one; for the underfed, and ill-nourished, and ill-kept furnish the most appropriate nidus for ringworm. All deficiency in meat should be rectified, and in case the attacked or the non-infected look sickly or pallid, the allowance of meat and fresh vegetables should be increased and supplemented by iron and cod-liver oil. So, again, the cubic space allotted to each child should be ample, ventilation free, and cleanliness enforced with exceptional strictness. One word more as regards the general health of children. If with a rigorous system of inspection in constant operation many cases rapidly appear, and, in spite of hygienic measures, spread, the children furnish clearly a very suitable soil, and the dietary of the children should be looked to. If ringworm becomes epidemic, with a *bad system of inspection*, it implies simply neglect, of course. Here isolation is the main thing needed to protect the healthy, and not feeding up.

The treatment must be very briefly referred to. In all cases in schools the hair should be cut short, close to the scalp. Recent cases are at once checked and often cured by simple blistering. The disease, not having reached the bottom of the hair-follicles, is at once accessible to remedies. Cases of ringworm may be dealt with in the same way. The use of strong acetic acid is perhaps as good as the blistering fluid. If the case is not very recent, epilation of diseased hairs, after the Paris fashion, should be practised. It is generally "too much trouble to do this." I next enforce the use, every few days, of Coster's paste to the extent of some five or six applications, and the subsequent use, night and morning, of some parasiticide ointment, diluted citrine ointment, or sulphur, creosote, and ammonio-chloride of mercury. The head should be washed each day and well greased. The latter prevents the escape and dissemination of fungus germs. If preferred the head may be kept soaked in diluted sulphurous acid; of course a proper cap of silk should be worn.

As regards the surroundings and belongings of the attacked.—It is scarcely ecessary to do more than refer to the necessity of thoroughly cleansing the rushes, combs, and towels of the diseased, and seeing that these are not used n common by the healthy and the infected. Towels should be well boiled. To ne novel point I must direct special attention. It is the disinfection of the air f the wards in which a large number of cases of ringworm have been. My recent bservations show that the fungus germs are floating in the air, and though I had until lately no experience to go upon, because the observation is a novel ne, yet I have no hesitation in saying that the air of the wards should be disnfected by burning sulphur if, after complete isolation has been practised where many cases of ringworm have occurred, other instances of disease still continue to appear amongst the previously healthy.

The prevention of new outbreaks.—As regards the reimportation by those apparently convalescent.—No more puzzling problem is presented to the practitioner than that of saying when a child "is well of ringworm," and "fit to go back to school." I err on the side of caution if there is the least doubt, and advise that the same course be taken by others. When a child is well—that is to say, ncapable of reimporting or redisseminating the disease amongst his fellows—there will be present certain naked-eye characters and microscopic appearances. The hair will be growing vigorously and naturally in the original sites of the disease; there will be no scurfness, no broken-off hairs, and the structure of the hair and its sheaths will be properly developed and free from fungus elements. If the hair is dull and dry, suspicion should be excited; and if the suspected surface is *studded over with short broken-off hairs (readily overlooked)*, there is still disease present. The fungus will be formed in abundance in the short broken-off hairs. As a rough guide, this is the best. Condemn a child any portion of whose scalp is studded with the little dark points of short broken-off hairs. This is the rule I observed. But no one can be certain in any case without a microscopical examination. If the root is well formed, and the hair sheaths likewise; if fenestrated membrane can be seen, and no fungus detected, then all is right. Of course, fungus in any abundance is at once discovered. The doubtful cases are those in which the root seems healthy, but the shaft of the pulled-out hair is observed to be surrounded at its follicular portion above the root with epithelial and exudation matter. This may be an indication that irritation is being set up by the remedies, the ringworm itself being well. I see many cases of this kind, and in them the roots and surrounding structures, and hair-shaft, are healthily formed, whilst no fungus elements are to be detected in the material surrounding the hair. The scalp in these cases is tender, more or less swollen, and reddened, the hair at the same time growing well and vigorously. Perhaps the plainest and easiest guide to disease still existing, is the presence of short broken-off hairs.

Rekindling of the disease from special causes.—In order that ringworm may not "break out afresh" in schools, the non-infected must observe all those directions which were referred to under the head of "dissemination of the disease." Especially it is important to keep heads perfectly clean by frequent washings, and to keep them fairly greased or oiled. To this latter point I attach much importance.

This, in short, is a sketch of the means to be followed in managing ringworm in schools. There are those who think the use of a weak parasiticide to the healthy is advisable. Well, there can be no objection to sponging the heads, even daily, of the healthy with diluted sulphurous acid, one part to six of water, or better, with diluted acetic acid, one part to four or six of water.—*Lancet, Jan.* 6, 1872, *p.* 5.

87.—ON THE TREATMENT OF ECZEMA.

By J. L. MILTON, Esq., Surgeon to St John's Hospital for Skin Diseases, Dublin.

External Applications.—In acute or very slight cases of eczema almost any mild astringent will suffice. Perhaps among the best we may rank subnitrate of bismuth in elderflower-water or camphor mixture, or liquor of the diacetate of lead, two drachms to six ounces of either fluid. When expense is not an object half an ounce of glycerine should be added, as from its faculty of retaining moisture it powerfully aids the purpose which lotions are intended to serve.

R. Bismuthi subnitratis, ʒ ss.; glycerinæ, ℥ ss.; spiritûs lavandulæ, ʒ iij; aquæ flor sambuci, ℥ vij., ♏ ft. lotio.

R. Liquoris plumb. subacetatis, ʒ ij.; spiritûs rectificati, ʒ iij; glycerinæ, ʒ iv.; mist. camphoræ, ℥ v. M. ft. lotio.

When the essence of camphor can be procured it may be substituted for the rectified spirits.

When there is a large weeping surface, particularly in children, or of long standing in adults, the reader may try Dr. Hughes Bennett's plan. It consists in applying a solution of carbonate of soda, half a drachm to a drachm in eight or ten ounces of water, by means of a piece of lint soaked in the liquid and laid upon the part affected. The lint must be covered with thin gutta-percha or oiled silk to prevent its getting dry, or else the management of the affair must be entrusted to the care of some person who will see that the lint is kept moist, for which purpose it should be sprinkled continuously with cold water, the lotion being only occasionally applied. If this plan be properly carried out, if the linen be kept wet all day long, and still more if this can be effected day and night, the results are often marvellous; but if it get dry the soda speedily irritates the skin. The addition, however, of glycerine, half an ounce to eight ounces of fluid, will mitigate this to a certain extent, but nothing compensates for want of attention. Dr. Wallace has reported some cases in which the beneficial results of this plan were very marked. My experience of it is that though it gives great relief, yet it has very little curative power, that it is only suitable for a large wet surface, and that it is apt, in eczema of the head, to give a bad cold.

In chronic eczema a lead lotion, the same as above, can be used. In many cases where the surface is very irritable, it answers best when gently warmed previously. So soon as the discharge and inflammation are checked, zinc ointment forms an admirable dressing. It should be gently melted down, or rubbed down with an eighth part of spirit of camphor, and smeared like thin cream on the part. All surfaces to which ointment is applied should also be covered with old linen. When once ointments are begun with no more watery applications should be used, nor should the part be washed, except when the eczema is seated on the head and discharging freely, as happens sometimes, especially in children, or when in parts where washing cannot always be avoided, as the hands, face, &c. At such times the discharge, when the eczema assails the head, is sometimes retained under the crusts, or the hair gets matted with it. Here it is very useful to poultice the crusts with mashed turnip or bread and water, till they are thoroughly softened, and then remove them by very gentle washing with hot water and yolk of egg, or what I prefer to anything else, the St. John's Hospital soap. Mashed turnips is the best poultice I know of; it possesses the great advantage of rapidly removing any unpleasant smell. In some cases a weak lotion of chloride of zinc, a grain to an ounce, with the addition of half a drachm of mucilage, is a very valuable application to the head; it is only here that I have found the chloride of zinc useful. But for the entire removal of eczema in the dry stage we must turn to a more potent remedy—one of the nitrates of mercury. I have never seen the oxide of zinc ointment—even that prepared by Messrs. Bell, of Oxford-street, really cure this disease when severe; and I have repeatedly treated in the same patient, one patch of eczema with the dilute nitrate of mercury ointment, and one with the oxide of zinc, the result being invariably most decidedly in favour of the former; I have also made similar trials with the ointment of the nitric oxide as against that of the yellow nitrate, and am disposed to prefer the latter. Indeed, in my hands, the ointment of the yellow nitrate diluted, has proved superior to any application I have seen tried. Two or three years ago I showed several surgeons the effects of its action compared with the chloride of zinc as recommended by Dr. McCall Anderson, and the zinc ointment. Three patches were selected on the same patient, pretty nearly of like size and in the same stage. One was treated with the solution of the chloride, one with the zinc ointment, and one with dilute nitrine ointment.

At the end of a few days it was manifest to every one that the action of the nitrate was superior to that of the zinc ointment, and this again to the action of the chloride; and by the time that the patch treated with the nitrate was healed, that to which the zinc ointment had been applied was much better, while that treated with the solution might be roughly computed, so far as such computations hold good, at only half way towards a cure. These applications were now abandoned, and the diluted nitrate was alone resorted to, under the influence of which the progress of the two remaining patches was soon visibly accelerated.

But if the application is to be useful it is indispensable that it should be properly made up and properly employed. In the first place, pure, well-made nitrate of mercury ointment only should be selected. A great deal of that which is generally used is totally worthless, being dry, dirty green, and rancid, spoiling almost as soon as it is diluted; whereas, when properly prepared it retains its bright yellow colour for months. The dilute ointment should only be prepared when it is wanted, and the best plan is simply for the patient to rub it down with a little sweet almond oil till it is of the consistence of cream, and always to throw away what is left. If the eruption be seated on the head in children, the hair should be cut off and the ointment applied night and morning; a linen cap should also be worn day and night. But grown persons cannot, or will not submit to this; the hair should, therefore, be parted, any crusts gently detached, and the ointment rubbed carefully, but not roughly, in, the cap being worn at night only. Should the hands be affected soft leather gloves with the tips of the fingers cut off, should be worn during the night, and, if possible, during the day also. Where there is very little hair, and the eruption is only slight, the ammoniated mercury ointment of the "British Pharmacopœia" may be tried; it is one of the cleanliest and least offensive preparations that we possess, but in point of efficiency I would place it decidedly below the nitrate ointment. Some persons with a strong tendency to eczema, or after the cure of the worst part of their complaint, suffer a great deal from a chapped, red and tender state of the skin; cold cream is often very useful in relieving this. A powder composed of equal parts of finely ground American corn flour, and oxide of zinc, with camphor, dusted over the face during the day time, especially if the patient is to be exposed to a cold wind, is also of great service. Whatever theoretical objections may be made, ointments cannot be too sedulously employed in eczema even during the day, and when the eruption is seated on the face and hands. When the hair is falling from eczema, I know of no remedy so effectual as cutting it quite short, and blistering it as for alopecia. With regard to the itching I never yet saw any remedy materially affect it. The cure of the itching is the cure of the disease. The same symptom holds good of another disagreeable but rare symptom—a kind of neuralgia or rheumatism of the skin sometimes seen in eczematous patients. Mr. Wilson says he has found no remedy for it equal to a solution of nitrate of silver, a grain in an ounce of spirit of nitric ether.

Dr. McCall Anderson seems to think the lard is the chief agent in the benefit said to be effected by mercurial ointments. It happens that I not only have subjected, but am every day subjecting, this doctrine to a test; for it is a constant practice with me to prescribe, at the same time, pure lard and ointment of the nitrate of mercury, the former to be used when the nitrate sets up much irritation, and I haver epeatedly had occasion to satisfy myself that the ointment is much more powerful than the lard.—*Medical Press and Circular, Nov.* 29, 1871, *p.* 479.

88.—CARBOLIC ACID IN PARONYCHIA AND STINGS.

By Dr. Alex. E. McRae, Fettercairn.

I have very frequently treated wounds by carbolic acid; not entirely according to Mr. Lister's elaborate system—for in country practice it is not always practicable—but by the more simple plan of syringing the wound, or washing the *surface with a weak solution* in water or in oil of the acid, and dressing it with the same. Its remarkable success over every agent I had ever used, or seen used, led me to use it in the following cases as a forlorn hope. The results obtained must be my apology for this record.

Case 1.—T. C., an officer of dragoons, had been suffering from whitlow of the last phalange of the right thumb for nearly three weeks. The thumb was very much swollen: but the first phalange was much more so, and redder than the last, although in the last the pain was very much greater. The wrist also was affected, and the glands of the axilla were tender and swollen. He appeared to have suffered a great deal, and was much weakened by want of sleep and of his customary recreations. He had tried several times to prick the swelling, but never went beyond the cutis vera. Much against his will, I laid the swelling freely open from the middle of the first to the tip of the last phalange, and a good deal of bloody pus escaped. For the next three weeks the wound healed greatly; a very offensive ichorous discharge of blood and pus now came away, and the wound looked unwholesome. On examination, the probe distinctly grated upon the bone, without any pain; but whenever the tissues alongside the bone were touched, the pain was very sharp. The relative merits of excision and amputation were considered. Before proceeding to either, however, I determined to try carbolic acid, by syringing several times a day; and the wound healed up in fourteen days. As the tissues were very loose, I applied a coating of collodion, and made a slit in the envelope thus produced, opposite the wound, as a vent for the discharge.

Case 2.—W. McA., a gamekeeper, healthy and robust, was bitten in the last phalange of the first finger of the right hand, by a tailor, in a drunken brawl. The marks of the teeth were on each side, and seemed deep; the parts were painful. In a few hours the pain increased, inflammation set in, and an unwholesome discharge commenced. The finger, hand, and arm, and ultimately the glands in the axilla, became much swollen. In about a month, the probe could be passed through a sinus from the one opening to the other, grating on the face of the bone. I recommended excision. The man consulted another surgeon, who was of the same opinion as myself, and gave the same advice. Case 1 was by this time better; and I resolved to try carbolic acid. I gave him a syringe and a quantity of the solution, with instructions how to use it. Not hearing of him for weeks, I concluded that he had gone to some hospital to save further expense. However, about a month after he got the solution, I met him by chance, and, on examination, found the wound quite healed up, the bone being intact, and a very slight cicatrix on each side. Flexion was impaired, but extension was perfect.

Case 3.—Two bees stung me on the tip of the left ear. My pupil, being at hand, extracted both stings with tweezers. No sooner, however, had he done so, than the part became very painful, and before I reached the surgery—a distance of only thirty yards—the pain had extended all over the left side of my head as far as the mesial line. The pain was exquisite; and over the temple and mastoid process was most agonizing. Thinking that undoubtedly nothing but a germ of some kind could produce such rapid results, I determined to try carbolic acid; and, having some solution ready (1 to 100), I had it injected by the hypodermic syringe into the orifices left by the stings. If the advent of the pain was rapid, its exodus was equally so. I had the feeling all that day, on the left side of the head, as if my hair had been sharply pulled; and the temperature of the left ear remained for the rest of the day much higher than that of the right.

It is, I think, very generally admitted that whitlow is caused by the absorption of disease-germs through the cuticle, either by the natural openings or by a solution of continuity. Undoubtedly this had been the case in Cases 1 and 2, and probably also in Case 3. Whether the original germ alone caused the mischief, is a point I must leave for others to decide. Case 3 may have been caused by a chemical agent; but the process of destruction did not go on until the antiseptic influence of carbolic acid was brought to bear upon it.

It does sound strange to say that a bone denuded of its periosteum, and in *all probability* dead, was restored to life, so to speak, by the application of an *antiseptic* agent. I can account for it in no other way than by supposing that *the* denudation was only partial, that vitality was kept up by the parts in ctual contact, and that, on the destruction and displacement of the intervening oreign body, the parts again healed and united, as we see in the healing of a inus, the transplantation of teeth, or the more recent skin-grafting.

I would advocate, therefore, most strongly the use of carbolic acid as a dressing for unwholesome wounds, however hopeless the case may seem. By its use, pain is decreased, or altogether becomes *nil;* excessive granulations are arrested; unwholesome discharges are checked; bad odors are dispelled; the endency to the destruction of tissue is replaced by a proclivity to heal; and he cicatrices are more sightly than those treated by the older methods—collodion and water-dressings perhaps alone excepted.—*British Medical Journal, April* 18, 1872, *p.* 392.

SYPHILITIC DISEASES.

89.—CARBOLIC ACID IN GONORRHŒA.

By George Ashmead, Esq., Brierley Hill.

Mr. Ashmead has been employing carbolic acid in combination with tannin and glycerine as an injection in gonorrhœa.]

The following has been the formula employed :—Carbolic acid, eight grains; tannic acid, eight grains; glycerine, half an ounce; water to one ounce. It appears to act as an antiseptic, arresting the discharge, and cutting short the disease. The following are two cases out of seven treated with carbolic acid.

A. B., aged twenty-two, sanguine temperament, presented himself with the urethral discharge, and complained of much smarting and scalding during micturition. Rest, abstinence from stimulants, and light diet were enjoined. This was, however, declared out of the question, as the following week was to be one of active exercise, and a more than usual amount of exertion. Inability to avoid stimulants was also pleaded. The above injection was ordered to be used twice a day, and a mixture containing bicarbonate of potash, hyoscyamus, and decoction of broom prescribed. After the first day a spot or two were observable on the lint in the morning, and the painful symptoms disappeared; on the fourth day, considering himself cured, he neither took medicine nor injected; and on the morning of the fifth found a considerable discharge and some scalding. I saw him again on the ninth day, when he considered himself quite well; but, at my suggestion, continued the medicine and injection for a short time longer.

B. C., aged twenty, came with a gleet of some months' standing, for which he had used a variety of injections to little purpose. He was ordered to use the carbolic acid injection twice a day. After injecting a few times the gleet entirely disappeared, but returned a fortnight afterward after a "big drink." This, however, soon vanished on resuming treatment. He keeps an injection by him "for fear it should return."—*Lancet, Dec.* 16, 1871, *p.* 874.

90.—THE IODIDES OF AMMONIUM AND SODIUM IN SYPHILIS.

By Berkeley Hill, Esq., Surgeon to the University College Hospital.

It not unfrequently happens in cases where iodine is required, and the patient has taken iodide of potassium with good effect for a certain time, that this medicine either ceases to produce any result, or is no longer tolerated. Faute *de mieux, mercury is sometimes* resorted to; but, as the cases are not suitable for mercury, they generally become worse while taking it. In this difficulty, the iodide of sodium and ammonium are very useful, for they affect the body somewhat differently from the potassic salt. They can be borne by, and produce the effect of iodine in many persons who have become nauseated by the latter. They moreover contain less alkali per weight of the salt, and their alkali is less deteriorating to the blood than potash. One or two cases will illustrate my meaning. A gentleman was brought to me by his medical adviser, with extensive and obstinate syphilitic ulcers of the tongue and palate. He had had several other forms of syphilis during the nine or ten years he had been infected, and was in a weak and low-spirited condition. Early in the disease, he had taken mercury and other remedies in the usual way. Latterly, for the ulcers in his mouth, iodide of potassium had been given in doses gradually increasing to a scruple, and even half a drachm; but, when I was consulted, these had ceased to check the disease, and had produced iodism. In consequence of this, mercury and several other drugs had been tried again without benefit. Though his tongue was foul, his appetite, but for the pain of eating, was good. This is important; if the digestion be deranged, it must be set right again before iodine can have fair play. Under these circumstances, the patient was ordered to begin with eight-grain doses of iodide of ammonium in bitter infusion three times daily. He bore this well, and in the course of a few weeks his tongue and palate were nearly healed. For complete cicatrisation it was necessary to alter the form to iodide of sodium; and, during two years, he has been taking this salt at short intervals to prevent the relapse of his disease. Here is another case of syphilis appearing unconquerable by either mercury or iodine, which had been given by several medical men whom the patient had consulted. The case was interesting from the probability of its being one of contagion through an infected monthly nurse "sucking the breasts." The child was infected at the mouth subsequently to its mother, and the history in the mother points to contagion having entered by the nipple. I did not see the patient until two years after infection; she had then severe ulceration of the tongue and of the soft and hard palate, and limited necrosis of the bone in the palate. From the prescriptions which she showed me, she had been taking scruple doses of iodide of potassium, for some time with good effect; but, latterly, even double doses of her medicine had only produced severe iodism without controlling the disease. It was with some reluctance that she consented to try iodine again; but, after a few eight-grain doses of iodide of sodium, coupled with frequent washing of the mouth with hyposulphite of soda and glycerine lotion, her sufferings were so far removed that she announced, a week afterwards, that she could eat crusts with ease; and, in a month, except that a morsel of necrosed bone still adhered, the mouth was quite well. The course of this case since has been obstinate; gummata appearing elsewhere after that in the mouth had healed; but she still takes iodine, sometimes in one salt, sometimes in another, and occasionally mixed with Bell's liquid extract of sarsaparilla. It would be easy to multiply instances where, iodide of potassium having lost its effect, the progress towards recovery can still be maintained by another form of iodine; but that is needless.—*British Medical Journal, Dec.* 23, 1871, *p.* 725.

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

91.—ON THE PROPER MANAGEMENT OF TEDIOUS LABOURS.

By Dr. G. Hamilton, Falkirk.

I have used for upwards of thirty years only the late Dr. Ziegler's straight forceps, and until lately only one pair of these. Shortly after Dr. Ziegler invented his instrument I called upon him in Edinburgh, and he recommended me to get the forceps which were then being made by Young, of Edinburgh. These consisted of the two blades which are now so well known, and also of a shorter blade, which Dr. Ziegler thought could occasionally be advantageously substituted for one of these. This forceps is thirteen inches long, and is a most admirable instrument. Its ingenuity, simplicity, and great utility, have hardly yet, I think, been justly appreciated by the profession, arising, perhaps, a good deal, in Scotland at least, from the circumstance that the brilliant reputation of Sir James Simpson, who shortly afterwards designed, and of course recommended, an instrument of his own, overshadowed and kept in the background this, in my opinion, the best of all forms of the forceps I have seen. My original instrument, after long use, I found to have certain defects, and on that account I have now laid it aside; but although I have done so, I look upon it with wonderful respect, as a servant that has been most useful to me in many trying situations. The defects of this instrument I have found to be, that it was barely long enough in certain cases that have occurred to me, that it is not strong enough to be used as a lever in my mode of delivery or even with powerful traction, that the hinge has not sufficient "play" to allow the blades easily to lock, and that the open fenestra in blade No. 1 is apt, when much leverage is used, slightly to injure the cheek or side of the head of the child next the pubes. Mr. Young has constructed me two instruments to remove these defects.

Comparing these forceps, I would remark, 1st, that Ziegler's are longer than any of the others, and, I believe, also stronger. 2nd, That Sir J. Simpson's, which come nearest to them in length, have two projecting horns, nine and a half inches from the extremities of the blades, which must, I should think, limit their introduction to this length, whereas those wanting these horns may be easily locked even within the parts of the patient, if necessary. These appendages were added, no doubt, to give additional traction power, but this is amply secured by the great strength of those I am using. 3rd, Mine, inside the blades, measure two inches and five eighths, and the others one eighth, two eighths, and three eighths of an inch more. Now this, again, may become important in difficult cases, for, as Burns properly remarks (*Midwifery*, p. 503), "forceps whose blades could come considerably within three inches of each other may, in a particular degree of contraction, act better and require less exertion" than others. 4th, With a width of three inches between the blades the instrument may be simply a double hook or tractor, while with two and five eighth inches it may lay hold of the head, and enable us to use it and the head as a lever. 5th, Dr. Beaty well observes (*Braithwaite's Retrospect*, vol. li, p. 361), that the "elongate curve" (which is given by the lesser width between the blades of Ziegler's forceps) renders the introduction of forceps more easy. 6th, The solid blade will, I think, be *found* a great improvement, not only by preventing, as I have said, injury to the skin of the child, but also by facilitating their introduction; and, when smeared with india-rubber paste, making the large friction-surface that lays hold of the scalp really of considerable importance. In fact, the forceps when thus assisted in laying hold of the skin, become almost, as it were, an instrument combining the properties of the forceps with what was aimed at by Sir James Simpson in his air-tractor. In fine, I think my instrument, from wanting projecting parts, and thus being less liable than double-curved ones to injure the mother; from the ease with which it can be introduced and locked; from its great length, which allows us to reach all kinds of cases; and from its great strength, which enables us firmly to lay hold of, move, and mould the head, is superior to any other I have seen.

I have remarked that straight forceps can alone be used so as to gain the leverage power which I have indicated, and also that they must have their blades placed antero-posteriorly in relation to the pelvis. If we inspect the bony pelvis, I think we see the reason of this, as well as the advantages which are thus gained. The outlet of the pelvis has been described as an irregular oval space, with its long diameter placed from before backwards. I would rather incline to describe it as *two* irregular spaces, the posterior always oval and the anterior often so, running into each other at the point formed by a line drawn between the two spinous processes of the ischia. It is, I think, in the anterior space that the head in my second and third classes mostly gets fixed, and we may, I think, perceive that mere traction must often have very great difficulty, or that it may be found impossible, with this alone, to move it, unless we either turn it round or throw it to a certain extent back (sometimes conjoining the two movements) into the posterior space. This is what I do in delivering in the way I have mentioned, as may easily be shown by performing the trifling but very striking experiment of placing the forceps in the bony pelvis, and making them describe under the symphysis pubis the semicircle, or more, I have alluded to. I have been called in by brother practitioners, using, as I did, Ziegler's forceps, who have yet been unable to deliver under these circumstances after hours of traction, where I have succeeded at once by altering the position and direction of the instrument—applying leverage, and thus throwing the head back into the posterior space. My friends, I dare say, have been apt to think that I possessed a superior instrument to what they used, but the true secret of my success consisted in the different mode in which I used it.

I can now, I think, with greater profit, say a few words as to how I came to use the instrument in the way I have described. But in truth I can hardly do this in a systematic manner, for I may say it grew up rather than was invented by me; and I confess I did not at first see, as I afterwards have done, the novelty and importance of the changes implied by it. Perhaps the first thing that particularly attracted my attention was the different position in these deliveries required to be occupied, in the bed, by the attendant. Formerly (the patient lying on her left side) she used to sit, say a little higher than the middle of the bed, gently elevating the patient's right knee. Now, I required that she should sit on the pillow, on *a line with the patient's head*, and I usually found that I had to make great exertions to get her to keep the patient's right knee and leg out of my way, I myself going into the bed and following the course taken by the forceps. I found myself always crying "Up, up, still further," till the nurse sometimes lost patience, saying she could do so no more. This, of course, arose from the much greater curve which I was now making the handles of the forceps take. This, in point of fact, is so great, that I often, as has been stated, find the handles of the instrument, at the termination of delivery, almost parallel with the abdominal parietes of the patient. In November, 1861, I delivered with the forceps a lady (primipara) lying in a bed which stood in the midst of the room, and I noticed that I introduced my forceps while sitting on the right hand side of the bed, that as the delivery progressed I went round the foot of the bed, that when the delivery was completed I was standing at about the middle of the *left*-hand side of the bed, and that in the last stage of the delivery I had changed the position of my hands in grasping the handles, and was then *drawing* the instrument forwards and upwards. I did this, as I mentioned, with *Ziegler's* forceps; but had I been using such an instrument as I commenced practice with, it is manifest that the discovery of such an operation being possible never could have been made.

Let me now give a single case illustrative of these remarks, in which I used one of the new instruments made for me by Mr. Young.

Dec. 30th, 1870.—Was called about 1 p. m. to Mrs. G., who has a midwife with her, and states that she has been in labour since 7 a. m. Is of the squat figure I have referred to as having tedious or very difficult labours. States that she has had thirteen children (the first twins), of whom five were stillborn, three of these being delivered with the forceps. The head was at the brim of the pelvis, the uterus easily dilatable, and the os uteri about twice the size of a crown-piece. Pains regular, but rather feeble. On examination the pubes and promontory of the sacrum were found to project inwards, and between these and the head the uterus was caught. My first care was to get rid of this obstacle, which I succeeded in doing, first, by using two fingers in pushing up the uterus in front; and secondly, as this failed, by introducing my hand and extricating the engaged part of the uterus posteriorly. After a few endeavours of this kind the uterus slipped over the head, the latter immediately descending a little into the pelvis. The head was still, however, so high up that it was with difficulty the tip of one ear could be felt a little to the right of the symphysis pubis. The face, I thought, was to the right (which I afterwards ascertained to be the case), but of this I was not at the time quite certain. After the head had come a little further down into the pelvis I tried to apply my new forceps; and, by directing the handle of the blade No. 1 well backwards, I succeeded in placing it over the ear next the pubes. The other blade easily slipped into its place on the opposite side, and these gave me a remarkably firm hold of the head. I used simply reaction at first, assisting each pain, until the perinæum began to be pressed upon, when I turned the instrument forwards and upwards (supporting the perinæum with my left hand), until the handles had revolved round the lower part of the pubes. When the head was born the instrument remained, as when applied, nearly in the mesial line of the woman's body. One blade was over the right ear and cheek, and the other over the opposite ear and parietal bone. There was a very slight excoriation over the left parietal bone; the anterior solid blade had merely compressed the skin and made it look slightly grayish. The umbilical cord was round the neck, and the child for some time failed to breathe. By at once, however, applying my mouth to that of the child and inflating the lungs, and compressing the sides of the chest in conjunction with this (as I have recommended and described in the Edin. Med. Jour., for May, 1855), the child soon began to breathe and cry, and both it and the mother did well.

In my notes I find the following remarks on this case:—Here the length and strength of my new instrument gave me great advantage, for it must be remembered that in such labours strength is equivalent to length. I think my old instrument would have been constantly slipping until the head advanced, and hence losing most valuable time. And time here, as far as the child was concerned, was of immense importance, for, had the labour been prolonged even a very little more, the child must have been lost. From the time I saw this woman till her delivery something more than an hour elapsed. I should say that probably another half hour, and certainly another hour, in the passages would have killed the child. As it was, not an instant of time had to be lost in order to save it; and here I would remark that slapping the buttocks, dipping in cold or warm water, &c., would have been bad practice. In such extreme cases inflation, as the major remedy, should instantly be used, for then even seconds of time are precious. In delivery, the force applied was at first almost pure traction, and at last it became almost pure leverage. At first I tried to turn the head into the hollow of the sacrum; but finding this not to succeed, I let the instrument take its own course, the head passing the *os externum* with the blades nearly antero-posteriorly. Looking at it as a whole, I think we have here a key to the success which has attended my mode of practice, for it is easy to see how enormously a want of decision and knowledge must in these trying cases increase the mortality to both mother and child. Even though the ear was at the brim of the pelvis, and so high up that it could barely be touched, the simplicity of the whole operation, and the ease with which it was performed, seemed to me very remarkable. Once the anterior blade had been placed over the ear, I seemed to take almost no heed as to all the rest. The posterior blade at once took its proper place, and locked without giving me either trouble or thought (for this is the rare quality in Ziegler's forceps, that they lock of themselves); as the head advanced I did not know, and I did not care, what turn it would take, or whether it would take any, for my forceps were adapted to all contingencies; so, as it were, I almost let them take their own way, only watching the handles, which at once told me what was being done. If they should keep antero-posterior, then leverage was the power I had mostly to trust to; if they became transverse, the curve they could go through, or the lateral oscillation that could be employed, I knew was very limited, and traction then would mostly be the power available. With such simple rules as our guides, it seemed to me almost impossible for a person of the most ordinary competency to go wrong.

But if, from timidity or other cause, I had allowed the uterus to be caught at the brim for an hour or two more, and as a consequence, the head to be arrested and the parts to become swollen, and had then introduced a curved instrument within the uterus, and had set out with a conviction that it must, and a determination that it should, describe a screw-like course in delivery, how very different might have been the results to mother and child. Looking at the difficulties of such cases, so managed, I think there can be little wonder that most writers on obstetrics have expressed a salutary dread of "long-forceps" deliveries, or that some should even have preferred to them opening the head.

The double curve appears to have been first given to the forceps by Levret, so that it might fit into the shape of the sacrum, and when the face has already got into the hollow of the sacrum, a short instrument of this kind suits very well. The curve has been retained on the longer instruments used since then on different grounds—as far as I can understand, because it gives a larger surface for laying hold of the head; because the head at the brim can be better reached with it; because, as Dr. Barnes says, it fits into the natural screw-like form of the pelvis. These and other reasons I have heard given for retaining the double curve, but I have never yet seen a case in which I would have preferred this to the straight instrument. In this I think the Dublin school, where Dr. Beaty's straight forceps are mostly used, is right. Dr. Barnes says he formerly used straight forceps, but has latterly laid them aside, in consequence of finding that they injured the perinæum. On this point I can only repeat what I have already stated, that I have never had in my *whole* practice rupture of the perinæum; and that, when using the forceps, if I find any unpleasant distension of it, I take them off, and support the parts with the palm of my *right* hand (not the *left*, as recommended by some authors), pushing the head strongly forwards and somewhat upwards, as recommended by the late Professor Hamilton, or in other words "shelling it out," as this manœuvre has been appropriately named by Dr. Barnes. As far, therefore, as I can judge, I would say that the instrument I now use is fit for the management of any case I have ever met with, and certainly it can be used in many where the double-curved forceps are wholly inadmissible.

My 4th class consists of such cases as cannot be reduced to the 1st, 2nd, or 3rd classes, which may arise from a variety of causes. To some of these I shall now advert; and here I must repeat the opinion I have already expressed, that, with increased knowledge and experience, and the use of better instruments than we formerly had, we may reasonably hope the number of such will in future be considerably diminished. Let us take, for example, such a case as I have recorded in the Ed. Med. Jour., Oct., 1861, p. 320, where the ear was at the symphysis and the face to the left (my 5th position). In this case I could not get the face to rotate to the left, and was therefore obliged to push up the head, rotate the face to the right, and then deliver. If I met with such a case now I would, of course, at once endeavour to use the forceps as a lever in the manner described, and my impression is, that with my stronger instrument I should succeed. Supposing we fail, however, the principle of rotation, as I have pointed out in the article referred to, can be resorted to with great advantage, and to an extent (with straight forceps) that would astonish many practitioners. Sir James Simpson used to be much interested in this subject, and I recollect relating to him a still more interesting example of this kind. The case had been a long time under the care of an ignorant, and, what was even worse, an obstinate midwife, who persisted in the vain endeavour to make unaided nature effect the delivery. When I saw the woman I found the parts dreadfully swollen, and the case fast assuming all the characters of "impaction." With considerable exertion I found that the ear and face were to the left of the symphysis (my 4th position), and succeeded in fixing the forceps over this ear and the head, but could neither turn the face into the hollow of the sacrum nor make it advance downwards. I then, as in the last case referred to, pushed the head above the brim, and rotated it so as to place an ear and the face to the right of the symphysis (my 1st position), and still I could not deliver. I then resorted to podalic version, and by that means saved the child. The mother, however, succumbed to peritonitis in three or four days afterwards. Besides showing the extent to which rotation can easily be carried, this case is one among many others confirming the doctrine that the base of the child's head, when brought down first, will sometimes pass easier through a narrow pelvis than if the vertex had presented. With my later ideas as to the mechanism of labour, and the powers of the forceps, I would give my adhesion to this great principle with reservation; and I would say, as the result of my own experience, that podalic version should never be resorted to until the forceps (with rotation to the right, should the face be to the left) have failed.

When I am absolutely compelled to apply the forceps within the uterus, the rule I follow is still, if I can, to place them over the ears, which mostly requires that the anterior blade (No. 1) should be thrown well back on the perinæum; or, if I cannot do this, or cannot exactly ascertain where the ear lies, I then lay hold of the head wherever I can (sometimes placing blade No. 1 posteriorly before introducing No. 2), and advance it until, by examination, I find its exact position, when I then know precisely how to proceed. At the same time that the forceps draw the head through the os uteri, I find it most useful, with the fingers of the left hand, to push the uterus up over the head. In these high operations the length and strength of my present instrument give me, I think, very great advantages.

Cases with the face to the pubes, occurring in primiparæ, have given me some trouble. My 732nd case, as I have stated, was of this kind, and I lost the child, and in an earlier part of my practice I recollect the same thing happening in another instance. It was interesting to me, therefore, since I have been using my new instrument, to meet with a case exactly similar. It was under the care of a midwife, who called me to assist her. I found the head jammed in the parts very tightly, and, as I could not at first ascertain with precision the position, I placed the blades antero-posteriorly, and used as much leverage as the case would allow. In doing so I suddenly felt the handles jerk round and become transverse. Taking the instrument off and examining, I now found that an ear was slightly to the left of the symphysis, with the face to that side also. Reapplying the instrument I again used leverage, when the head made another jerk round, placing the face in the hollow of the sacrum, and delivery took place immediately. Here, again, I was much impressed with the great power my new instrument and mode of delivery gave me, and I was much pleased with the firm hold the india-rubber on the inside of the blades (which I had lately been using) seemed to give. The simplicity of the operation especially struck me, for it was nature, and not I, that took the turns. I found, on examination, that the solid blade (No. 1) had at first been placed over the left eyelid and cheek, which were a little red and swollen, but not in the least degree excoriated. This disappeared in a few days, and mother and child did excellently.

After all, of course it will occur that in some instances the disproportion between head and pelvis is so great that delivery cannot be effected with the forceps, though I am now inclined to think, if the case has been well managed, and absolute deformity of pelvis or hydrocephalus be absent, this is much rarer than is generally imagined. In such cases, if the mother has not previously been too much exhausted, podalic version naturally presents itself to us as another resource before perforation. From the line of practice which I have followed, I have had so much less experience in podalic version than most hospital accoucheurs, that I cannot venture to speak practically on the subject with great confidence. I would remark, however, that in such cases, where the head is very large and refuses after version to enter the pelvis, or where it is caught high up and does not advance after prolonged traction, it is perhaps the best practice, if the want of beat in the funis indicates the child's death, at once to use the perforator. In the case immediately preceding my series of 731 cases I did this, the child being almost a monstrosity in point of size. In another case, where I had to assist two brother practitioners, I did the same, the cervical vertebræ having given way before it was resorted to. In the first case the mother died also, from peritonitis, while, in another case still, I was called by two practitioners to assist them, and found the head alone in utero, and the mother moribund. Such examples, conjoined with the mortality to the children in cases under my own care, already referred to, have, I confess, given me a great dread of turning and high footling cases.—*British and Foreign Medico Chirurgical Review*, *Jan.* 1872, *p.* 171.

92.—ON PUERPERAL FEVER.

By Dr. Edward Martin, Professor of Medical Midwifery in the University of Berlin.

[In the year 1860, Professor Martin first put forward the theory that puerperal fever depended upon a diphtheritic process set up in the female genital organs. The views of this disease held by the profession are extremely divergent, owing to all the febrile diseases of lying-in women being included under one rubric.]

Febrile conditions may be met with in lying-in women as well as in non-pregnant women, whether as a consequence of inflammation in almost any organ (but which has no connexion with the puerperal condition) or in connexion with various contagious diseases, as scarlatina, variola, &c. There may even be febrile affections consequent upon inflammatory action in the genitals of lying-in women, but which are essentially different from puerperal fever in the alarming sense of the word. Entirely unconnected with this, lying-in women may have very severe fever from inflammation of the breasts or nipples, after contusion or laceration of the uterus or vagina, as well as consequent on abscesses or ulceration which may ensue upon effusion of blood into the connective tissue. Such fever neither in its course, symptoms, or issue, resembles the conditions which arise from the diphtheritic process; and it is this which should be regarded as the essential characteristic of puerperal fever. Even the existence of thrombosis is not as a matter of course to be attributed to puerperal fever, as in many cases this remains entirely isolated, as contrasted with the thrombo-phlebitis which accompanies or follows the diphtheritic process.

Limiting in this way the conception of puerperal fever, the question naturally arises—What are we to understand by the "diphtheritic process?" You are aware that recent investigations have thrown most important light on the nature of diphtheria affecting other organs—especially the pharynx, where it has been shown to consist of a fungous formation, the spores of which are seen under the microscope to penetrate not only into the tissues, but within the bloodvessels—producing in this way a generalised disease. In diphtheria of the genital organs investigations have as yet not been extended thus far, and it remains a question calling for farther examination. Admitting, however, that the diphtheria is here due to a fungous formation, other questions arise. Is the fungus in question specifically different?—since we are familiar with various fungi which germinate in the vagina of both pregnant and non-pregnant women without giving rise to any dangerous affections;—is the fungus the mere carrier of the contagium? or is the puerperal fever produced in consequence of the special condition of lying-in women favouring the production of certain fungi, by reason of changes taking place in the organic substances and fluids?

Leaving these considerations, we may next advert to an examination of what the microscopico-anatomical basis of puerperal fever is. In the majority of cases we find on the external genitals and the vagina a diphtheritic deposit covering those wounded spots which, in the form of larger or smaller lacerations of the mucous membrane, so frequently occur during labour. The circumference of hese spots is more or less considerably swollen. In many cases the dipththeritic deposit is thus confined to the external genitals, and the disease pursues ts course by casting off the deposit without any or with very little general disurbance. But in the majority of cases coming under medical recognition, the diphtheritis is not confined to the entrance of the vagina, but is found deep within the canal, covering the large or small lacerations of the os uteri, and within the cavity of the uterus itself. Here it occupies both the site of the placenta and the upper parietes of the organ; and it is sometimes found exclusively here, and in no places accessible to the eye.

It may be objected that in many autopsies of women dying of puerperal fever no diphtheritic deposit has been found. This is a fact which I have myself verified in several instances, in which not only have the symptoms been present, but careful examination of the patient during life has shown the presence of the deposit. In explanation of this apparent contradiction, we must not forget that the diphtheritic deposit in many cases very quickly disappears, and especially when injections or caustics have been employed, while its consequences may persist and undergo farther development. That we should not be able during life to see the diphtheritic deposit when within the uterus is conceivable enough, but the diphtheritic flocculi may be recognised in their expulsion with the returning uterine injections.

As a general rule, the diphtheritic process spreads rapidly from the genital organs, but it does so only rarely towards the skin of the thigh, nates, &c. These then exhibit an erythema (which has been well named puerperal scarlatina) or pass into ulceration. More frequently the diphtheria extends into the urethra or the rectum, if it have not already appeared there primarily; but its most common modes of spreading are either by means of the connective tissue surrounding the vagina and neck of the uterus, by the mucous membrane of the tubes to the peritoneum, or by the lymphatics and veins—these various modes of its extension being often combined with each other.

In various but frequently combined modes of extension of the diphtheritic process of the genital organs, the great glandular organs of the abdomen, the kidneys, liver, and spleen are soon implicated, so that they are usually met with in a state of parenchymatous inflammation; and finally, the lungs, especially at their lower lobes, not infrequently exhibit the turbid-serous infiltration, pleuritic exudations being also associated with the peritoneal. A more infrequent result of the diphtheritic process, because in general a longer duration of the affection is required for its production, is inflammation of the peripheric cellular tissue, which may happen in different parts of the body. This most frequently occurs in and around the joints, around the muscles of the extremities (e.g., in the pernicious form of phlegmasia dolens), or around some of the superficially placed glands, as the breast or parotid.

It is precisely this great multiplicity of local affections, and their combination with each other, that constitute the peculiar characteristic of puerperal fever. As, however, sometimes one and sometimes another of these occupies the foreground, we are furnished with the explanation of why different authors have come to regard these different local affections, whether peritonitis, phlebitis, lymphangiotitis, phlegmon, &c., as the essential feature of puerperal fever.

Although, in regard to our knowledge of the etiology of puerperal fever, decided progress has been made in recent times, yet many points remain obscure. Thus, in relation to the admission that the disease is autochthonous—i.e., that it may arise from the spontaneous decomposition of retained portions of the placenta—we must not overlook the fact, that remains of the placenta or membranes are not unfrequently retained for days, weeks, or months within the genitals without any putrid decomposition taking place, or any symptoms of puerperal fever appearing, while their presence often gives rise to repeated attacks of hemorrhage. If, then, in numerous other cases the retention of such remains is followed by septic decomposition and puerperal fever, it is evident that some other circumstance has to be sought for which has determined this unfortunate occurrence. From the known influence of the air in exciting putrefaction in fermentable bodies, it results that decomposition of the retained remains of the placenta would be especially expected when these protruded from the os uteri into the vagina, while they would be more protected from the influence of the air when enclosed within the cavity of the uterus. And, in fact, in this last case putrefaction does much more rarely occur; but it must not be overlooked that the remains of the placenta are then more intimately united with the wall of the uterus. However, there are plenty of examples of the occurrence of puerperal fever, notwithstanding complete expulsion of the placenta; and in such cases we must seek for other causes. Numerous cases have proved to me that women who are delivered while the subjects of recent gonorrhœa frequently become affected with puerperal fever, the diphtheritic process being immediately set up, and proving difficult of arrest. I must therefore admit that a preceding inflammatory condition of the mucous membrane of the genital organs stands in a certain relation to the occurrence of the diphtheritis. In the great majority of cases, however, the germ of puerperal fever gains access in other manners; and this is very positively shown by the well-known fact (confirmed by the numerous figures of the Vienna Lying-in Hospital, as also by the results observed in my own clinic, that the so-called street-births (*Gassengeburten*) are scarcely ever followed by puerperal fever. The transport of the diphtheritic germs takes place beyond all doubt very frequently during labour, more rarely after delivery, and sometimes shortly prior to parturition. In what the transported germ consists is less made out. Experience has taught us that cadaveric products and decomposed animal substances place puerperal women in danger, especially when an internal examination is made by fingers that have had to do with dead bodies without having been afterwards cleansed—although practitioners may also convey the disease who have observed care in washing. The dead bodies in question have not always been those of the subjects of puerperal fever, although these entail a greater degree of danger. Again, certain secretions from suppurating wounds and ulcers conveyed to the genitals of a puerperal woman may give rise to diphtheritis. The epidemic prevalence of puerperal fever in Berlin during the winter of 1870–71 may with strong probability be attributed to the employment of so many of the civil practitioners in the military hospitals. Still more decidedly are diphtheritic products—which not infrequently are produced in scarlatina, typhus, cholera, suppurating cancer, &c.—dangerous to lying-in women. The most usual mode of propagating the diphtheritic poison from the sick to the healthy is its direct conveyance by means of sponges, dirty towels, catheters, clyster-pipes, or the fingers of the accoucheurs; and in this way epidemics of puerperal fever are brought about most frequently in hospitals, although they are also met with in private practice. What relation this origin bears to an incubation stage is uncertain; for although Veit has observed this to vary between twenty-seven and forty-eight hours, the number of cases adduced are as yet too few to allow of any general statement being made.

Diphtheritis of the genitals is not only met with in puerperal women, although they—on account of the denudation of the mucous membrane of its epithelium, and the numerous lacerations of tissue, as well as the ready decomposition of the lochial secretion—exhibit a special predisposition for contracting the disease, while the dilated vessels present a favourable condition for generalising the affection. Paul Dubois, forty years since, observed that the pupils at the Maternité, who, while menstruating, tended sick puerperal women, also became the subjects of an affection resembling puerperal fever. In Germany similar observations have been published, showing that, under certain favouring circumstances, a similar diseased process may be set up in non-pregnant women. I remember the case of a women 52 years of age, who was admitted into the gynæcological clinic of the Berlin Charité on account of repeated hemorrhage. This arose from a large crumbling myoma, for the removal of which I used a forceps which probably had not been properly cleansed after a former employment. The woman died of diphtheritis of the internal genitals fifteen days after the operation. The autopsy disclosed the same lesions as are found in women who have died of puerperal fever—viz., diphtheritic deposit upon the wounded surface whence the tumour had been removed, lymphatic vessels filled with pus, and peritoneal exudation. It would seem to result, from other cases, that this diphtheritic process of the genital organs in non-pregnant women is but rarely followed by dangerous general disease.

With regard to the prognosis of puerperal fever in general, if we except the ases in which the diphtheritis remains localised, it is upon the whole unfavourble; for we must admit that one-third of the cases in which fever has ensued pon diphtheritis of the genitals terminate fatally. Death takes place most requently up to the fifth day, and then up to the eleventh day. In some cases he disease may last even for months.

I have only a few words to say concerning treatment. The prophylaxis lays laim to our most earnest attention, and the etiology of the disease indicates nany important points for the exercise of this. The extremest cleanliness of ll having to render service to the lying-in woman, both as regards their person nd their clothes—especially their fingers and sleeves—and cleanliness in egard to all clothing, catheters, sponges, enema-pipes, &c., must be most tringently insisted upon. It is very much to be desired that all the utensils f labour should be new for each woman, and the same elastic catheters should ever be employed for several lying-in women. As mere washing the hands vhich have become contaminated with infectious matter does not seem to afford ufficient security for internal exploration, I think it best under such circumtances to rest satisfied with external exploration. Especially does this rule pply to lying-in hospitals when cases of diphtheritis have appeared; and my wn experience on this point entirely confirms the propriety of the advice given y Halbertsma and Litzmann. How necessary, then, is that complete practice f external exploration which I have taught since I first held the Professor's hair, speaks for itself. Lastly, in regard to the curative treatment. It must be re-eminently symptomatic, and, as long as the temperature continues high, efore all things the fever should be diminished. Internally, digitalis with itre or acids, and externally tepid or cold applications, contribute to this end, fter due evacuation of the bowels has been secured. I cannot speak so well f quinine as do many authors. Local treatment has during the last ten years ightly been much tried. Cleansing out the vagina by syringing and injections f tar, or creosote-water, with carbolic acid, chlorine, or solution of nitrate of ilver, has without doubt proved of great utility, even although it has not often appened that the process has been cut short by their agency. Injecting the ame substances, suitably warmed, through a large catheter, *à double courant* nto the cavity of the uterus has sometimes been followed by a considerable iminution of temperature, as well as cleansing out the uterine cavity; but a ecided general improvement has been by no means of such frequent occurence as might have been hoped.—*Medical Times and Gazette, Nov.* 11, 1871, . 583.

98.—CARBOLIC ACID AS AN ANTI-ZYMOTIC IN PUERPERAL DISEASES.

By Dr. J. D. Trask, Astoria, United States.

The poisonous effects of carbolic acid when given internally appear to have revented the proper trial of it for the purpose of arresting the progress of lood contamination. In this case no ill effects of any kind exhibited themelves.]

On the morning of the 25th she was seized with a rigor, which was repreented as of very great severity, followed by intensely high febrile action, alterating through the day with oft-recurring chills of short duration. I was preented from seeing her until four o'clock in the afternoon. At that time there were great heat, profuse sweating, pulse 160, extreme restlessness, and increased enderness over the region of the uterus, and in the right iliac fossa. There ad been urgent diarrhœa, which yet continued, the dejections being involunary and of intolerable odour. The lochia were suppressed. There was not nly no nausea, but a craving for food. Taking into consideration her experince before labour set in, I could not but feel that there was a positive septic ondition of the system in connection with symptoms of peritonitis, and it ocurred to me to attempt to impregnate the system as far as possible with carboic acid. Its administration by the mouth, rectum, and vagina was at once ommenced. A half-drop of Calvert's solution was given in mucilage by the mouth every two hours. The solution for the rectum was one drop increased to five drops to an ounce of mucilage, a half-gill being thrown up after every dejection. The vaginal injection was not less than five drops to the ounce every three or four hours; the vagina, in my experience, being more tolerant than the rectum of the first impression of the acid. There was at the time a supposed idiosyncrasy forbidding the use of opium.

Within twenty-four hours the diarrhœa ceased. The pulse was reduced to 120. There was still exquisite tenderness over the uterus, and great thirst. The inflammatory symptoms were henceforth in the ascendant, and the signs of septicæmia, although recognised in different degrees of distinctness, were in abeyance. The system seemed to be saturated with carbolic acid. She tasted it in every thing, inhaled it, as she thought, in every breath (I cannot say that she *exhaled* it), and was so disgusted that she refused to have it longer brought near the bed for any purpose. No other antiseptic was substituted by the mouth, but the bisulphate of lime or the permanganate of potash was employed abundantly in vaginal injections until convalescence began.

By the 29th tenderness had almost disappeared, but the skin was still hot and dry; there was delirium at night; the urine scanty; still great craving for food; involution of the uterus apparently not begun.

June 1. Her aspect was much worse. There was dorsal decubitus with knees drawn up, area of tenderness increased, irritability of the bladder and rectum, some diarrhœa, and that offensive, and a great deal of tympanitis. There was urgent desire for food, and constant tendency to reject it in small quantities from the mouth. The pulse was 140; respiration, 40; temperature, 106.5°. A large blister was applied to the abdomen, and its beneficial effects in reducing tenderness were very marked: the pulse fell to 120. It became necessary to withdraw the urine by the catheter.

June 2. Countenance in repose haggard and anxious; respiration, 40; the pulse 128, a mere thread; surface covered with cool moisture from head to foot. The process of involution had made no perceptible advance; the lochia consisted chiefly of a light-coloured, sanious mucus; the tongue red and tremulous; the urine now copious.

Dr. B. Fordyce Barker saw her on this day, and expressed the conviction that within three or four days she would sink. This was based upon a consideration of the state of the pulse, and the evidences of metritis, peritonitis, and septicæmia, in the light of cases of a similar character which he had seen.

She was at this time taking beef-tea and milk-punch freely. It was found on trial of the aqueous tincture of opium with bromide of potassium, and afterwards of opium alone, that she could take it advantageously for relief of the irritation of the bladder and occasional diarrhœa. The urine examined at this date was found to abound in granular casts, and to be highly albuminous. She could not be convinced that she was seriously sick, much less that she might not recover, but was determined at any rate to get well.

By the 5th the urine deposited blood corpuscles in large quantities, but there was much less albumen. The uterus was sensibly diminished in size.

June 7. For three mornings there has been a slight rigour, followed by a degree of reaction; the lochia profuse, and very offensive when antiseptics are intermitted; is constantly hungry, but can take only fluid nourishment, and in small quantities; pulse, 120; respiration, 20; temperature, 99°.

June 8. Increased fever; pulse, 160; urine of the 7th abounds in pus and ephithelium, with a few casts.

June 12. Surface cool to touch and moist; temperature, 101·5°; involution sensibly progressing; tympanitis continues.

June 18. Tenderness only on firm pressure; uterus not to be felt above the pubes.

June 27. Cystitis renewed with great severity; the exciting cause was evidently the misconduct, for two or three days, of the wet-nurse. Mental excitement always produces irritability of her bladder in health.

July 7. She got an attack of pleuritis with effusion.

July 9. For four days there had been headache and absolute loathing of food. She woke on the 9th, crying out for something to eat, and found that her menstrual flow had come on.

July 21. The suffering from cystitis has been protracted and severe; the urine ost of the time being very turbid with pus and mucus, and constantly alkane. She is at present able to sit up for a half-hour, and but for the cystitis her nvalescence would have been much more advanced. To-day I find the uterus uch enlarged, anteverted, and fixed in its position by lymph thrown out anteorly, posteriorly, and laterally, during the progress of peritonitis in the pelvis. he roof of the pelvis is a plane of induration, with the uterus implanted in e centre. In a recumbent position she suffers from uterine tenesmus, and om a sense of falling of the viscera of the pelvis in standing up.

August 7. Enjoyed a ride in a carriage.

In this meagre abstract of the notes of this case it will be seen that pelvic eritonitis probably existed to a certain degree before labour; after being conned to the pelvis for several days it invaded additional portions of the peritoeum through a considerable portion of the abdomen. But, apart from the ymptoms due to peritonitis, there was throughout the case another class that ould be attributed only to blood-poisoning. The profuse and highly-offensive iarrhœa, which occurred unconsciously to the patient, accompanying the violent igours, cannot well be explained on other grounds than the introduction into e system of a septic agent, which produced this great disturbing influence. ater in the case, when the peritonitis had subsided, the influence of this subtle gent could be recognised in the slight rigour and exacerbation of almost daily ccurrence, and in a very peculiar odour of the cutaneous secretions, which was o persistent as to attach itself to the fingers for hours.

If any consider the symptoms related as evidences of nothing more than puereral peritonitis, I refer to the fact that my opinion was confirmed by Dr. arker, whose opportunities for studying puerperal diseases have been rarely exelled by those of any observer.

Now, if it be asked of what use was the carbolic acid if the septicæmia conued in *any* degree after its employment ceased, I would suggest that it is not nreasonable to assume that the first charge of poison which the blood received as neutralized by the carbolic acid, and that by the persistent subsequent emloyment of antiseptic injections any further impregnation of the system in any nsiderable degree was prevented. During the twenty-four hours of its admistration six drops were taken into the stomach and at least sixty drops ought in contact with the vaginal and rectal mucous membranes; doubtless inconsiderable portion of the latter was absorbed into the circulation.

The readiness with which carbolic acid may be taken up by mucous memanes was first suggested to me by the fact that a patient, to whose endomeium it had been applied, spoke almost immediately afterwards of tasting the rbolic acid in the mouth. In using it in the present instance as an injection to the rectum, apart from its antiseptic influence, I had much confidence in s ability to moderate the diarrhœa by its sedative influence. The anæsthetic fluence of a weak solution in burns is now pretty well known to the profeson. Its value for allaying rectal tenesmus is not, I apprehend, so generally ppreciated. In irritations about the anus, combined with glycerine and a ineral astringent, it is invaluable, and in dysentery I have found it capable of fording great relief when dissolved in mucilage. The susceptibility to its use aries in different persons. When thrown into the rectum, I generally comence with a solution of one drop of Calvert's solution to the ounce, increasing e strength if necessary, until a feeling of decided warmth is produced by its lministration, and this is followed by local anæsthesia.—*New York Medical ournal, Oct.* 1871, *p.* 357.

4.—REMARKS ON UTERINE FIBROID TUMOURS AND POLYPI; THEIR PATHOLOGY AND REMOVAL.

By Dr. Thomas Skinner, Physician to the Liverpool Lying-in Hospital and Ladies' Charity.

There is no doubt that the more immediate the removal of a pediculated fiboid, or fibrous, or any kind of pediculated tumour within the uterus, the better; onsequently, those who have had the largest experience in the removal of such tumours never resort to the ligature. The modes of operating may be briefly stated as follows :]

1. *Preliminary Steps.*—In order to facilitate the removal, the first step to be taken, is to open up the passages for the introduction of the necessary instruments, and to obtain greater certainty as to the position, relations, and attachment of the tumour and its pedicle. This, we are all aware, is best obtained by the introduction of sea tangle and sponge tents, and, if necessary, by incising the cervix uteri.

2. *Immediate Steps.*—As a rule, the fixing of the tumour by means of a volsella, or by means of Dr. McLintock's corkscrew, which, though common-place, is a most efficient instrument, and generally easy of application; transfixing with a cord, or placing a noose over it is next required. As regards volsellæ, there is no instrument in surgery worse made; simply because they are almost always made too sharp in the prongs. The prongs are generally made round, small and sharp—whereas, they ought to be square-shaped, tapering and sharp, combined with strength in all their bearings. As a rule, they are too long also between the joint and the prongs. The small and sharp-pronged volsellæ do not keep their grip; as soon as traction is made use of, they tear their way out; while the square-shaped ones keep their hold like bull dogs. The tumour, being firmly grasped, is pulled as low as it will admit of, and held there, whilst an ecraseur is passed over it, which may be accomplished in various ways. A noose may be made with the wire or chain and passed over the handles of the volsella, and ultimately over the tumour up to the pedicle, which latter may then and there be severed. Sometimes, the pedicle is so thick and tough that the division is not so easy as *à priori* we might be disposed to imagine; as in the removal of one specimen where the pedicle was two inches in diameter, and fibrous throughout. Such being the case, our next best step is to divide the pedicle immediately above the wire or chain of the ecraseur, with blunt-pointed curved scissors; or with a blunt-pointed curved bistoury, protected with lint where the cutting surface is not required; or, best of all, with the ingenious polyptome of the late Sir James Simpson. Two tumours were severed from their attachment to the uterus in a few seconds by means of this instrument—an instrument positively harmless to the surrounding parts. The best form of intra-uterine scissors for the purpose are the invention of my esteemed colleague, Dr. Grimsdale, of whose manipulative dexterity and surgical skill in the removal of such tumours it would be impossible to speak too highly.

Some of these pediculated tumours admit of being removed by traction with the fingers. I may instance one case of a patient of Dr. Swinden's, of Wavertree: a lady who had flooded more or less for three weeks after her confinement, and who was *in articulo mortis*, when I saw her with Dr. Swinden. A pediculated fibroid, the size of a pigeon's egg, was attached to the fundus uteri. By gradually dilating the parts, I got my hand in the vagina, and my fore and middle fingers into the uterus. By passing a finger on either side of the tumour, and finding that the pedicle was small and yielding, I pulled it off with a twisting motion. All hemorrhage ceased; and the patient, from the last stage of anæmia, made a perfect recovery. In another case of a similar kind, which I lately saw with Dr. Swinden and Dr. Le Gross, of Wavertree, I removed a small fibrous or warty growth, along with a few mucous polypi, after the passage of sponge tents, by means of the nail of my right middle finger. In this instance the patient was quite as anæmic as the last. All hemorrhage ceased with the removal of these trifling offenders. As an aid in the removal of such polypi, and of rugous and hemorrhagic conditions of the lining membrane of the uterus, the uterine scrapers of Recamier, Simpson, and Locock are most useful.

There is still one other class of uterine tumours admitting of immediated removal, though not pediculated. They have no name other than fibroid polypi; they are really intra-mural tumours of the sessile or submucous variety, but they are small, almost always single, generally limited to the cervix-uteri, and not infrequent in their occurrence. They are generally of an ovoid, almond, or walnut shape, and one extremity generally projects into the cavity of the cervix or presents at the os uteri. I have seen several of this variety, and I have assisted the late Sir James Simpson in their removal. I have already alluded to them, and their mode of removal, in the first or pathological part of this article. I shall only repeat that, if *come-at-able*, they are perfectly safe to remove by Simpson's polyptome, and torsion, or by crushing with a pair of forceps I got made for that purpose and for simplifying and shortening the operation of craniotomy. It would appear that the life is easily *crushed out* of these growths; the curious part of it being, that, although they themselves are easily destroyed, it is sometimes very difficult to get at them to kill them, without endangering the life of the patient. This leads me to allude to another method of removing this growth; and I commence doing so by asking a question of myself. Is it wise to partially remove a fibroid polypus? Undoubtedly it is; and the more we can remove the less there will be, as a rule, of hemorrhage thereafter. I remember a case, in which Dr. Grimsdale assisted me, at Waterloo. We broke two strong wires, and at last succeeded in passing a strong chain ecraseur over all that was *come-at-able*—about one half of the substance of the tumour. We removed this mass; and the patient, who, for ten or twelve years before had flooded at each menstrual period, never more lost a drop of blood. Previous to the operation, a tumour of the size of a cricket ball was felt in the hypogastric region. On examination of the patient, about eight or nine months afterwards, there was no trace of any tumour whatever; nothing ever came away at all resembling a solid substance. This case was the first to lead me to the conclusion that it is sound practice, in severe hemorrhagic cases, to remove as much of the tumour as is possible; and, if it cannot be cut with the polyptome or by the ecraseur, it may be crushed out of existence, or by a combination of crushing and cutting. As regards the justice and safety of this line of treatment, I know that it was practised by Sir James Simpson, by Mr. Baker Brown, in his gouging process, that it has been practised by McLintock, of Dublin, is patronized to a certain extent by Dr. Matthews Duncan, and that it has forced itself upon various practitioners, as recorded in our journals. One of the most instructive examples will be found in the transactions of the Obstetrical Society of London, vol. x., where Dr. Hall Davis removed part of a large intra-uterine fibroid tumour, 6¼ ounces, then excised in pieces 3 ounces, and ultimately a mass, which was thrown off entire, weighing 8¼ ounces; in all, the tumour weighed 18 ounces. The lady made a perfect recovery. Such a case is very encouraging; but, as our American cousins would say, "the conservative drag after all is a valuable institution."

Regarding one tumour, which weighed half-a-pound, I must say a word or two. The late owner of it consulted me on account of a draining menorrhagia three years ago. As the complaint resisted the usual and best remedial measures, I suspected organic mischief. On examination I detected a small intra-uterine polypus. I told the lady there was nothing for it but a surgical operation. I saw no more of her for three years, two of which she had spent under the care of a homœopathic physician in London, who told her that a surgical operation was quite unnecessary, and that he would cause the tumour to be *absorbed*. Of course she believed this, even although the monthly and fortnightly flooding went on the same. After being two years treated thus, circumstances forced her back to Liverpool, and she received advice, first from one and then from another homœopath, for another year, but all to no purpose. As the patient's life was ebbing, and all but *in articulo mortis*, she and her friends begged of me to take her case in hand. I took counsel with my friend Dr. Grimsdale, and we determined at once on removal of the tumour, which was executed in about twenty minutes, by means of Braxton Hicks's ecraseur, and Simpson's polyptome. From the time of the operation until now, nearly nine months, she has never lost one drop of blood, or ever "looked over her shoulder." The result of three years of homœopathic absorption by means of specifics was, that the tumour *grew* from about half-an-ounce to eight ounces.

In conclusion, I shall enumerate the necessary *armamentaria* in the removal of fibroid tumours and polypi. The uterine sound or probe is most useful in diagnosis, and for measuring the probable size and even weight of the tumour. From the length of the cavity, one can frequently guess correctly the weight of the tumour. The wire ecraseur of Dr. Braxton Hicks, and a chain one also; Simpson's volsellæ, large and small, and his polyptome; Grimsdale's intra-uterine scissors; strong, blunt-pointed curved scissors, for incising the lips of the cervix uteri, if necessary; a small uterine syringe, for hæmostatic injections; one or more uterine scrapers; my own polypus crusher, and a small and narrow pair of midwifery forceps, with sponge and laminaria tents, and their guides or introducers; wire and other ligaturing material; sponges, and an ordinary pocket case of instruments, may all be required in the removal of a single fibroid tumour or polypus, anywhere within the os uteri. In some few instances, additional instruments are required; those mentioned, however, will generally meet every requirement, especially where the operator is up to his work. I have not mentioned the vaginal speculum, because in tumours of the kind treated of, it is rarely of any use.—*Liverpool Medical and Surgical Reports*, *Oct.*, 1871, *p.* 1.

95.—ON THE TREATMENT OF FIBROID TUMOURS OF THE UTERUS.

By Dr. ALFRED MEADOWS, Physician-Accoucheur to and Lecturer on Midwifery and the Diseases of Women and Children at St. Mary's Hospital, &c.

It is generally admitted, I believe, that morbid growths partake more or less of the character of the tissues in which they are developed. Hence bones develop bony tumours; fat, fatty tumours; nerve, neuromata; cartilage, enchondromata, and so forth. Those which we are considering are no exceptions. Examined microscopically, fibroid tumours of the uterus present almost the same histological characters as the uterus itself. There may be slight variations in the relative proportions of the several elements, as compared with the proper tissue of the uterus, and different tumours will present different proportions of these; but in the main, there are the same structures, viz., smooth or unstriped muscular fibres, bound together with varying quantities of connective tissue; sometimes one, and sometimes another, of these elements will predominate.

Now, it is thought by some writers that, such being the composition of these tumours, there is nothing remarkable in their gradual absorption and disappearance, any more than there is in the diminution of the uterus after delivery, as in the ordinary process of involution. It is implied, in fact, that the molecular changes are the same in both cases, and surprise is expressed that any one should entertain a doubt on the subject. The fact remains, however, that the great majority of observers do entertain very serious doubts on this question; or perhaps I ought rather to say that they have no doubt at all about it, but are decidedly of opinion that such changes, if they ever happen, are of extremely rare occurrence. I have myself seen many scores of these cases, but I have never yet met with one in which I was able to satisfy myself that any appreciable diminution of the tumour was effected either naturally or as a result of medical treatment. It seems to me, moreover, that the reputed explanation, if it have any foundation in fact, should be capable of almost constant demonstration; that, in short, the cure of fibroid tumours of the uterus ought, if this hypothesis be correct, to be the rule rather than the exception; for we all know that we have almost complete control over the process of involution; and that, if it be delayed after delivery, we can easily stimulate it into action. Why, then, can we not secure this result in the case of these morbid growths?

In regard to pain, these tumours are not, as a general rule, what would be called painful, unless their size be such as to cause pressure on neighbouring parts, or unless they be so placed as to project prominently from the peritoneal surface. A patient may have a very large tumour growing into the uterine cavity with little or no pain, but a tumour even of small size growing from the peritoneal surface will often occasion very great suffering. I have generally noticed that pain and hemorrhage are in inverse proportion the one to the other; and just as, if there be much pain, the tumour will most likely prove to be subperitoneal, so, if hemorrhage be the prominent feature, the tumour will in all probability be either interstitial—that is, in the wall of the uterus, or, as more often happens, it is submucous, that is, growing chiefly into the uterine cavity. Now, we have not yet arrived at such perfection in therapeutics as to be able to soothe the pain of particular nerves or sets of nerves by the administration of a particular drug; we can only act upon any given set of nerves through the general nervous system, spinal or sympathetic, or by topical application. In the absence of this special knowledge, it seems desirable, and in my experience has been fairly successful, to apply the anodyne as nearly as we can to the seat of pain. Hence the employment of medicated vaginal pessaries; but, inasmuch as it is no part of the function of the vaginal mucous membrane to digest fats, and as fats without digestion cannot be absorbed, and are apt, moreover, to hinder the absorption of other substances, it is desirable, I think, that we should not use greasy substances of any kind. For this reason I long ago gave up the employment of cocoa butter, and I now invariably use, as the basis of the pessary, gelatine and glycerine in the proportion of one part of the former to four of the latter; into this we can put any ingredient we wish, and I know no better anodynes than atropine, morphine, or conia. Other agents of this class may of course be used; and if it be preferred, they may be given either subcutaneously, or by the mouth, or by the rectum. It has seemed to me that, when given *per vaginam*, they are more effective, and certainly do not produce so much constitutional disturbance as when given in the other ways. For the treatment of what I may call mechanical pain, of course mechanical remedies will also be required. I allude chiefly or entirely to external support, for I cannot imagine that any one would advocate the use of instruments internally in the treatment of these cases.

Next, as regards the hemorrhage, the symptoms which, of all others, we shall probably be called upon most frequently to treat. Here there is certainly no one remedy which is applicable to all cases; perhaps I might almost say that here is no remedy which is applicable to any case, for in truth the treatment of this symptom is, as a general rule, most unsatisfactory and disappointing. There is, however, a very general consensus of opinion in favour of ergot, and certainly know no drug which excels its hemostatic properties in these cases. believe that it is of most value where the tumour is more interstitial than submucous; it fails, therefore, not unfrequently in the very cases where it is most needed, for it is in the submucous varieties that we get the greatest amount of hemorrhage. The cause of this failure, or rather of this partial success, is apparent when we consider that the vessels are larger and more numerous in he substance of the uterus than in its mucous surface; and further, the special action of the drug is greater in the substance of the uterus than in its mucous ining; in the one case, it can control blood-supply by diminishing the calibre of large vessels; in the other, the tissue to be acted upon, viz., the capillaries n the mucous membrane, lie, as it were, outside the range of its action.

In the great majority of cases, however, hemorrhage, especially when it occurs to any extent, is not merely of mucous origin; still less is it solely interstitial, but it comes from both sources, and hence it is that a combination of ergot with any purely astringent hemostatic answer better than either singly. Moreover, in consequence of the great losses sustained, and the necessary impoverishment of the blood which remains, anæmia more or less marked is sure to follow: chalybeates, therefore, in some form or other seem plainly indicated. Hence the rule with me, in the great majority of cases, is to administer ergot with the peracetate of iron. I find this combination the most generally successful; but ron alum, or any other astringent preparation of iron, will probably answer as well. I need not particularise other forms of hemostatics, for their name is egion; but I very much question if their value be at all proportionate to their number or variety. In many cases, none of them will suffice; all are equally neffectual. Then it may be necessary to resort to topical applications, and I know none which is so effective as the small solid stick of anhydrous sulphate of zinc (No. 5 size), first introduced into practice by Dr. Braxton Hicks. I greatly prefer this to the use of fluid injections into the uterus, as I believe it to be fully as useful, and far less dangerous. Through the speculum it may, without any difficulty, be passed quite up into the uterine cavity. The plan recommended by Mr. Baker Brown of freely incising the cervix for the purpose of curing the hemorrhage, I have on several occasions tried, but I cannot say that I have ever seen any good result from it.—*Brit. Med. Jour.*, *Dec.* 2, 1871, *p.* 636.

96.—MULTILOCULAR OVARIAN CYST NEVER TAPPED: OVARIOTOMY; RECOVERY.

By Dr. John C. Gooding, Cheltenham.

[The patient was sixty years of age, and unmarried. She had always had excellent health. The symptoms of the disease dated back eight years. Just before the operation was performed the abdomen was entirely filled by a fluctuating tumour, extending above the ensiform cartilage; the uterus was mobile, and in its normal condition; the tumour could not be felt by the vagina.]

June 15th, 1871. The patient is in excellent health, and was out even up to yesterday making little purchases in preparation for her seclusion. She is hopeful, but resigned. The bowels acted during the night, and the bladder was emptied before the operation. Beef-tea was given three hours, and brandy-and-water immediately before the administration of the chloroform, which was kindly undertaken by Mr. C. J. Bennett. The patient, warmly clad, was conveniently placed on a table in a room the temperature of which was 70° F. An incision, three inches long, was carried midway between the umbilicus and symphysis pubis, through the skin, fasciæ, and rectus, down to the peritoneum, which was then divided on a director. The hand, introduced between the tumour and abdominal walls, found no adhesions, not even in the left iliac region, where I expected there might have been some resulting from the recent inflammatory attack. The cyst was punctured, and withdrawn until it resisted gentle traction. To introduce the hand to determine the detaining cause it was necessary to extend the incision by an inch, as the partially extruded cyst occupied a portion of the wound. I found, high up in the epigastrium another cyst, the contents of which were evacuated through the first, and then the whole growth was easily withdrawn. The pedicle, very short and thin and two inches broad, was transfixed through a translucent part and tied in two portions with stout silk, severed, and replaced in the pelvis. The other ovary was healthy. The insignificant oozing from the divided rectus was sponged away; no fluid having escaped into the abdomen (thanks to the efficient assistance of Messrs. J. Humphreys and C. J. Newton). The wound was closed by five silver sutures traversing the peritoneum at least half an inch from its cut edges, and two or three superficial wire sutures and broad strips of plaster. A large pad of cotton wool, retained by other strips of plaster, filled and supported the concave abdomen and completed the dressing. The patient was replaced in bed; pulse 85, good.

The tumour was made up of two large cysts, which between them held twenty-five pints of thick dark fluid; a third cyst, about the size of an orange, containing glairy white fluid; and numerous small cysts, embedded in and projecting from the interior of the walls of the larger cysts, and crowding round the pedicle. When the cysts were all emptied, the solid portion weighed 2 lb. 10 oz.

Four hours after the operation the patient was comfortable; had felt slight nausea, which was completely allayed by ice; the pulse was 96; skin warm and perspiring. Eight ounces of urine drawn off. One tablespoonful of cold milk was given, and ice only ordered for the next few hours. At midnight—eight hours after the operation—I found that the patient had slept soundly for an hour; her aspect was good; pulse 90; skin warm and moist. Six ounces of urine drawn off. On the following day I found that the patient had taken thirty minims of nepenthe to relieve slight pain occasioned by the vermicular action of the intestines; she had slept well, and was comfortable. A teacupful of milk, and half that quantity of beef tea, had been taken during the past twenty-four hours. On the third day flatus passed, the patient continuing her favourable progress. On the seventh day all the sutures were removed, as the wound was found to have healed throughout its entire length; but at the upper angle, from the want of another superficial suture, one lip was more elevated than the other, exposing half an inch of raw surface. A piece of lint soaked in carbolic acid lotion, and long strips of plaster, embracthe hips, were applied; the wool and plaster as before to support the abdo- n, which continued undistended. The use of the catheter was continued up he tenth day, from the patient's inability voluntarily to evacuate the blad- . The bowels were relieved by castor oil on the ninth day. On the twelfth a tonic mixture was ordered, to stimulate her appetite; on the fourteenth was allowed to sit up in bed; and on the twenty-first, the wound having some days past completely healed, she took an airing in a wheel chair. A days after she went into the country; and on her return recently came to me, and was looking exceedingly well.

o tie and return the pedicle certainly seems the next best mode of dealing h it, when, owing to its shortness, the clamp cannot be used. The portion the pedicle on the distal side of the ligature—the stump,—surrounded as it by *warm* tissues, no doubt retains its vitality long enough for it to become ached by lymph—rapidly effused and organised as we know this to be—to acent parts. And in the same way that a completely detached portion of if quickly readjusted, *and its warmth be maintained*, will soon become part the body again, so does the stump become vivified by blood conveyed to it ough the newly formed vessels rapidly developed in the effused lymph. The ature, when tightened, buries itself too, and brings into apposition the peri- eal covering of the pedicle on either side of it, and between these adhesion bably soon takes place. The *material* of the ligature would scarcely seem, oretically, to affect the result, for by the complete closure of the abdominal und air is excluded and decomposition prevented; there being then no trefying fluid for the ligature to absorb, hemp and silk would be on an ality with metal. The results of cases by those who have had many oppor- ities of treating the pedicle as in this case seem, practically, to favour this w.—*Lancet, Dec.* 2, 1871, *p.* 776.

.—A SUCCESSFUL METHOD OF TREATING CERTAIN CASES OF DYSMENORRHŒA AND STERILITY.

By Dr. J. Protheroe Smith, Senior Physician to the Hospital for Women, London.

he dependence of dysmenorrhœa upon a mechanical constriction of the cer- al canal of the uterus was first pointed out by Dr. Mackintosh, of Edin- rgh, in 1844. In 1847, Sir James Simpson described his operation of incis- the cervix.]

The instrument used by Sir James was a kind of *lithotome cache*, by which divided the strictured part, the incision commencing at the union of the vix with the body of the uterus, and passing more and more into the sub- nce of the cervix as the instrument was withdrawn, and so dividing its er edge.

n the year following, I had a double hysterotome constructed in Paris, with blades cutting laterally; and Dr. Greenhalgh subsequently contrived a ingenious instrument of this description, by which he proposed to gauge rately the extent of the incisions on both sides. Dr. Black, however, rves that—"In consequence of the gratifying results sometimes produced cision of the cervix uteri in obstructive dysmenorrhœa and sterility, that ation became an extremely favourite one with Simpson. Certain risks con- ed with it, however, and, in particular, its liability to be followed by pelvic mmation, inclined him ultimately to a rigorous selection of cases, and to enjoining of recumbency for several days after the performance of the ation."

imulated by Sir J. Simpson's favourable opinion, for some time I followed mode of treatment; but the results, as regards the removal of dysmenor- a and sterility, were not such as to convince me of the advantage of its eral adoption. On the contrary, in several instances, severe metrocellulitis arred, together with considerable hemorrhage, requiring the plug to control In others, the painful menstruation was unrelieved, and the patient ained sterile. My objection, however, to the use of the hysterotome was confirmed by observing that, in several instances, it had aggravated the evil; for not only did the patient continue barren, but the original constriction was increased, and, with this straitened state of the uterine canal, the consequent distress was augmented. The pathological condition I discovered by the use of sea-tangle tents. The first case which yielded this information to me was one in which Sir J. Simpson had performed the operation some years previously. For a time, after recovery from its immediate effects, relief from the distressing symptoms was experienced by the patient; but after a short time the dysmenorrhœal sufferings returned, and, as they gradually increased in severity, she was led to consult me. Finding that the uterine sound could be passed only with extreme difficulty through the os internum uteri, I introtroduced a small sea-tangle tent, which, on the following day, I had considerable difficulty in withdrawing. It was only after continued traction, kept up steadily for nearly twenty minutes, that this was accomplished. When removed from the grasp of the cervix, its form explained the difficulty. At an inch and a half from its proximate end, the tent was not altered in form from its size before it was introduced; whilst above and below this narrow waist, it had swollen to its utmost extent; from this compressed part ran a narrow groove on each side, evidently showing that, at the constricted os internum, as well as longitudinally on each side, there existed unyielding tissue, not only at the points of the original stricture, but also at lines corresponding with the wounds made by the hysterotome, which led me to the conclusion implied in these remarks—viz., that the deep grooves and the longitudinal furrows were the impression of hard cicatrices, the effects of the previous operation. Seeing that the result of incising the cervix, if successful in enlarging the canal, must be to destroy the perfectness of its structure to the extent of the tissue divided by the hysterotome, or that, if rendered unsuccessful by the healing of the wound, it might leave the patient worse than before, from the hardened cicatrix which might follow the operation, I soon adopted a course which I have pursued for many years with a success which encourages me to hope that its announcement will be acceptable to obstetric practitioners generally.

In my lectures, at the commencement of this year, on flexions, torsions, and displacements of the uterus, at the Hospital for Women, I observed that, "finding sterility and dysmenorrhœa were often benefited by dilating the cervix uteri by the bougie, after the manner advocated by Dr. Mackintosh, and with a view of improving upon the plan suggested, I got Messrs. Fergusson, instrument-makers to St. Bartholomew's Hospital, to make me in 1841 the instrument I now exhibit, after the model of Heurteloup's lithotrite, by which the extent and direction of the uterine cavity was easily measured, and a constriction in any part of the passage as readily overcome." With this uterine dilator, I conceived it might be practicable to dilate permanently the constricted os internum, and afterwards, when necessary, to give the normal shape to the os tincæ by dividing it latterly at the commissures of the labia uteri *per speculum*. I have now adopted this mode of treatment for more than a quarter of a century; and although, in some instances, I have failed to cure, yet I have succeeded in so many others, that I feel warranted in advocating its adoption in the selected cases in which it is eligible. To diagnose such, it has been my custom to ascertain from the history of each case, as well as by a careful physical examination of the uterus, that it is one simply of stricture of the os internum, and narrowing of the cervical canal and mouth. But should there be any inflammatory condition, whether of the mucous lining or of the deeper tissues of the uterus, or any thickening or induration of these parts, it is essential to success, however tedious the process may be, to remove such morbid conditions before adopting the *extension forcé*, by which the stricture is to be overcome.

The plan which I then pursue is, first to prepare the patient by a purgative dose, and by abstinence from local excitement, and from alcoholic drinks, or much animal food. When hyperæmia exists, I scarify laterally the labia *uteri* repeatedly at the commissures of the labia, by which the vascularity of *the organ* is reduced, and the shape of the os tincæ, when constricted, is im*proved*. After accustoming the uterine canal to bear a metal bougie, which should be repeatedly and daily introduced, and increased in size until that of a No. 10 catheter can be borne without any pain, then the uterine dilator may safely be employed. It will be seen that this instrument consists of two short blades, two inches and a half long, the inner being continuous with the sliding shaft, with which it is nearly at a right angle, having at its proximate end a screw worked by a nut so as to mark precisely, by an index on the handle, the extent of dilatation employed. This should be used at first cautiously about every second day, always ceasing to screw as soon as pain is experienced. This is immediately relieved by a turn or two of the screw the reverse way.

It will be found, in a short time, that the uterus becomes accustomed to the dilatation, when it may be employed to a greater extent; and in the course of a few days or weeks, as the case may be, a forced dilitation to the extent of an inch or an inch and a half may be used with impunity. After this it will only be necessary to use the dilator daily for two or three days, and afterwards at longer intervals, to keep the parts open till they permanently heal in the state of distension effected by the operation.

Should any congestion or inflammation result, scarifying at the commissures of the labia will relieve by free bleeding, whilst, at the same time, the os tincæ is made to assume a more open, and, therefore, a more normal shape. I prefer, generally, to effect this by the repeated use of a small scimiter-shaped knife, as I find that by so doing the risk of inflammation is diminished, and it prevents cohesion of the cut sides of the labia uteri, both which accidents occasionally attend the operation when performed at once by a hysteratome. When preternatural shortness of the uterus, from original malformation, exists, the operation is contraindicated; also when stricture depends on endometritis; when, otherwise, metritis or metrocellulitis is present; when there are fibroid tumours causing inflammatory adhesions; when dysmenorrhœa is characterised by deciduous membranes; when there are conical hypertrophy and elongation of the cervix; when there is globular enlargement of the anterior labium uteri, embraced by the posterior lip in the form of a crescentic membrane; and when displacements and dislocations of the organ complicate the case—these and all other organic diseases which may attend this malady should be removed prior to the adoption of forcible extension by the dilator.—*British Medical Journal, Dec.* 16, 1871, *p.* 694.

98.—ON THE TREATMENT OF SIMPLE MENORRHAGIA AND SIMPLE METRORRHAGIA.

By Dr. J. Matthews Duncan, Edinburgh.

The treatment of simple menorrhagia and of simple metrorrhagia is a very easy matter, so far as it goes. It is divided into the treatment before the loss begins, and the treatment while the loss is going on.

The former treatment, that before the loss begins, is only possible in chronic cases, or cases where there has been at least one previous hemorrhage; for in any other its occurrence has not been and cannot be foreseen. It consists in the application of remedies to any condition which may appear to predispose to, or to aid in maintaining, the complaint. Congestion of pelvic vessels may be diminished by saline laxatives. Congestion of the portal system may be diminished by so-called antibilious medicines. A state of anæmia may be diminished by the use of chalybeate medicines. The general health may be improved by remedies or by hygienic regulations.

The great treatment or chief treatment is that applied during the continuance of the flow. It is of various kinds.

Mechanical treatment is most valuable, and probably as efficacious as any other. It consists, first, in maintaining rest in the horizontal position. By this means the pressure of blood on the vessels of the uterine mucous membrane is diminished, and the bleeding proportionately discouraged. This treatment is favoured by keeping the patient cool. It is applicable in every case. A second kind of mechanical treatment, that by the vaginal plug, is only applicable in severe cases in women who have cohabited, or in the very severest cases where cohabitation has not occurred. This remedy discourages the bleeding by impeding or resisting its escape. The vaginal plug generally resorted to in cases of simple menorrhagia or simple metrorrhagia is of an imperfect kind, yet often efficient: that is, it is not sufficient absolutely to prevent or arrest hemorrhage which proceeds with any great force, as would be the case, for instance, were an artery opened by incision; but when the hemorrhage may be, as it often justly is, likened to an oozing, its action is powerful enough to arrest it. This imperfect plugging is effected by pushing adroitly into the vagina a piece of sponge as big as the fist when not compressed, or even bigger. For the sponge, bits of lint may be substituted, and inserted one after another. It is common to dip the plug, before using it, in vinegar and water, or in a weak solution of alum. It is, no doubt, possible to explain the action of the plug, by supposing that it excites contraction of the muscular tissue surrounding the uterine vessels, which contraction will restrain the hemorrhage; but the purely mechanical explanation is more in accordance with ordinary medical thought. The plug must be removed in twelve, or, at most, twenty-four hours, and a new one placed, if it is required. More thorough and powerful plugging is very seldom used in cases of the kind under description. To effect it, the vagina is crammed with pieces of lint so as completely to occlude it. Each piece is, when rolled up, about the size of a walnut, and is introduced separately as high as it can be pushed. If the lint is previously dipped in a solution of perchloride of iron, the plug is more efficient, probably because it soon gets hard in the vagina, and more powerfully resists the tendency to hemorrhage. But this dipping in solution of perchloride of iron makes the plug more difficult and painful to remove.

Another kind of treatment resorted to only in extreme cases is probably also mechanical in its action. This consists in painting the bleeding surface—that is, the mucous membrane of the cavity of the body of the uterus—with perchloride of iron. A syringe, containing about a drachm of the pharmacopœial solution or liquor of perchloride of iron, is fitted to a hollow uterine probe after it has been passed up to the fundus uteri; the iron is then gently injected into the uterine cavity; only what is easily thrown in being passed. This hardens and destroys the mucous surface, and so arrests the oozing hemorrhage. The superficial layer of mucous membrane is sometimes subsequently thrown off like the membrane of dysmenorrhœa. I have observed the application and efficiency of this remedy with the naked eye in a case of chronic inversion of the uterus, with excessive menorrhagic loss.

The local application of cold is much esteemed as a remedy by many practitioners, but I have so often seen it fail, and so often seen it apparently increase the loss, that I am very doubtful as to its real value. Cold is, with this view, applied to the lower part of the belly, the vulva, the insides of the thighs, and occasionally even to the interior of the vagina and rectum. It is done by using cloths, wrung out of cold or iced water, and frequently rewetted and reapplied, or by the use of suitable mackintosh or indiarubber bags filled with pounded ice. To the inside of the vagina and rectum cold is applied by injection of iced water, or by the insertion of pieces of ice whose edges are well rounded. The use of cold is probably derived from the application of the same remedy in post-partum hemorrhage. When used in post-partum hemorrhage, as it is in cases of simple menorrhagia, its value has always appeared to me very doubtful. But in cases of post-partum flooding it has another invaluable application, which is probably the parent of all the comparative inefficient uses of it which we have described or referred to. Its sudden application, acting diastaltically or through the reflex system of nerves, will often excite an obstinately inert uterus to activity at once when friction and kneading of the uterus have failed; and this single excitement of uterine action may be sufficient. After this result of suddenness of application is gained, the repeated use of it, or its continued use, is comparatively inefficient; and it is this continued use only which is resorted to in cases of menorrhagia or metrorrhagia.

The lapse of hours or days, during which the patient rests, brings the arrestment of most cases of simple menorrhagia or simple metrorrhagia. At the me time the physician administers remedies, and while he thus comforts his tient, himself blindly supposes he is contributing to what he calls her cure. is the so-called hæmostatic medicines that are universally resorted to in ses of this kind. There is nothing worthy of the name of evidence to show at they act. Like my brethern, I use them, and believe they have some ight influence in diminishing or arresting the discharge. They comprise the ineral acids, especially the sulphuric, ergot of rye, digitalis, gallic and tannic ids, and many others.

Dilute sulphuric acid is the hæmostatic medicine in which I have most confi- nce. It is often given in the form of the infusion of roses. To find what it n do, it may be necessary to use it freely. From ten to twenty drops pro- rly conveyed may be given from four to eight times daily.

Ergot of rye and digitalis I have used in many ways, and, having no reason prefer one way to another, must leave the details of their administration to e intelligence of the practitioner or to his caprice.

The chances of therapeutical benefit to the patient may be increased by ombining the ergot of rye with the dilute sulphuric acid, or the digitalis with e same.

Gallic acid is probably more extensively used during the loss in these dis- ases than any other remedy. It is given in doses of from ten to twenty grains hree or four times daily or oftener. It has the advantage over the mineral cids of not setting the teeth on edge; but it is, I believe, far less useful than hey are. The same remarks apply to tannic acid.—*Edinburgh Medical Jour- al, March* 1872, *p.* 774.

99.—THE VALUE OF ARSENIC IN MENORRHAGIA AND LEUCORRHŒA.

By Dr. J. H. AVELING, Fellow of the Obstetrical Society of London, &c.

[In properly selected cases of menorrhagia and leucorrhœa arsenic is a remedy of great value. Its use was suggested to Dr. Aveling many years ago by a remark in a work on midwifery, stating that a teaspoonful of Fowler's solution of arsenic is of service in hemorrhage after delivery.]

Numerous as were the disorders treated by arsenic in the beginning of the present century, it was not until 1838 that its employment was, as far as I am able to discover, first suggested in cases of menorrhagia. In March of that year, a paper was read before the Royal Medical and Chirurgical Society of London, on the Use of Arsenic in some Affections of the Uterus, by Dr. Henry Hunt of Dartmouth, now of London. In 1834, he administered to a woman suffering from cancer of the uterus four drops of the liquor arsenicalis three times a day, gradually increasing the dose to ten drops; and found that, as the poisonous effects of the mineral presented themselves, in the same proportion the pain in the womb subsided. This fact, coupled with the recollection that inflammation of the genitals sometimes follows administration of arsenic as a poison, induced him to hope that it might be useful in some disorders of those parts. He was further strengthened in this opinion by the experiences of his father and of Sir Charles Locock. The latter told him that he had cured a lady of menorrhagia by arsenic, having recommended it to her for a disorder of the nose, being ignorant at the time that she was subject to the former disorder, she having neglected to mention it, considering it quite irremediable, as it had baffled the skill of every physician she had hitherto consulted. Dr. Hunt's father had met with a similar case in a girl of seventeen, who had consulted him for a leprous eruption on the knees and elbows, for which he had prescribed three drops of the liquor arsenicalis three times a day. At the end of three months, her mother called on him to thank him for not only curing her daughter of the eruption, but for making her regular, she having menstruated too frequently and profusely. Dr. Hunt, after relating several cases in which arsenic had proved successful in his own practice, says that, from the immediate and progressive improvement which succeeded the administration of the arsenic, the cessation of the menorrhagia may be fairly attributed to the action of that medicine. He explains the benefit derived from it as being due to its stimulating action upon the mucous membrane; and states, consequently, that it can be given with greatest advantage in those disordered conditions of the uterus which have been induced and kept up by debilitating causes.

It is remarkable that, notwishstanding the clear manner in which the claims of arsenic have been urged by Dr. Hunt, it is still little used by gynæcologists. One may look through book after book, and no mention of its employment will be found. In two of the most complete recent systematic works on the diseases of women, it is referred to in the following brief sentences. "The arsenical preparations," remarks Dr. Courty, "appear to me to be followed, after long usage, by an advantageous result, when there seems to be a relation between the uterine disorder and a herpetic diathesis." Dr. Fleetwood Churchhill simply says, "Arsenic has been tried with success in menorrhagia and cancer uteri by Mr. Hunt." In 1858, Dr. James Begbie quoted, in the Edinburgh Medical Journal, the experience of Dr. Hunt, and stated that both Sir James Simpson and himself had used arsenic with success in uterine complaints. Professor Hardy recommends arsenical medication in leucorrhœa of the neck of the womb; and Dr. Barnes tells me that he has used the same remedy in cases of menorrhagia. I am happy to be able to add the name of another author who has not neglected to mention the virtues of arsenic. Dr. Tilt, in his Handbook of Uterine Therapeutics, says: "When we consider the structural analogies that exist between the skin and the mucous membrane, it is surprising that arsenic has not been more frequently tried. I have given it with good results in cases of chronic uterine subacute inflammation with marked tendency to relapses." He does not agree with Dr. Courty in inferring that a disease of the womb must be herpetic because it yields to arsenic. In the Proceedings of the Gynæcological Society of Boston, U. S., Dr. Wells relates a case of spasmodic menorrhagia in which the arsenic treatment succeeded admirably.

The arsenical waters of Bagnères-de-Bigorre have been used for some time in the treatment of these diseases. Dr. Macpherson informs me that the arsenical water of Whitbeck, on the Furness and Whitehaven Railway, has been highly recommended as a therapeutical agent by Dr. Robinson of Newcastle; but I am not aware whether its effects have been tried in the disorders we are now considering.

I will not detain you longer with further historical remarks, but will at once proceed to give you the results of my own experience.

The preparations I have usually employed are two—the liquor arsenicalis; and the arsenious acid in granules, each containing one *milligramme*. This latter is an elegant form of administering the remedy; for, as it has to be taken at meal-times, the granules can be placed on the tablecloth, wrapped in a morsel of bread, and swallowed unobserved. Considerable difference of opinion exists as to the best mode of giving arsenic, some employing large doses in quick succession, and others small, extending over a long period. Aran says: "The rapid mode of administration is better than the long continuance of small doses, because the economy habituates itself to the latter, and the therapeutical effects may be lost; and the proceeding has the additional disadvantage of leading more easily to the saturation of the economy, and consequently to intolerance." On the other hand, Sir James Simpson says: "Most reliance ought to be placed on small and very long continued doses of arsenic; and it is infinitely better and safer to trust to the curative effect of the long continuance of such small doses of this remedy, than to attempt to arrive at the same result by throwing in larger doses for a shorter time." Dr. Hunt says: "Large doses taken for a short time produce much distress, without the desired effect on the uterus." The plan of small and long continued doses is the one which I have always used; and the result has been so satisfactory, that I have never thought of adopting the more rapid method. The doses with which I commence are from two to six drops of the solution, and from one to three of the granules, three times a day, at meal-times. These are small doses, when we remember that a Styrian arsenic-eater has been known to take as much as five nd a half grains of arsenic at once; but they are strong enough to commence rith, and may be increased from time to time as the necessities of the case uggest, and the patient's capability of bearing the remedy permits. It is adisable to suspend its administration occasionally for a short time. This, ineed, may sometimes be necessary, should diarrhœa, nausea, or pains in the tomach, supervene. It is also better not to discontinue the doses abruptly. hey should be gradually diminished in quantity, and taken less frequently.

The first effect of arsenic is to improve the digestive powers. The appetite eturns; and often in two or three weeks the patient has improved in appearnce and increased in weight. Besides this improvement of nutrition, there is oon evident increased tone of the nervous system. Respiration and secretion re better carried on; and M. Lolliot has established that ten *milligrammes* of rsenious acid, taken each day, produce lowness of temperature and diminuion of urea. The Styrian arsenic-eaters are generally strong and healthy perons, courageous, and of strong sexual disposition. If requested to explain vhy they take arsenic, they say it is to make them strong and healthy, and to mprove their wind in ascending mountains. M. Isnard, in his work on Arsenic in Diseases of the Nervous System, shows that it replaces altogether edatives, antispasmodics, and tonics, calming pain, spasms, and convulsions, and stimulating depression of the nervous force. For my own part, I think the most valuable therapeutic effect of arsenic is its decongestive action upon mucous membranes.

But, before endeavouring to explain the mode in which arsenic effects a cure in cases of menorrhagia and leucorrhœa, it would be well to examine the primary pathological condition of the uterus which causes them. This condition is, in a great majority of cases, one of hyperemy, which has been defined by Andral, the inventor of the word, as excess of blood in the capillaries. Hyperæmia of the uterus may have a physiological or a morbid origin—physiological, when caused by sexual excitement, menstruation, or pregnancy; morbid, when it is the result of pre- or post-inflammatory action, of traumatic, chemical, or morbid irritation, of an atonic, obstructive, or hypostatic cause, or of heat, cold, etc. Of course I do not wish to deny that menorrhagia and leucorrhœa may be produced by polypi, muscular fibroids, cancer, and many other pathological conditions; but I would at the present time draw attention more particularly to those forms of menorrhagia and leucorrhœa which have a hyperæmic origin, because it is in these that arsenic will be found most efficacious. In short, it is the morbid condition, of which menorrhagia and leucorrhœa are but the secondary phenomena, which I propose to treat with arsenic; for, if we can cure the former, the two latter must necessarily disappear. Yet I would not have it understood that these consequent symptoms are to receive no attention. Both must be checked when excessive. All gynæcologists, however, know how imprudent and injurious it is to stop abruptly discharges which are often nothing more than Nature's method of relieving the hyperæmic condition of the parts from which they emanate. Hyperæmia of the passive or atonic character is that which is most benefited by the use of arsenic. The uterus, when in this condition, is larger and softer than in its normal state. It is usually tender to the touch, but not always so. To the eye it appears of a deeper red than is natural. After death, the capillaries are found dilated, and the tissues tinged with red. Unlike the colour produced by inflammation, however, this redness can be removed by careful washing. A patient coming to you with her uterus in the state just described, will, in addition to a host of other subjective and objective symptoms, most probably complain of the too frequent recurrence of the catamenial period, of the excessive discharge at that time, and, in the intercatamenial period, of persistent and distressing leucorrhœal flow. Now, in such a case as this, I should commence by administering two drops of the liquor arsenicalis, or one granule of arsenious acid, three times a day, at meal-times. This dose I should continue for a fortnight. If, at the end of that time, no conjunctival irritation had displayed itself, I should increase the dose to four drops of the solution or two of the granules; and then again, after another interval, to six eight, ten, or even more drops, or granules in proportion, watching the patient, and being guided by her tolerance of the remedy.

Besides the general effects of arsenic already alluded to, the first result of this treatment will be the lengthening of the intercatamenial period; and it is remarkable how gradually this is sometimes extended, one or two days being only gained at a time. By persisting in the remedy, however, the interval will become greater until it arrives at its normal duration. Occasionally the progress is more rapid, and the proper interval is at once attained. Besides the improvement in this respect, the amount of the discharges will gradually decrease, and in like manner all the other hyperæmic symptoms disappear. I have never found it necessary to administer large doses, and cannot remember ever having produced any of the premonitory symptoms of arsenical poisoning beyond that of conjunctival tenderness. I have been obliged, however, to continue the remedy for several months, and have had to recur to its use more than once when the hyperæmic symptoms have reappeared. In some cases, an excessive leucorrhœal discharge has the effect of supplanting the catamenial. In these the cure of the former has the result of removing the amenorrhœa. The late Dr. Wright says: "Arsenic has succeeded in my own practice when a long succession of other remedies has previously failed to induce or re-establish menstruation." If I am asked to explain what is the therapeutical action of arsenic in these cases, I frankly own my ignorance, and admit that, like many other medicines, I use it empirically, and patiently await the explanation which science may some day be able to give. Theories are not, however, wanting. Dr. Hunt, judging from the fact that inflammation of the genital organs has been produced in some cases of arsenical poisoning, infers that the action of arsenic in curing uterine disorders "may be explained by its acting on the mucous membrane of the uterus as a stimulant." Dr. James Begbie, believing the uterus to be affected in its functions and structure through rheumatic and other morbid conditions of the blood, says that "the efficacy of arsenic resides in its powerful alterative effects upon the blood." Another theory, which, without denying the possible truth of the two foregoing, I feel most inclined to adopt, is, that the arsenic, circulating with the blood, acts upon the vaso-motor nerves of the capillaries as a stimulant and tonic, causing them to contract and expel the superabundant blood. But, whatever theory may be correct, the practical results of the administration of arsenic in menorrhagia and leucorrhœa are most satisfactory; and I shall be glad if these few remarks have the effect of once more drawing the attention of the profession to a safe and potent remedy, by the means of which two of the most common and distressing complaints from which women suffer may be brought under subjection and finally cured.—*British Medical Journal, Jan.* 6, 1872, *p.* 10.

100.—ON MENSTRUAL COAGULA.

By Dr. John Haddon, M.A., Eccles.

Abortion is a very convenient name, and under it are classed many cases which have no claim to such a designation.

It is not so generally known as it ought to be, that blood may be retained in the uterus for one, two, or more months, without any untoward symptoms, until hemorrhage begins, and the clot is expelled with pain and flooding.

Such cases as these may have, no doubt, the history, as well as some of the signs and symptoms, of abortion; and even the product may be mistaken for an aborted ovum—more especially if any of the decidual uterine membrane, which is seen in membranous dysmenorrhœa, be present.

I had not long been in general practice before meeting with cases which I supposed to be abortions, where, nevertheless, I could find no trace of any part of the ovum. There is no great harm in telling a woman in married life that she has aborted, where the symptoms of abortion were present, even though no trace of the ovum can be discovered. When it happens, however, as it did with me, that you meet with such a case in an unmarried young lady, you must pause and consider the case very minutely before even hinting at the possible occurrence of conception. I shall relate the case which led me to suspect that abortion may be in many cases simulated by an affection having its origin in some peculiar state of the menstrual function.

A. H., aged 28 years, and unmarried, states that, when sixteen years old, she had a fever during which menstruation began. It continued normal till about the age of twenty, when, after sitting on damp grass, she had an affection of the uterus, which confined her to bed for some weeks. Since that time menstruation, though regular, has been attended with considerable pain, and followed by more or less leucorrhœa. She has also been subject to hysterical attacks in the form of convulsions. From April 1870, menstruation was regular every month, lasting about three days, and followed by copious leucorrhœa, which continued during the interval. She also complain of pain in the back and in the left side, was unable to walk much, and very subject to hysterical convulsions. She improved under general and simple local treatment. In June she had no hysteria, very little leucorrhœa, no pain, and was able to walk long distances.

On 12th September, 1870, I was called in the night to see her. I found that, since June, she had been quite regular, with very little leucorrhœa, and had felt stronger than usual. On 1st September, which was her monthly period, menstruation did not occur. She did not feel ill in any way until 12th September. On that day she had pain in the abdomen, and was altogether out of sorts. During the night she became hysterical, and lay in a half-unconscious state. Her pulse was normal. There was no uterine discharge; but she complained of pain in the left inguinal region, which was increased on pressure. Over the tender part there was some fullness, and slight dulness on percussion. Poultices were applied, and she was confined to bed. The pain gradually subsided, but she was advised to stay in bed till the next period had passed. On 30th September she began to menstruate. On 1st October it became very profuse. I found her flooding, and was shown several clots the size of walnuts, which had been discharged. The face was pale and the lips blanched: the pulse was quick and weak. On vaginal examination, the uterus was found low down in the vagina, and quite movable; the cervical walls were thickened, the body larger than natural, and the os just open enough to admit the tip of the forefinger. Ergot was administered, and other means used to stay the flooding, which soon abated. The sanguineous discharge continued. She had occasional sharp pains in the back, and some difficulty in micturition. On the twelfth day from the beginning of the discharge, when it had almost ceased, a mass was passed which in shape resembled a cast of the interior of an enlarged uterus. On vaginal examination at this time, the uterus was higher up and larger than before—as large, indeed, as it would be in a two months' pregnancy. The os was open enough to admit the forefinger. After the appearance of the mass, to be described immediately, the discharge entirely ceased. She menstruated again in the end of October, and has been well since.

Description of the mass.—It was slightly pear-shaped, and as large as a small hen's egg. Its consistence was firm, and from its narrower end hung long shreds of fibrin. On section through the middle, a cyst was found in the broader part. This cyst was an inch in length, half an inch in breadth, contained clear serum, and was lined by a smooth shining membrane. Around the cyst the substance was of a dark red, almost a black, colour; while the outer part was of a firmer consistence, and paler in colour. No organized structure could be detected by the microscope.

Such is an account of the facts of the case. Had it occurred in a married person I should have thought it was an abortion, but as there was not the least suspicion of pregnancy, I was induced to give the case more consideration. There could be no doubt as to the mass expelled being an old clot of blood. What its age may have been is a matter of opinion; and some may doubt whether it was uterine. I am inclined to believe that it was formed in the uterus on 12th September, because when menstruation began the uterus was larger than natural, and continued to enlarge until the mass was expelled.—*Edinburgh Medical Journal, Jan.* 1872, *p.* 611.

101.—ON THE SIGNIFICANCE OF UTERINE AND VAGINAL DISCHARGES.

By Dr. Robert Barnes, Obstetric Physician to St. Thomas's Hospital, &c.

[Air may get into the vagina, if not into the uterus, in the non-pregnant state. Under peculiar circumstances air enters in large quantities, to be expelled with noise. In one case related by Dr. Hartley there was found to be retroversion of the uterus, and this displacement being remedied, and the patient's health improved, a cure ensued.]

If you observe the vagina when the duct-bill speculum is applied, you will see movements of rise and fall under the influence of the rise and fall of the diaphragm. Dr. Adolph Rasch has investigated the phenomena with great care. He says, if a multipara whose genitals are normal be placed on her back, with the thighs flexed and abducted, and the vaginal orifice closed, movements caused by respiration are seen, but no air enters. In the lateral position the same thing is observed even if the vagina is lax, and even when the perineum is ruptured. When the patient is placed in the prone position, or on all fours, if the vulva be open, air will enter, because the intestines falling downwards by gravity causes a vacuum. Under this condition violent exertion may expel air, giving rise to vaginal flatus. If the abdomen be supported by the hands or by a bandage, no air enters.

There are several interesting applications of this knowledge. It teaches that the best position after labour, if not during labour also, is the dorsal; that the same position is also best in the case of pelvic abscess or hæmatocele discharging into the vagina; and that we must carefully consider this respiratory rise and fall of the vagina when selecting pessaries. It is by turning to account this action that we derive the greatest advantage from the spoon or Sims's speculum. The blade drawing the perineum well back, whilst the semi-prone position of the patient favours the falling forwards of the abdominal viscera, air fills the vagina, counteracts the effect of inspiration, and thus enables us to get a good view of the os uteri. The same position also greatly aids our efforts at reducing inversion of the uterus, and in replacing a prolapsed umbilical cord. In most operations, however, upon the uterus and vagina, where it is of importance to bring the uterus as low down near the vulva as possible, the dorsal position, by bringing the force of gravity to counteract the respiratory rise of the uterus, which can further be greatly aided by direct pressure by an assistant's hand above the symphysis pubis, is the best.

If we were to follow out in detail all the indications supplied by the examination of the discharges, we should be led through almost the entire range of uterine and vaginal pathology. This, in fact, is simply the converse proposition to one already stated—namely, that almost every uterine or vaginal disease is attended by discharge. But to adopt the discharges as our point of departure, from which to proceed to study and classify the diseases of which they are consequences, would be a most intricate course, and not the most orderly or profitable. I will therefore not pursue this method further than to give one instance of how we may apply it to clinical analysis. We will at our next meeting take hemorrhage, and sketch what its purport is.

The Watery Discharges.—When these occur, you must first of all determine the presence or absence of pregnancy. It is no uncommon thing that discharges of water, more or less profuse, take place in pregnant women. This is the hydrorrhœa gravidarum. Gushes of water, quite clear, may occur at almost any time during pregnancy; but they are more frequent in the latter months, and especially in the last month. Happening at this time, they are commonly taken as an indication of commencing labour, and many are the false alarms which patient and doctor have to suffer from this cause. "The waters have broke," says the nurse. You go as in duty bound, and find probably the os uteri closed, nothing resembling active labour-pains. What are you to do? If you wait for labour, you may wait for a week, or two or three weeks. If, on examination by ballottement, you find the child still floats in the uterus, the os uteri not open, and no active pains, you may go home and wait in peace for another summons.

What is the source and nature of this hydrorrhœa gravidarum? Several theories have been expounded. The character of the fluid differs in some respects from that of liquor amnii. It is odourless, and resembles blood-serum or the serous fluid effused in the peritoneal sac. Ruysch and Rœderer thought it came from ruptures of lymphatic vessels, or of hydatids of the uterus; Böhmer thought it escaped from a second abortive ovum; Delamotte and Cruveilhier that it came from a cyst near the ovum; Deleurye, Puzos, Naegele, and Dubois that it came from the inner surface of the uterus, being secreted externally to the ovum. Dubois says it is the result of loosening of the membranes from the uterus when the vessels pour out serum. Hegar says the source is the uterine glands of the decidua. Thus, he describes the glands of the mucous membrane as being found in the decidua at the sixth month of gestation, and argues that their sudden disappearance in the subsequent months is improbable. In a case of hydrorrhœa he found in the decidua vera, at the beginning of the eight month, an enormously developed glandular body. At the bottom of this morbid growth was a general hypertrophic condition of the decidua and its glands. These gave out the excessive secretions. In a case related by Dr. Graef, repeated discharges took place, and the fœtus was expelled at the end of six months. The membranes were very delicate, and openings were found in them. In this case, it is probable that the fluid was true liquor amnii. In another case the patient suffered, during the last three months, from repeated watery discharges; the uterus rising and falling with the gathering and escape of the fluid. The membranes were found without rent. Graef regarded this as a case of catarrhal hydrorrhœa.

I believe there are various sources. In some cases the fluid is liquor amnii. This may come either from rupture of the membranes; from rapid transudation under pressure from rapid formation and accumulation of liquor amnii in the amnion; or from the bursting of a cyst formed between the amnion and chorion, or between two layers of chorion, the proper amniotic sac remaining intact. In the majority of cases, however, the fluid is not amniotic; for when once the amniotic sac has fairly ruptured, labour is not far off. It is, then, the result of a rapid secretion from the uterine glands or from the cervical cavity. In the early months, whilst there is still a free space between the decidua vera and the decidua reflexa, there is a large area of developed glandular surface.

I have observed a *puerperal form of hydrorrhœa*. Thus watery discharges may continue for a month or longer beyond the proper lochial flow. Generally in these cases the water is dirty, discoloured, occasionally stained with blood, and offensive. The most common cause I have found to be the retention of a portion of placenta or of clots in the uterus; but a polypus may produce like results. The watery discharges alternate, but not always, with discharges of blood. The fluid may, under certain conditions, collect in considerable quantity in the uterus, so that the organ becomes greatly distended before the collection is expelled in a gush.

Sometimes watery fluid is mingled with air, constituting *physo-hydrometra.* This is also a puerperal or post puerperal condition; and is commonly the result of retention of some portion of placenta or membranes, and the admission of air into the uterine cavity. If an examination is made when the uterus is relaxed after labour, especially if the hand be introduced into the uterus, the vaginal walls are separated from their usual contact, and a channel is formed along which air easily enters. Merely turning on the side, or a little more prone, will often, by favouring the fall of the uterus forwards, produce a vacuum into which air will rush. This is one reason amongst others, why I am unable to approve of the abolition of the old-fashioned binder, which some people would condemn, for no better reason that I can see than because it is old-fashioned. After labour, especially in pluriparæ, the abdominal walls are so relaxed that they can give no support to the uterus. The binder does temporary duty for the inert abdominal walls. The history of physo-hydrometra is, I believe, this: a portion of placenta, membranes, or clots, remain in the cavity of the uterus after labour; some air gets in as I have described; decomposition ensues, and the gases of putrefaction are added to the air from without, whilst the os uteri is occluded by the placenta or blood-mass falling over it. When this occurs, there is invariably hectic or irritative fever; peritonitis and septicæmia commonly attend; great abdominal pain; the enlarged, distended uterus can be mapped out rising as high as, or higher than the umbilicus; and resonance is made out on percussion.

One condition, the result of impregnation, often leads to copious and repeated discharges of watery fluid: the *hydatidi-form degeneration of the chorion*. In this case the ordinary signs of pregnancy may not be present, and even the patient herself may not think she is pregnant. There is, however, always evidence of enlargement of the uterus, and generally great pelvic distress. The water escapes in gushes at uncertain times; it is often tinged with blood, resembling red currant water; it has not the offensive odour belonging to the watery discharges of cancer; sometimes, but not often until late in the progress of the case, cysts will be found swimming in the water; it is generally expelled with painful uterine contractions. In a case we recently had in Adelaide ward, the nature of the disease was not at first suspected. There was some abdominal enlargement, retention of urine requiring the catheter, and most distressing pelvic pain with irritative fever. The os uteri was found high up above the symphysis pubis, whilst behind it the pelvic cavity was filled with a large, rounded, firm mass, taken to be either the retroverted gravid womb or a fibroid tumour. One day a large quantity of water, blood, and a mass of chorion-cysts were expelled. We had, in fact, the condition of retroverted gravid womb complicated with hydatidiform or cystic degeneration of the chorion.

Apart from pregnancy, watery discharges are often of grave significance. During and after the climacteric period, the most frequent cause is some form of malignant disease; especially the so-called cauliflower excrescence of the uterus. In this case other symptoms will probably point to the seat and nature of the disease. The fluid discharged is seldom clear; it is generally turbid, dirty, often tinged with blood, resembling water in which flesh has macerated; it contains shreds or flocculi of solid matter, the proceeds of superficial erosion or necrosis of the surface of the diseased growth, and is almost always of a peculiar offensive odour. It often alternates with hemorrhage. Local exploration will place the nature of the case beyond doubt. Another form of malignant disease giving rise to watery discharges is the "oozing excrescence of the labia."

But we must remember that similar discharges may take place from polypus or inversion of the uterus. Hence we have another example of the wisdom of not pronouncing a diagnosis until we have made an internal examination. Water may escape in large quantity from the rupture or perforation of an ovarian cyst into the vagina. In such a case, the rapid concurrent diminution of the abdominal tumour will lead to the right conclusion.—*Lancet, Feb.* 10, 1872, *p.* 177.

102.—ON HYSTERIA AND ITS INTERPRETERS.

By Dr. Edward John Tilt.

I think it requires two factors for its production—1. A predisposing nervous state; 2. The stimulus of some determining cause.

Of the predisposing cause, we may safely say that it must depend on that modification of the nervous system which makes the nervous system of woman more prone to emotion than her mate; otherwise how is it that the disease is in the main feminine, and only met with in men whose nervous systems are built on the feminine type? We moreover know that, although a disease of every climate and social condition, hysteria is most frequent in women of the upper classes of the civilized races, in whom emotionalism is intensified, at the expense of reason and self-control, by injudicious training in childhood, and the subsequent pampering that ill fits them for the trials of life. We can go no further than say that this undue action of the brain is the predisposing cause of hysteria. It may be that, in severe cases, this predisposition may be so strong as to be of itself sufficient to bring on the disease. At all events, we know that there are various degrees of intensity in this predisposition, and that the slightest determining cause will make some women hysterical. In a family with which I am intimate there are ten healthy children, whose parents are not in the least nervous; but a paternal uncle is insane; two maternal uncles died of delirium tremens; one brother has been epileptic from childhood; and a sister died of meningitis. Out of these ten children, two little girls—one seven, the other eight years old—burst into tears if they are looked at, if they are not placed as they like at table, and are not helped in their right turn. They pass rapidly from laughter to tears, which will flow for hours and very abundantly. They have sometimes globus hystericus. These symptoms have been repeatedly quelled by preparations of iron; but they occasionally return, and must be taken as evidences of the hysterical state, very likely to be followed by the worst manifestations of the disease on slight provocation.

With regard to the determining causes of hysteria, I must first mention those that intensify all nervous affections—debilitating influences like loss of blood, diseases, physical shocks, mental and emotional shocks, prolonged worry, and want of sleep. Neither should I omit the contagion of one hysterical nervous system on another predisposed to become so.

Coming to the most important causes of hysteria—those originating in the viscera—I will first remark that, as with our mental acts, so with our emotions, they are conceived in the brain; and that old physiology and the poetry of all times have erred in placing the actual origin of our passions in our abdominal organs. Still universal consent shows how strongly they are acted on by emotion—that, in fact, in the viscera are the reflex centres of emotion that stimulate the nervous system to emotional acts.

If I have, therefore, been correct in ascribing hysteria to undue action of the brain as an organ of emotion, a potent cause of hysteria must be found in undue action of one or other of our viscera. It is, no doubt, wonderful that bodies shared by us with the lower animals should not only support the bodily structure, but, by their healthy action on the brain, give lucidity to the mind and warmth to the feelings, making genius more admirable and charity more godlike. This sounds like poetry, but becomes plain matter of fact when we remember how often anger has caused jaundice, and how frequently a host of distressing mental and emotional sensations are due to that state of liver and stomach derangement that we call biliousness, and which doubtless acts by deranging the functions of the neighbouring great ganglia. I have likewise seen repeated attacks of hysteria brought on by biliousness, and their recurrence prevented by such measures as are best calculated to prevent biliary derangement. Such cases are, however, very rare, when compared with those in which the determining cause of hysteria is an ovarian or uterine ailment. The statistics of Landouzy, Brierre de Boismont, and Dubois d'Amiens, as well as the recent assertions of Dr. Crichton Browne, show this to be the case; and those who deny it must bring forward similar masses of equally well digested facts.

What, then, are the diseases of the sexual system that cause hysteria? Not those in which the structure of the ovary and womb are almost destroyed—acutely, as in abscess of the ovary, slowly, as in ovarian tumours and uterine cancer—but, as a rule, the mildest forms of anæmic ovarian uterine disease; showing that it is not the intensity of the disease that causes hysteria, but the fact of its coincidence with a nervous system prone to become hysterical. Thus hysteria is most frequently caused by those limited ovarian lesions that I have described as sub-acute ovaritis, lesions depending on morbid ovulation, and that frequently pass unrecognised under the disguise of diseases of menstruation. Of uterine affections, it is chiefly the milder sort—that are mucous membrane deep—which cause hysteria; and sometimes, by applying nitrate of silver to an ulcerated cervix, we most unwittingly bring on an attack of hysteria, in patients who presented no signs of its being likely to come on, and thus experimentally prove that the two complaints may stand in relation as cause and effect. On one occasion, I thus brought on an attack in a lady, who had never before had *one*.

How is the brain, laden with emotion, to be brought into contact with the viscera, the reflex centres of our emotions? The late Dr. Todd thought that hysterical delirium and other hysterical phenomena might be explained by toxæmia resulting from retained menstrual blood; but hysterical phenomena frequently arise before there is any menstrual blood to be retained; and Dr. Handfield Jones agrees with me, that with hysteria, as with other neuroses, there is no blood-poisoning. The distance between the brain and the viscera, between mind and appetite, is bridged by the ganglionic nervous system, which unites the viscera by a federal bond of union, and places this federation in intimate connexion with the cerebro-spinal system. When the ganglionic nerves transmit healthy impressions to the brain, they pass unnoticed; but a hysterical fit shows how differently nerves and ganglia act when visceral action is more or less diseased.

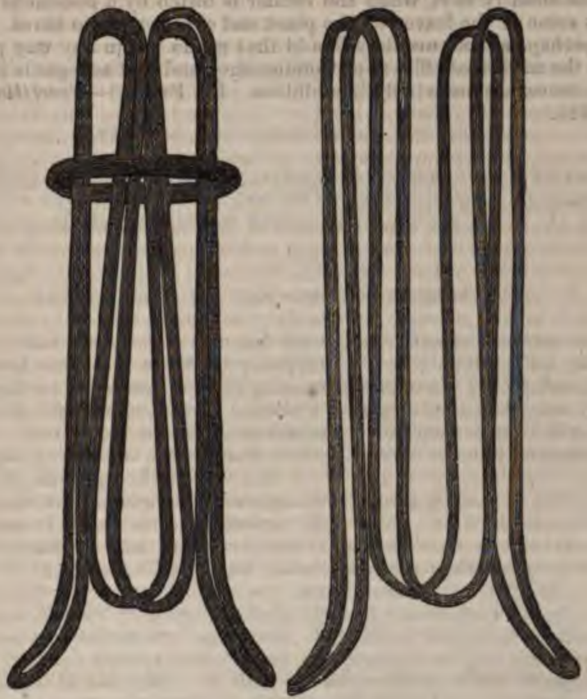
In many hysterical fits, after a period of incubation, in which the system seems to become more and more charged with excitement, the attack begins by pain in the womb and ovaries. Soon the hysterical aura passes to the epigastric ganglia, and, concentrating there, gives rise to the suffocation and distress characteristic of the disease. Ascending still higher, the hysterical aura reaches the cervical ganglia, producing the sense of strangulation; it then attacks the brain, deranging its functions in ways too numerous to be mentioned, and, at the same time, deranging more or less the functions of the spinal cord, according to the degree of tension of the hysterical aura. For a time pain will thus concentrate—sometimes in the visceral ganglia—and the patient collapses into prostration when the system has been sufficiently relieved by convulsions and by critical discharges. It has been possible, in cases published by Romberg and Schulzenberger, to produce the succession of phenomena just described by simply pressing on the ovaries; and I have repeatedly brought on unconsciousness in a nervous patient of mine by pressing the left ovary.—*British Medical Journal, Dec.* 16, 1871, *p.* 691.

103.—CHLOROFORM AND BICHLORIDE OF METHYLENE IN UTERINE OPERATIONS.

At a meeting of the Royal Medical and Chirurgical Society, Mr. SPENCER WELLS said he had long felt objections to chloroform, especially as regards the vomiting it produced. This was particularly objectionable in many of the operations he had to undertake, especially in the abdomen and vagina. He had tried ether on the advice of Dr. Keith of Edinburgh, but it was so troublesome that he was glad to take to bichloride of methylene. This he thought the best anæsthetic; sickness after it was rare. Some said it was only a mixture of chloroform and hydrochloric ether; that might be so, but as long as it suited his purpose he did not care what it was chemically. It was best given by Jünker's apparatus.—*Lancet, Nov.* 4, 1871, *p.* 638.

104.—BLACKBEE'S RESILIENT SKELETON SPECULUM.

The engravings here shown present the open and closed views of an entirely new form of speculum, of a most simple and novel construction, and which may be said to possess great advantages without mechanical complexity. Its chief features are as follows:—1. From its blades—outline or skeleton shape—being composed of hard German silver wire strongly plated with pure silver, it offers the most extensive view possible of the entire circumference of the vagina and cervix uteri, permitting the easy application of necessary remedies to their whole surfaces. 2. Its resiliency is a great feature in its favour, rendering it easy of introduction and safe in withdrawal, readily adapted, self-adjusting and self-retentive, thus allowing the free use of both hands of the operator. 3. Being free at both extremities, this speculum, by slight manipulation, may be contracted or expanded to suit special cases and requirements. 4. As to durability (having no screws or levers), it may be deemed practically indestructible, as nothing but absolute violence could possibly injure it. 5. It is extremely light for a metal instrument, and the cheapest speculum yet offered to the medical profession. The ring, useful at first, may, after a little acquaintance, be entirely dispensed with.



This speculum may be obtained of Messrs. Maw, Son, and Thompson, Aldersgate-street.—*Lancet, Dec.* 9, 1871, *p.* 820.

105.—CONDURANGO—A REPUTED REMEDY FOR CANCER.

[Some wonderful cures of cancer by this drug have caused quite an excitement on the subject in America, and in New York especially it has commanded an enormous price.]

Dr. Bliss, of Washington, is stated to have accomplished by its means several notable cures upon prominent personages, and considers it "as reliable a specific in cancer, scrofula, and other blood diseases, as cinchona and its alkaloids have proved to be in zymotic diseases." The resident practitioners of Ecuador are, however, less confident of its efficacy as a cure for cancer; but believe it to be very useful in cases of rheumatism and secondary syphilitic disorders. Dr. Jaramillo, of Guayaquil, has been eminently successful in curing syphilis, as well as intestinal, urethral, and uterine ulcers; the application being in some cases a decoction of the wood without the bark, but more often of both. Reduced to a powder, he says, an ounce will kill a good-sized dog. The milk of the plant he applied to ulcerated surfaces, and found that it promoted cicatrization. The name *Condurango* or *Cundurango* is said to signify in the Quicha language "Vine of the Condor," from the tradition of the country that, when the condor is bitten by a poisonous serpent, it swallows some of the leaves of the plant and experiences no harm.

[It is perhaps almost needless to add that we do not in any way pledge ourselves to the medicinal efficacy of Condurango; and that as regards its asserte action in cancer we are wholly incredulous. Ed. Pract.]—*Practitioner, Apr* 1872, *p.* 221.

SYNOPSIS,

(ARRANGED ALPHABETICALLY), CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE HALF-YEAR.

AFFECTIONS OF THE SYSTEM GENERALLY.

CANCER.—The most recent views of the nature of cancer are, that it is not a constitutional disease at all at first, but purely local; and that by subsequent absorption of its elements the system becomes affected. It may be said to be hereditary, in the same way as a tendency to any other affection (as warts) is hereditary. The use of caustics after removal of the mass of the tumour by the knife is coming more and more into favour. Chloride of zinc is especially suitable. (Mr. H. Arnott, p. 91.)

Condurango in Cancer.—Condurango is a climbing plant growing on the western slope of the Andes in Ecuador. It is said to be a remedy of value in the cure of cancer. The powdered bark is the part used, and is 'the rage' in New-York at the present time. Its action, however, must be more definitely ascertained before we should advise any one to trust to it. (Dr. Bliss, p. 376.)

CHOLERA.—*Nitrite of Amyl.*—Nitrite of amyl causes great dilatation of both the systemic and pulmonary capillaries. The blood flows rapidly through the capillaries into the venous system, and the pressure on the arteries becomes correspondingly diminished. As, from the observations of Dr. George Johnson, it appears certain that the collapse of cholera is owing to contraction of the pulmonary capillaries and arrest of the circulation commencing there, it seems not improbable that this peculiarity of the action of nitrite of amyl may be the very thing wanted in cases of cholera collapse. It would have to be given by the stomach, or subcutaneously. It has not yet been tried in actual practice. (Dr. J. L. Brunton, p. 88.)

Subcutaneous Injection of Morphia.—Dr. Patterson, of Constantinople, reports that in the late epidemic of cholera at that city, finding all other treatment unsatisfactory, he determined to try the subcutaneous injection of morphia. In the first case a quarter of a grain of the acetate caused relief to the cramps and vomiting in a quarter of an hour, and the skin became gradually warm and moist, and the pulse returned. In ordinary cases he found one or two injections sufficed, in a few three were given, and only once four. He does not maintain that the treatment is a specific against cholera, but that its action is more speedy, certain, and effectual than any other tried by him. Out of 32 cases in which the treatment had a fair chance, there were only ten deaths. (Dr. J. Patterson, p. 87.)

FEVER.—In severe cases of fever, what our efforts should be principally directed to avert is stagnation of the blood in the small vessels, and cessation of the capillary circulation over a considerable part of the body. The principal cause of this is failure of the heart's action. During the critical period it may be necessary to excite the organ to increased action by the administration of stimulants, and it is very important to watch carefully for any indications of approaching failure. (Dr. L. S. Beale, p. 86.)

Incubation of Typhus, Relapsing, and Enteric Fevers.—The period of incubation of typhus is about twelve days, of relapsing about nine days, and of enteric (typhoid) fever about two weeks. These periods are, however, extremely variable, and must be only taken as averages. (Dr. Murchison, p. 12.)

Extreme Heat in Fevers.—As bearing upon the important question of restraining extreme heat of the body in disease, we must remember that, whereas the nitrogen-containing tissues are hardly consumed at all in circumstances of health, the febrile state involves a large destruction of them, and this at a time when, from the state of appetite and primary digestion, scarcely any supplies of nitrogenous food can be taken into the system. Every additional degree of fever heat implies so much additional destruction of the most important organs of the body, including the heart and the nerve centres. Excessive heat does not mean plenty of strength, but exactly the opposite; it is an absolute proof that the reserve forces of the body are exceedingly low, and are being constantly and rapidly reduced. (Editor of Lancet, Feb. 8, p. 158.)

HYPERPYREXIA.—*Treated by Withdrawal of Heat.*—It is a fact which no theory can alter, that if the temperature of the body rise up to, or above 107°, death is imminent; partly from the effect of the excessive heat on the vitality of organs, and partly from the excessive tissue-change which must be going on to produce this heat. Excessive and very rapid rise of temperature is seen more frequently in acute rheumatism than in any other disease. Dr. Wilson Fox relates a case in which, when the temperature was 107° in the rectum, the patient was placed in a bath at 96°; the heat, however, continuing to rise to 110° in the rectum, iced water was used and ice placed down the spine. In half-an-hour the temperature was reduced to 103·6°, and in another half-hour to 99·5°. During this time six ounces of brandy were given, and subsequently eighteen ounces a day for several days. (Dr. L. S. Beale, p. 26.)

The statistics published in Germany, comprising more than a thousand cases of typhoid fever, treated in various hospitals, show that the mortality in that disease has been reduced by more than one half by the cooling treatment. This consists in not allowing the temperature to rise above a certain standard. If it does, it is at once lowered by baths or other means. This method of treatment is therefore not directed against the disease itself, but simply against a symptom, upon which a series of other symptoms depend. The patient is not plunged in a cold bath, but in one ten degrees below the heat of his body. It is only in certain cases that the use of absolutely cold water or ice is required. As many as ten or twelve baths in the twenty-four hours are sometimes required, in order to keep down the excessive fever heat. (Editor of British Medical Journal, p. 58.)

There can be no doubt that some cases prove fatal simply from the excessive temperature of the body, consuming the tissues so rapidly. No case can long go on favourably with a temperature of say 106°, and as it is proved by a considerable amount of experience, especially in Germany, that an excessive temperature may, without any danger, be lowered artificially, this plan of treatment is imperatively called for in cases where an excessive temperature exists. Withdrawal of heat must, however, be practised with judgment, and not too rapidly, as the temperature will then swing back to and even beyond its former place. The rule ought to be, that the withdrawal of heat should be the slowest possible for the due attainment of our end. The patient should be placed in a bath of 100°, and the temperature of this gradually lowered, so that no shock is given to the system. On the removal from the bath quinine in doses of from five to twenty grains, according to circumstances, as advocated by Binz, will arrest the rapid reproduction of heat. This mode of treatment requires the incessant use of the thermometer, and the personal supervision of the medical attendant. A very interesting and successful illustrative case is related: it was one of cerebral rheumatism, which had supervened upon an ordinary attack of rheumatic fever, without heart affection. The temperature was 105·8°. A bath not being at hand, ice was put in five wide-necked bottles and placed in bed, close to the body, and ice also applied to the head. The cold was thus applied partially, and the same effect produced as by a less degree of cold applied to the whole surface of the body. (Dr. T. C. Allbutt, p. 61.)

Dr. Roberts's Cooling Pad.—The use of cold baths, and ice, for the reduction of temperature, in cases of hyperpyrexia, is attended with a good deal of difficulty in private practice. The author recommends the use of a pad consisting of circular coils of india-rubber tubing, cemented to a canvas backing. The pad is laid on the patient's body, and a constant stream of water, of any desired temperature, passed through it. It is found that the effluent water is raised from twenty to thirty degrees, and the temperature of the patient can be lowered two to four degrees. This is not effected so rapidly, nor can the lowering be carried on to the same extent as when a bath is used, but quite sufficient for any but exceptional cases. (Dr. W. Roberts, p. 57.)

RHEUMATISM.—*The Synthesis of.*—The author relates some experiments which were made on two persons, in which all the symptoms of acute rheumatism were produced by the internal administration of lactic acid. The acute articular rheumatism came on when the acid was taken, and ceased when it was discontinued. Coinciding with the development of the articular affection was the appearance of perspiration, in the severe attacks copious and acid. (Dr. B. W. Foster, p. 67.)

SCARLATINA AND SMALL-POX.—*Copaiba.*—Copaiba, administered internally, has the effect in cases of scarlatina and small-pox, of causing a free flow of urine, and rapid improvement in the general symptoms. It seems as if it had some direct action upon the virus of the disease, altering and destroying its character. (Dr. Rowand, Quebec, p. 84.)

SMALL-POX.—*The Antiseptic Treatment of.*—The apices of the pustules should be touched with pure carbolic acid, the odour of which may be effectually disguised by a little oil of wild thyme. A solution of one part of carbolic acid in three of olive oil should then be applied over the individual pustules night and morning. The pustules become white, and dry up when treated in this manner. The general surface of the body may be washed with any soluble antiseptic such as coal-tar soap. Sulphite of sodium should be given internally in scruple doses every four hours. By this means the patient is disinfected from the outset of his malady. (Dr. A. E. Sansom, p. 77.)

The secondary fever of confluent and semi-confluent cases of small-pox is due to the presence in the body of products of decomposition, which commence to be formed as soon as the lymphy contents of the hitherto vesicles become purulent. The dermatitis which springs into existence at this period is a necessary consequence of the irritation of the now numerous sub-epidermic abscesses. Sulphurous and carbolic acids, are agents capable of checking the decomposition of the pus, and so obviating the consequent septicæmia. The sulpho-carbolate of sodium should be given internally in doses of from seven grains occasionally, to sixty grains every third hour. Sulphurous acid much diluted with water should be used to wash the body with, and sulphur burnt in the room, so as to impregnate the air with sulphurous acid. Diluted sulphurous acid may also be given as the usual drink. (Dr. A. W. Foot, p. 79.)

In an outbreak of small-pox which occurred on board a U.S. vessel at Yokohama, Japan, the free internal administration of hyposulphites (in the absence of sulphites) proved of the most marked use in checking the disease. The good effects of the salt were generally manifest after the first dose; the patient losing the heat and dryness of the skin, and expressing a feeling of relief. There was produced not only a subsidence of the fever, but a tardy or incomplete development of the eruption, and that in an epidemic of unusual severity. (Dr. W. A Corwin, p. 82.)

Mr. Stephens, of Plymouth, who has had charge of two small-pox hospitals at that town, recommends strongly the local application to the pustules of a mixture of olive oil and carbolic acid. By this means the patients are themselves disinfected, and rendered innocuous to the community at large. (Mr. J. N. Stephens, p. 84.)

Prevention of Pitting in Small-Pox.—The reason that pitting occurs only or chiefly on the face is, that it is exposed to the air, and the scales become hard and dry, whilst the body is in a state of comparative humidity. In a most severe case of small-pox, with extraordinary heat of the face, in the hope of saving the eyes, poultices were applied over them. The patient recovered, but with deep and permanent pitting, except where the poultices had been applied. Since this case occurred to him, Dr. Stokes has adopted as a routine practice the application of light poultices over the entire face, or of a mask of lint steeped in glycerine and water, and covered with a corresponding mask of oil silk; and he has found that, with but one exception, the pitting was effectually prevented. (Dr. W. Stokes, *p.* 73.)

Warm Baths in Small-Pox.—The relief which is given in a severe case of small-pox by a warm bath is very great. It soothes and softens the inflamed and parched skin, relieving pain and procuring sleep. This treatment is called for towards the latter rather than the earlier stages of the disease, when there are black fœtid scabs. In such a case all pain will be relieved, and all fœtor immediately and completely destroyed. (Dr. W. Stokes, p. 76.)

TYPHOID FEVER.—*Use of the Thermometer in.*—What use is the thermometer in the diagnosis of the typhoid fever? During the first four days the temperature rises one degree each day, starting at 98¾ the morning of the first day, but the evening temperature is always two degrees above that of the morning of the same day. The disease is therefore not typhoid fever, if, (1) on the second, third, or fourth evening the temperature approximates even to the normal (98.6° Fahr.); (2) if, during the first two days the temperature rises to 104° F.; (3) if before the fourth and sixth days the evening temperature of a person under middle age does not reach 103°; (4) if the temperature on two of the first three evenings is the same; or (5) if it is the same on the second and the third morning. (Dr. P. W. Latham, p. 15.)

The Bowel Lesion of Typhoid Fever.—The generally entertained opinion that the bowel lesion is the result of nature's efforts to eliminate, is entirely erroneous. Were this true bowel lesion it would relieve rather than aggravate the constitutional symptoms. The inflammation of the agminated and solitary glands bears exactly the same relation to the fever that the sore throat of scarlet fever does to that disease, that is, it is the direct effect of it. No doubt the sloughs and discharges from the ulcerated glands carry the poison of typhoid fever and are capable of conveying the disease from one person to another, just as the discharges from the mouth and nostrils in scarlatina are capable of transmitting their peculiar poison. (Dr. Maclagan, p. 41.)

Lacto-phosphate of Lime in Typhoid Fever.—Lacto-phosphate of lime is at once an aliment and an article of food, and a medicament of the highest value. It excites the appetite and facilitates digestion. In cases of acute disease, especially typhoid fever and forms of inflammation, it is most valuable, acting to some extent in the same way as alcohol. The first effect is that the pulse becomes less frequent and the temperature lower. It is especially useful during recovery, being at once the chemical agent of digestion and the natural excitant of nutrition. (M. Blacke, Practitioner, Feb. p. 65.)

AFFECTIONS OF THE NERVOUS SYSTEM.

ALCOHOLIC PARAPLEGIA.—Chronic alcoholism affects the whole nervous system, but the spinal cord is the part most prone to suffer. This is not uncommon in females. The condition of the patient is this: She lies in bed or on a couch complaining of severe pains in all the limbs, more especially in the lower ones, which are much wasted, or of a sensation like electric shocks running through them, together with numbness and considerable anæsthesia, and at the same time only slight power of movement, or total inability to stand. There is generally enlargement of the liver, with sickness and all the usual signs of chronic alcoholism. The only treatment required is resolutely to break off the stimulants. There need be no fear of inducing delirium tremens, however much has been taken, and however suddenly it is discontinued. (Dr. S. Wilks, p. 106.)

NERVOUS OR SICK-HEADACHE.—In those forms of sick-headache which are preceded by disturbed vision, or other signs recognisable by the patient as preceding an attack, the patient should lie down with the head as low as possible, and if the glimmering be on the right or left of the field of vision he should lie on the opposite side. He should at the same time take some powerful diffusible stimulant. By this means the defective supply of blood to some portion of brain, which is the real disease, is counteracted. There is always a loss of tone about the cerebro-spinal system in cases of this kind, and of course all measures calculated to improve the general health should be adopted. If the attack is followed or preceded by great mental depression nothing acts like half a drachm or a drachm of the ammoniated tincture of valerian. A remedy which is often given with great advantage during a severe attack is bromide of potassium in doses of 5, 10, to 30 grains, combined with 30 or 40 minims of sal volatile. If the attacks have been very frequent, or if there be any scrofulous tendency, the iodide of iron may be given in the following form: ℞. Ferri et ammon. cit., gr. v.; potassii iodidi gr. ij.; aquæ ℥j.; and, according to circumstances, 15 to 20 minims of tincture of henbane, or 20 or 30 minims of aromatic spirit of ammonia may be added. If the stomach is irritable this may be given in the effervescing form. In other cases citrate of iron with ammonia and strychnine may be given with great success. (Dr. P. W. Latham, p. 102.)

NEURALGIA.—*Galvanism.*—In order to cure neuralgia by galvanism we should use the continuous current, by means of small wet sponges attached to small conical conductors. The constant galvanic current has a truly marvellous effect over pain, whereas the interrupted current is of little or no service. The battery used should be Weiss's, sometimes known as Foveaux's. Eight cells of this splendid battery suffice, with very small sponges (about as large as would fill the end of a thimble). They should be applied to the painful part, an inch or two apart, and moved about, without being actually removed from the skin, for about two minutes. After resting a minute they should then be applied again for two minutes. Three very interesting cases illustrative of this plan of treatment are given. (Mr. J. Stead, p. 112.)

PERIVASCULAR SYSTEM OF THE BRAIN.—The vessels of the brain substance are themselves contained in a larger, rather loose, structureless membrane, completely surrounding the vessel. This membranous investment is easily detected with the naked eye in the fresh vessel; in some places a large space lying between them, in others the membrane lies so close to the vessel that it is with difficulty detected. Everywhere these membranous canals exist around the vessels, and they are quite sharply defined externally, as is proved by injecting them with wax. The use of these canals is probably to act as a reservoir for the fluid which exudes from the vessels, in order to permit of the rapid nutritive changes which must be necessary in such an organ as the brain. This perivascular system was first described by Robin in 1855. (Mr. W. W. Wagstaffe, p. 98.)

RESTORATION OF THE FUNCTIONS OF A NERVE AFTER EXCISION OF A PORTION.—The author reports a case in which he exercised a small neuroma of the ulnar nerve, along with half-an-inch of the nerve in its entire thickness, bringing the divided ends together with carbolized silk ligature, and closing the wound, which was treated on strictly antiseptic principles. The functions of the nerve had been in abeyance for nine years, and yet in fifteen days sensation had completely returned everywhere, except at the extreme tips of the two fingers. (Mr. T. R. Jessop, p. 110.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

HEART AFFECTIONS.—*Digitalis.*—Digitalis is not merely a temporary remedy in cases of insufficient power of the muscular walls of the heart. It may be given continuously and uninterruptedly for years with excellent effect. With this, however, we should not forget the great necessity and advantage of rest, and improvement of the nutrition of the heart itself by iron, cod-liver oil, arsenic, and other remedies which increase blood formation. (Dr. J. M. Fothergill, p. 118.)

POPLITEAL ANEURISM.—*Compression.*—When the artery is ligatured, consolidation of the aneurism usually occurs within four hours. This is owing to the complete arrest of circulation throughout it. In applying pressure for the cure of the disease, we have to imitate this complete arrest. The weight used should be 16½ or 17 lbs., and the apex rather conical, so as to penetrate deeply. It should first be placed on the artery at the groin, and retained there about two hours, and then be removed to the superficial femoral in the middle of the thigh. It is a good plan to administer a grain of morphia to deaden the pain which will be caused. By alternating the position of the weight we gain this great advantage, that the task of establishing a collateral circulation is transferred from one set of branches of the femoral to another, and time is not given to either to do this, so that a collateral circulation is not established for a much longer period than if the weight is maintained in one place. The edge of the pubic ramus is the place where the artery can be compressed most easily and with least pain to the patient. Pressure applied diagonally to the bone is better than that applied perpendicularly to it, a less weight by two or three pounds being required. The groin ought to be well shaven, and powdered very thickly. It is of the greatest assistance to have the weight cool, and the upper part of it should be deeply hollowed for the reception of a frigorific mixture. (Mr. G. E. Walker, p. 205.)

Double Torniquet, with Shifting Pressure, for the Treatment of Popliteal Aneurism.—It is extremely difficult, with the single torniquets usually employed, to maintain the instrument in a fixed and immoveable position upon the thigh, without screwing down the compressing pad so tightly upon the artery as completely to obliterate its tube, in some cases to produce agonising pain. By the use of a double torniquet we get over these difficulties completely, and can regulate the flow of blood through the artery to a nicety, relaxing one tourniquet pad, and screwing down the other, as may be necessary to relieve the pain produced by the pressure. It defeats our object if we completely arrest the blood current, as a slow and feeble current is necessary for the deposition and lamination of fibrine in the cavity. (Mr. F. E. Bulley, p. 211.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

Asthma.—Asthma should, with a view to its successful treatment, be viewed as a neurosis of the pneumogastric nerve, of which sometimes the cause is disturbance of healthy function at the brain end, and sometimes at the gastric or hepatic. Thus bismuth and hydrocyanic acid are of great value when the neurosis is of gastric origin. Carlsbad salt, nitric acid, and at times small doses of mercury are all unmistakeably curative when the hepatic system requires relief. Other remedies such as ipecacuanha, belladonna, and nux vomica are of use in appropriate forms of pneumogastric disturbance; whilst iodide of potassium, sulphur, and arsenic are the remedies indicated if there is a gouty or rheumatic diathesis at the root of the malady. (Dr. J. C. Thorowgood, p. 134.)

Asthma, Angina Pectoris, and Gastralgia.—These three affections are intimately allied, being neuroses of different portions of the pneumogastric nerve, of central origin. These neuroses are in great measure owing to inherited peculiarities of the central nervous system. There is only one remedy which is of supreme efficacy in all these affections, namely, arsenic. In doses of five minims of Fowler's solution it gives rapid relief in many otherwise obstinate cases. It is particularly useful in that form of gastralgia which accompanies asthma. There are a certain number of persons who cannot tolerate arsenic, and the subcutaneous injection of the $\frac{1}{120}$th to the $\frac{1}{60}$th of a grain of strychnine should in that case be tried. If given by the mouth, the proper dose is from $\frac{1}{40}$th to $\frac{1}{24}$th of a grain three times a day. (Dr. F. E. Anstie, p. 129.)

Hemoptysis.—*Ergotin.*—The subcutaneous injection of five grains of ergotin is one of the most rapid and effectual means we have of arresting profuse hemoptysis. It produces contraction of the muscular coat of the arteries, and so diminishes the blood pressure. This is no mere theory but has been established by direct experiment. (Dr. Ritchie, p. 137.)

Hooping Cough.—To a child five years of age give the thirty-second part of a grain of morphia, with three grains of bromide of potassium, in solution, every two hours; letting the mother be instructed to suspend the medicine for four hours at any time, if unusual drowsiness come on. (Dr. J. K. Spender, p. 137.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

Acute Intestinal Strangulation.—*Tapping the Bowel in.*—The surgeon should no more hesitate to perform abdominal section in a case of acute internal intestinal strangulation than he would to perform herniotomy in ordinary strangulated hernia. The cases are precisely parallel. When there is an immense accumulation of flatus, before performing such a serious operation as abdominal section, we are quite justified in puncturing the bowel and letting off flatus. The danger attending this is exceedingly small. The objection that we are liable to cause extravasation of some of the contents into the peritoneum does not hold good in practice, as may be seen when a strangulated hernia is tapped to let off the flatus. Nothing escapes at the minute puncture made. If this fails the abdomen should be opened. (Mr. T. Bryant, p. 219.)

Chronic Gastritis.—We can see with the eye the effect of morphia in some cases of acute sclerotitis and iritis; why should not the same "antiphlogistic" power be exercised in other painful inflammatory affections? Morphia is a remedy of some value in chronic gastritis. Let a twelfth of a grain be given twice on the first day, three times on the second, and so on until the patient consumes from one to one and a half grains in the twenty-four hours. (Drs. Barclay and Stokes, Med. Chir. Review, Jan. p. 218.)

Constipation.—*The Pulvis Glycyrrhizæ Co., of the Prussian Pharmacopœia.*—The compound liquorice powder of the Prussian Pharmacopœia, is a very pleasant and efficient laxative in cases of simple constipation. It does not produce serous transudation, but calls into play the peristaltic action of the intestine. It is composed of the following constituents so prepared as to form an almost impalpable powder: Senna leaves, ℥vj.; liquorice root, ℥vj.; fennel seed, ℥iij.; sulphur, ℥iij.; refined sugar, ℥xviij. (Mr. D. Page, Practitioner, May, p. 276.)

Dyspepsia of Liquids.—There is a form of dyspepsia, extremely rare however in this country, in which the patients cannot digest liquids. The presence of any amount of fluid in the stomach causes great uneasiness. There is sometimes also dryness of mouth, with dry skin and constipated bowels, and urine loaded with lithates. The best plan of treatment is the avoidance of liquid—in fact, a dry diet. In one case, on gently vibrating the stomach fluid was heard splashing about within it, and this sound could be always produced irrespective of any liquid recently drunk. (Dr. J. C. Thorowgood, p. 130.)

Ecraseur for the Removal of Hemorrhoids.—Nott's ecraseur for the removal of internal hemorrhoids is an instrument of some value. It is easily and rapidly applied, and removes in a right line the exact amount of tissues required. It is consequently not liable to be followed by stricture of the anus. Its use is not followed by pain, owing to the devitalization of the tissues crushed, nor by hemorrhage, because of their not being completely divided. It consists of two parallel blades coming together like a clamp. One blade has a narrow fenestra running its whole length of about three inches, and the other presents a rough edge like a fine saw, so constructed as to pass into and fill up the fenestra when the clamp is closed. There is a shoulder projecting on each side of the blades, for the purpose of crushing more perfectly the tissues operated upon, a little beyond the thin edge of the blade. If the hemorrhoid is within the sphincter, it is safest to tie a ligature in the sulcus made by the ecraseur, for fear of some secondary hemorrhage—the tissue is so compressed that a very small pedicle is left for the ligature. The part of the tumour outside the ligature should be cut away with scissors. (Dr. J. C. Nott, p. 226.)

Hernia.—*The Taxis.*—The best rule to adopt in the employment of the taxis is to use chloroform at once, if there is the least difficulty in reducing the hernia. It is no use your pushing one way, and the patient involuntarily pushing the other; great injury results. Do away at once with all chance of muscular effort by means of an anæsthetic, and then if you fail operate at once. (Mr. J. C. Forster, p. 216.)

Strangulated Umbilical Hernia.—Interference with large umbilical herniæ of old standing is nearly always followed by death. Do not attempt to return them; they are almost always chronic, slow in formation, and exist for many years before strangulation takes place. The best plan in large herniæ, when strangulated, is to puncture and let off the flatus, but if this fails to relieve the strangulation never be tempted to expose the bowel, simply cut down to the neck of the sac, divide the structure by a herniotome, and leave the rest to nature. (Mr. T. Bryant, p. 219.)

Reduction of Strangulated Hernia without Operation.—Invaginate the scrotum as in Wützer's operation for the radical cure of hernia, by passing the index finger *behind* the testicle and cord up to the external ring. The hernial tumour is then pressed downwards over the finger towards the back of the hand, so as to make the structures in the ring tense, and consequently smaller. The invaginating finger is then forced firmly upwards and outwards in the direction of the internal ring. As soon as the finger is firmly grasped, the hand should be slightly turned and the finger pushed towards the middle line. Considerable force may be applied in this way, as all the delicate structures are behind the finger, which acts mainly on the stricture. On withdrawing the finger, the hernia can usually be easily returned. The same principle is equally applicable to femoral hernia. (Dr. P. C. Smyly, p. 217.)

Tapping a large Strangulated Scrotal Hernia.—If of very large size, and requiring exposure and manipulation of the bowel, death not unfrequently follows herniotomy. Why not puncture the bowel with a grooved needle or very fine trocar and let out the flatus? The knuckle of intestine will at once collapse and may then be easily reduced. This has not been yet tried in actual practice, but the suggestion seems good. (Mr. T. Bryant, p. 219.)

TAPEWORM.—Turpentine acts with considerable certainty, but is apt to produce intoxication and strangury. Kousso is not easily obtained genuine, and is better therefore avoided, as it will be found uncertain in its action. Kameela has an unpleasant griping action, but is otherwise a good remedy. The etherial tincture of male fern is the best remedy. It is efficient and free from the objections to the other remedies. For its successful use the patient should take no food after breakfast except a little mutton broth or tea, and in the evening he should take a brisk aperient. The following morning early, a drachm and a half of the liquid extract or etherial tincture rubbed up with half an ounce of mucilage, should be taken in about two ounces of milk. It is better to lie quiet for two or three hours after taking this dose, otherwise it is apt to cause nausea and faintness. At the expiration of that time another aperient dose should, if necessary, be administered, as the male fern itself does not always purge. Milk is the favourite food of the worm, and is therefore the best vehicle for the administration of the medicine. After a week's rest, this ordeal should be repeated in order to be certain that the whole of the worm is expelled. (Mr. W. Date, p. 224.)

AFFECTIONS OF THE URINARY ORGANS.

CATHETERISM.—*Necessity for Caution in.*—In cases where catheterism is necessary in stout elderly men, in whom there is reason to suspect weakness of the heart, it is advisable to enjoin the recumbent position, in order to avoid syncope, till after the removal of the catheter, if not for a longer time. A fatal result may ensue if this rule is not observed. (Mr. F. D. Lys, p. 261.)

DIABETES.—*Cantani's Treatment.*—Prof. Cantani, of Naples, bases his treatment of diabetes on the theory that the question is not so much one of increased production of sugar, as of defective combustion. This he believes to be owing to the production of a morbid form of glucose, which is incapable of being transformed into lactic acid, and therefore cannot be burned, and is passed unchanged in the urine. The heat of the body is thus maintained at the expense of the albuminates and fats. The treatment consists in an exclusively animal diet, combined with the administration of lactic acid. Lactic acid is chemically intermediate between glucose and carbonic acid, and is burned off at the lungs, so saving the important albuminates and fats. This exclusively meat diet means rigorously one of plain meat, roast or boiled, without any sauces of milk or eggs, and certainly without any bread, flour, or any vegetable matter whatever, the only seasoning permitted being salt, oil, or a little vinegar. This mode of treatment is put forth as affording relief in most cases, and as effecting a cure in a larger proportion of cases than any mode of treatment hitherto devised. (Dr. G. W. Balfour, p. 145.)

EXAMINATION OF THE URINE IN VESICAL DISEASE.—Never let a patient pass the whole of the urine to be examined into one vessel, but a little into one and the bulk of the liquid into another vessel. The small quantity passed first contains anything which may have been in the urethra. This is especially of service in the diagnosis of chronic prostatitis, which so much resembles stone in the pain being severe in the tip of the penis, and accompanying and following micturition. There is generally a little mucous discharge in chronic prostatitis, which, except for this mode of examination, we might think came from the bladder. (Sir H. Thompson, p. 230.)

Blood in the Urine.—Blood is very rarely seen in simple cystitis, but it is as common in vesical calculus as hæmoptysis is in phthisis, and is especially common after some exertion, as driving or riding. Blood from this cause is rather florid in tint, whilst blood passed from the kidney remains long in the bladder, and from contact with urine is brown in colour. Hemorrhage may also occur in hypertrophy of the prostate. (Sir H. Thompson, p. 229.)

Diagnosis between Prostatic and Calculous Disease in an Elderly Man.—In cases where symptoms suggest one or other of these affections, enquire whether the frequency of micturition is greater at night or in the day. If in the day it is probably calculus, because the movements of the body are accompanied by corresponding movement of the stone, and consequent increase of irritation. If in the night it is probably prostatic disease. Prostatic enlargement does not trouble a patient much in the day, and calculous disease not much at night. (Sir H. Thompson, p. 228.)

Pain in the Bladder or Penis.—A patient complains of pain in the region of the bladder or perineum. There is almost certainly chronic cystitis. Ask whether he feels the pain before, during, or after passing urine. If the pain is before it is because the mucous membrane is becoming uneasy in consequence of distension. If the pain is during and after passing water, and in the end of the penis, he is likely to have stone; and especially also if the pain is increased by exercise. It is almost pathognomonic of stone to have the pain in the tip of the penis. Chronic prostatitis simulates stone more than any other disease. In both the pain is at the tip of the penis. (Sir H. Thompson, p. 228.)

LITHOTOMY.—*Buchanan's Method of performing.*—The feature of this operation, upon which its success depends, is that the angle of the staff is held fairly below the central point of the perineum, consequently the deep perineal fascia, and the transverse muscle and artery of the perineum, are missed by the knife in making the first incision, while they are all divided in the operation of Cheselden. In Dr. Eben Watson's hands the mortality of these operations has been only 1 in 23½. (Dr. E. Watson, Lancet, May 4, p. 605.)

Median Lithotomy.—Median lithotomy is not justifiable where the stone is upwards of half-an-inch in diameter, as the orifices of the ejaculatory ducts are very likely to be obliterated and permanent impotence produced. (Mr. W. F. Teevan, p. 247.)

To pass a Lithotrite.—Keep the handle close to the patient's belly, and delay the turning movement until it is called for by the fact of the lithotrite not going any further, thus announcing that it has arrived at the turning point. The instrument ought to be oiled from end to end, and the penis pulled well forward on to the lithotrite until the beak has passed the sub-pubic arch. If the patient's prostate be enlarged, a good stiff bolster under the buttocks will make all the difference between success and failure. (Mr. W. F. Teevan, p. 243.)

Rupture of the Prostate during Lithotomy.—Whether we examine cases of lithotomy on the living or dead, this same conclusion only can be arrived at—that complete rupture of the prostate always occurs during extraction of even an ordinary sized stone by a limited incision, and subsequent dilatation, or what is called so. Prof. Ellis has demonstrated that there is no such thing as dilatation of the prostate, and that what surgeons call dilatation is in reality complete laceration. The practical result of this is that stones should be cut out, and *not torn out.* (*Mr. W. F.* Teevan, p. 246.)

Retention of Urine from Impermeable Stricture.—Take a catheter of medium size, make a perforation in its extreme end, and pass it down to the stricture. Then take an india-rubber ball with nozzle to fit into the end of the catheter, squeeze it so as to drive out the air, and connect it with the catheter. The patient must now be directed to make a very gentle effort to pass his water and whilst he does this the ball is to be allowed to expand gently. The surgeon and patient will be agreeably surprised to see the urine flow through th catheter. This does not of course always succeed. (Dr. P. A. O'Connel p. 251.)

Retention of Urine from Enlargement of the Prostate.—In most of these cas the retention first comes on in consequence of holding the water too long i railway journey, or at some public meeting. Men advanced in years should instructed to wear some apparatus by which the necessity for so long retain the urine is obviated, when about to be placed in the circumstances named (Mr. R. Quain, Medical Times and Gazette, May 18, p. 568.)

Squire's Vertebrated Prostatic Catheter.—This instrument is like an ordinary silver catheter in appearance, but the last three or four inches is composed of a series of perfect joints, so as to allow of a large amount of flexibility. Thus the instrument is sufficiently flexible to accommodate itself to any curves of the prostatic portion of the urethra, but it cannot double upon itself as a flexible gum-elastic or caoutchouc catheter sometimes does. At the same time it possesses longitudinal stability, that the propulsion of the hand may be transmitted without loss to the beak. This instrument has been very much approved of in New York. (Dr. T. H. Squire, p. 253.)

Stricture Dilator.—*Mr. Hill's New Wedge-Shaped.*—A considerable amount of force is sometimes required to push up the dilator of Holt's instrument. If, instead of a circular dilator involving a good deal of friction resistance, the rupturing force is wedge-shaped, great increase of power is obtained, with increased facility in applying it. Mr. Berkeley Hill's dilator, as used by him at University College Hospital, consists of a split sound which equals the calibre of a No. 2 or No. 3 catheter. The halves of the sound can be separated by passing between them a wedge fixed on a slender stem. The instrument is most effective and will split the hardest strictures with ease. (Mr. B. Hill, p. 250.)

Urine.—*Alkaline Phosphates in.*—To determine the amount of phosphates present in urine as alkaline phosphates, add to a portion of the urine ammonia in excess; the white precipitate consists of alkaline-earthy phosphates; filter and add ammonium chloride and magnesium sulphate; the white crystalline precipitate indicates the amount of phosphates which was originally present as alkaline phosphates. (Dr. J. C. Brown, p. 144.)

To determine the Amount of Lime Salts in Urine.—To a certain amount of the urine add ammonia, and filter; then add ammonium oxalate; the white precipitate contains the calcium as oxalate. (Dr. J. C. Brown, p. 144.)

Varicocele.—The object of treatment of varicocele is the obliteration of the mass of veins of which it consists; i.e. when the case is so bad as to require operative interference at all. Mr. Wood, of King's College Hospital, recommends continuous elastic wire pressure for this purpose, the elasticity being obtained by means of the instrument used. The instrument consists of two limbs united by a strong spring at one extremity. One limb ends in a thin round steel shaft, joining the limb at right angles, and terminating in a transversely oval and obliquely placed eye. The loop of wire having been made to surround the veins is passed through this eye, and the free ends are twisted round the end of the other shaft, which is first made to approximate its fellow. The spring will now cause constant tension upon the wire surrounding the veins. (Mr. J. Wood, Brit. Med. Journal, Sep. 16, p. 316.)

[The ordinary plan of obliterating these veins is by passing two or three small hair-lip pins underneath them, and winding a figure of 8 ligature round the free ends of the pins. Eds.]

Vesical Calculus.—*The Prevention of.*—As nineteen out of twenty calculi have uric acid for their basis, practically the problem is, how to prevent the formation of this calculus. Prior to the actual formation of stone there is usually a period in which the patie [illegible] bitually passes gravel, and this without any [illegible] orrection of diet. The ordinary plan of treatment, viz., the exhibition of alkalies in large doses is bad, as merely masking, by solution, the excessive formation of lithic acid, without remedying the cause, consequently on giving up the remedy the disease generally returns. Vichy and Vals waters are alkaline, and are not suitable. What is wanted is to stimulate the organs which are really at fault, viz., the liver and digestive organs generally, and not the kidneys, which are already doing their best. For this purpose the mineral waters of Friedrickshalle or Carlsbad are suitable. The chief constituent of these is sulphate of soda. (See table at p. 237). These waters may be procured at home, and a short course of Carlsbad, followed by a longer course of Friedrickshalle, will cure most cases of persistent lithic acid deposit. Of course a fair amount of exercise in the open air should be taken, the skin being sufficiently protected. Promotion of the action of the skin is very necessary. Alcohol, saccharine and fatty matters should be excluded as much as possible from the diet. (Sir H. Thompson, p. 280.)

AMPUTATIONS, DISLOCATIONS, FRACTURES, AND DISEASES OF BONES AND JOINTS.

Amputation at the Lower Part of the Thigh.—It is always well to make a circular division of the muscles at the bottom of the wound in order that the large nerves may be divided high up, and consequently that the bulbous enlargements which occur at their extremities may be out of harm's way. If this is not done a painful stump is liable to result. (Mr. J. C. Forster, p. 177.)

Caries of the Hip-Joint.—Out of 126 cases of excision of the head of the femur for caries, 71 recovered with more or less useful limbs. A great proportion of these would have otherwise terminated fatally. The objection to the operation, that it is useless removing the head of the femur because this does not remove the carious state of the cotyloid cavity, is not in practice proved to hold good. The acetabulum, however, is not invariably diseased, for out of 81 cases it was healthy in 18. (Mr. H. Hancock, p. 165.)

Dislocations of the Femur.—A proper knowledge of the ligament which passes from the anterior inferior spinous process of the ileum to the anterior intertrochanteric line of the femur, is of immense importance in understanding dislocations of the hip, and consequently in successfully reducing them. This ligament is of great strength, almost equal to that of the tendo Achilles. It may be called the Y ligament, because, like that letter upside down, it bifurcates into two portions, not however widely separated. It is this ligament which restrains the movements of a dislocated femur, and regulates the position of the limb. A careful consideration of this ligament shows that dislocations into the sciatic notch should be reduced by extension with the thigh bent at right angles to the pelvis. For this purpose a special apparatus has been invented, see woodcut p. 178. Where this is not at hand the surgeon should place his foot unbooted on the patient's pelvis and then lift from the knee. (Dr. Bigelow, Mr. J. Hutchinson, p. 172.)

Endoscope in Military Surgery.—Dr. Fenger, of Copenhagen, in 1869, made some experiments with the endoscope on horses, and came to the conclusion that pieces of cloth in wounds, or bullets driven into bones, could be seen by its means. Dr. Fenger has stated that during the late war he was enabled in several instances, on examining wounds some weeks after they had been inflicted, to see their interiors distinctly by means of the endoscope, without causing pain, hemorrhage, or any subsequent irritation. (Mr. T. Longmore, p. 187.)

Foreign Bodies in Wounds.—*Lecomte's Stylet-pince for the Detection of.*—This instrument consists of a central steel rod, on which slides a slender canula. The rod terminates in two small branches, each of which has a small cup-like blade or curette at the extremity. These curettes have very fine and sharp edges, which meet and overlap, forming a smooth steel knob when the canula is pushed forward, but which open wide when it is slightly withdrawn. This instrument is one of the greatest utility, as it brings away a small fragment of any substance over which the edges of the curettes glide in closing. The glistening appearance of a minute scale of lead thus removed strikes the eye at once. This instrument responds as an indicator with even more distinctness than the Nelaton probe, in all cases in which that test would be of service, while it answers for a variety of other cases in which the Nelaton probe would give no indication at all. (Mr. T. Longmore, p. 194.)

FRACTURES OF THE FEMUR.—*The American Splint for.*—This is a very useful apparatus. It consists of a long external splint reaching from the axilla to some inches below the foot, and a shorter internal splint extending to the same distance below the foot. The two splints are connected together at the bottom by a thick cross-piece of hard wood, perforated by a female screw, through which works a male screw terminating in a couple of hooks. Of course the two splints are padded internally, and have soft cushions at their upper extremities. A loop of plaister is now made below the sole of the foot, the five or six long straps of which it is composed passing upwards and downwards along each side of the leg, and retained in position by other transverse pieces of plaister. The hooks are passed through the loop below the sole of the foot, and the splints bound together by straps of leather passing round them both and the included limb. A perineal band is now applied as in the usual long splint. Now by means of the screw the limb can be extended as much as is necessary. No bandages are required, and the thigh is always open to inspection. (Mr. B. W. Richardson, p. 162.)

SETONS IN STRUMOUS DISEASES.—Nothing does so much good in the various forms of tubercular disease as a free discharge of pus; the system seems thereby relieved of morbific matter. This is seen in the application of a seton in strumous ophthalmia. This remedy will generally prove effectual, and is sometimes our only means of arresting the disease. Even in such a disease as chronic tubercular meningitis a free discharge of matter from a large scalded surface has been known to at once arrest the disease. A seton was introduced in the chest in a case with a large cavity in the left lung, the right universally crepitant. There had been several attacks of hemoptysis. For six months a continuous discharge was kept up from setons, with so happy a result that fifteen months later she was attended in confinement, and lived six years after. (Dr. E. Crossman, p. 197.)

SPINA-BIFIDA.—*Antiseptic Treatment.*—Dr. Wilson, of Glasgow, relates a case of spina-bifida which he treated thus: The tumour being moistened over with carbolised oil was opened by a free longitudinal incision, under an antiseptic veil of surgeon's lint soaked in carbolised oil. The lint was removed, and a large piece of carbolised lac plaster applied, and over this another, kept in position by adhesive plaster, one edge being left comparatively free for escape of fluid. A soft folded handkerchief was laid over all. A good firm pad of skin resulted. (Dr. J. Wilson, p. 193.)

WOUNDS AND ABSCESSES.—*A Substitute for Lister's plan of Dressing.*—The carbolic or antiseptic system of Lister so utterly fails, when any of the precautions recommended is neglected, and consequently involves such care and time that it becomes impossible to practice it in the out-patient department of a large public charity. The following is a good substitute for it. Wash the wound with tepid water, then with a lotion composed of equal parts of methylated spirits and water. A pad of lint dipped in the lotion is then to be laid on the wound, and in the case of a large abscess partly introduced into the same. A piece of gutta-percha tissue or oil-cloth is to be placed on the lint or tow, and a common bandage applied. (Mr. T. Cooke, Practitioner, May, p. 284.)

Disinfectants and Antiseptics to Wounds.—There is a difference between an antiseptic and a disinfectant, and this difference is of some importance in surgery. An antiseptic, as carbolic acid, will prevent decomposition, but does not neutralize and render innocuous the contents of a wound already in a state of decomposition. This is done by a disinfectant, the best of which for use in surgery is chloride of zinc. Chloride of zinc acts as an antiseptic and prevents decomposition, or as a disinfectant, and removes it if present; it is also not itself poisonous, like carbolic acid, nor is it volatile, and lastly its effects are not fugitive as are those of Condy's fluid. The fact of chloride of zinc solution being innocuous and non-volatile, renders a solution of it more easy to use and more generally applicable than one of carbolic acid. No complicated system of dressing is required. (Mr. C. Roberts, Lancet, April 27, p. 570.)

AFFECTIONS OF THE SKIN, ETC.

Anthrax.—*Treatment by Strapping.*—The growth of an anthrax may be arrested, or at any rate considerably curtailed, by firm strapping with strips of plaster. These should be two feet long, each two inches wide at one end, and one and a half at the other. They are to be applied in pairs, the broad ends being first attached at opposite sides of the anthrax, in such a manner that when the narrow ends of the plaster are drawn together they shall pass over the tumour. The anthrax will require dressing in this manner once a day. (Mr. G. A. Gloag, Brit. Med. Journal, Feb. 8, p. 126.)

Eczema.—The dilute yellow nitrate of mercury ointment is a better local application than oxide of zinc ointment. Let anyone dress two patches with the two ointments respectively, and he will soon see the difference. But the nitrate ointment should be well made. Much of the ointment made is worthless, being dry, dirty, green, and rancid, spoiling almost as soon as it is diluted, whereas it ought to retain its bright yellow colour for months. The dilute ointment should only be prepared when it is wanted, by being rubbed down with a little sweet almond oil, until it is the consistence of thick cream. If the eruption is seated on the head in children, the hair should be cut off, and the ointment applied night and morning. A linen cap should also be worn day and night. (Mr. J. L. Milton, p. 318.)

External Acute Inflammations.—*Tartrate of Antimony.*—Tartar emetic is of unrivalled efficacy in the treatment of some external acute inflammations, as erysipelas, and inflammation of the breast. But it is generally given in too large doses, fifteen drops of antimonial wine every hour being sufficient. Some inflammations are more curable by morphia or opium, and these are generally characterised by the predominance of pain or nervous irritability. Well marked pyrexia should suggest antimony in the doses stated. (Dr. J. K. Spender, Med. Chir. Review, Jan. p. 224.)

Ringworm in Schools.—Dr. Tilbury Fox reports that in an epidemic of ringworm, in a large school, he found the dust of the room to contain abundance of spores of tricophyton, the fungus of the disease. If therefore the air of such a room be not thoroughly disinfected by burning sulphur in it, the disease is liable to break out again. Recent cases may be at once checked, and often cured by simple blistering. If the disease has reached the bottom of the hair follicles, epilation should be practised. A good plan is to keep the head soaked in a solution of sulphurous acid; of course a proper cap of silk should be worn. The head should be washed each day. The question frequently has to be decided whether a child is well, and fit to return to school: the best guide is the presence or absence of short broken-off hairs; if these exist the disease is not cured. The hair ought not to be dull and dry, and the scalp should be free from scurfiness before the child is allowed to return. (Dr. T. Fox, p. 315.)

Skin-Grafting.—The first and perhaps only essential point is that the surface of the sore on which the graft is placed should be healthy. The piece of skin grafted should be small, and should include only the upper layer of the true skin and rete mucosum, but not the fat beneath the skin. For the removal of this the best instrument is the skin-grafting scissors of Mr. Macleod. The fragment of skin should be placed on the thumb nail, and divided into three or four pieces, and then placed about half-an-inch or three-quarters from the margin of the sore, and about one inch apart. They should be covered with a piece of oiled gutta-percha skin, and this supported with cotton-wool and a bandage. The surface of the sore will require daily cleaning, by a stream of tepid water squeezed from a sponge. Small pieces of skin, as recommended, do better than large ones. (Mr. T. Bryant, p. 188.)

Stings of Bees and Wasps.—Inject into the puncture a solution of carbolic acid. The pain will instantly cease. (Dr. A. E. McRae, p. 323.)

Varicella and Variola.—*The Correlation of.*—Variola and varicella are more intimately related than has been hitherto admitted by pathologists. Cases have occurred of variola being communicated from varicella. (Mr. F. T. Porter, p. 85.)

Whitlow.—*Carbolic Acid.*—After freely slitting up the parts and giving vent to the matter and sanious discharge, use a solution of carbolic acid freely. The wound will rapidly assume a healthy character. (Dr. A. E. McRae, p. 322.)

VENEREAL DISEASES.

Gonorrhœa.—*Carbolic Acid.*—The following is an excellent injection in gonorrhœa; carbolic acid, eight grains; tannic acid, eight grains; glycerine, half-an-ounce; water to one ounce. (Mr. G. Ashmead, p. 324.)

[We can bear testimony to the value of this combination, but have not employed it quite of the strength recommended by Mr. Ashmead. Eds.]

Syphilis.—*Iodides of Ammonium and Sodium.*—It sometimes happens that in cases of later secondaries, or tertiary syphilis, requiring the prolonged use of iodide of potassium, the drug ceases to produce any result, or is no longer tolerated. This may be possibly owing to the deteriorating action of the base on the blood. Ammonium and sodium are not open to the same objection, and iodides of either of these bases should be substituted. (Mr. B. Hill, p. 325.)

AFFECTIONS OF THE EYE AND EAR.

Acute Sclerotitis and Iritis.—*Morphia.*—Apply a compress of lint dipped in warm water over the eye, which is to be kept closed, and give the patient the twelfth of a grain of morphia every hour for the first twenty-four hours (omitting as usual the eight hours of the night), and one-tenth, or one-eighth of a grain, every two hours for the subsequent two or three days. The patient will scarcely be aware that he is taking a narcotic. He should be restricted in diet and an occasional purgative should be given. The vascular congestion will rapidly disappear, and the pain and photophobia diminish. The case may often be cured in four or five days. (Dr. J. K. Spender, Med. Chir. Review, Jan. p. 220.)

Amaurosis.—*Hypodermic Injections of Strychnia.*—In a case of amaurosis the ophthalmoscope showed dulness of the media, anæmia of the disc, retina pale and anæmic. Total amaurosis had existed for nine months. The twenty-fourth of a grain of strychnia was injected, and the next day, the patient could count fingers, and distinguish colours. A continuance of the treatment led to almost complete recovery of sight. (Mr. A. S. G. Jayaker, p. 310.)

Amaurosis from Smoking.—There is undoubtedly a form of amaurosis produced by tobacco smoking. Excessive smoking will not in some people have any such effect, for this deplorable result is due to some extent to an idiosyncracy. It fortunately happens that there is sufficient warning of the failure of sight, and if the habit is resolutely broken off complete recovery results. (Mr. J. Hutchinson, p. 308.)

Cataract.—*Rate of Progress of Senile Cataract.*—Those cases which commence with fine stripes in the edge of the lens while the nucleus remains clear are usually very slow in their progress, so much so, indeed, that the patient often outlives his cataract. Broad stripes in the cortical part with accompanying opacities of the nucleus are on the other hand often extremely rapid in progress. (Dr. H. B. Swanzy, p. 286.)

Extraction of Cataract.—There are certain disadvantages in Graefe's operation for the extraction [illegible] especially the necessity for iridectomy, with its attendant risk of prolapse of the vitreous humour; and the necessity of performing it upon the upper part of the cornea, in order that the enlarged part of the pupil may be covered by the upper eyelid. During the whole operation the eye has to be kept open by a speculum, and drawn down by the forceps. This is both painful and injurious. The operation recommended by Mr. Liebreich is as follows: the section of cornea is to be made in the lower third by means of a small Graefe's knife. The puncture and counter-puncture are to be made in the sclerotic about one millimeter beyond the cornea, the whole remaining incision passing with a very slight curve forwards, so that the centre of the flap made is about one millimeter and a half distant from the margin of the cornea. Iridectomy is not necessary, and the only instruments required are the Graefe's knife, and a cystotome with Daviel's spoon at the other end of the handle. This operation is so easy that any one, without special practice, can perform it. (Mr. R. Liebreich, p. 277.)

Extraction by a Peripheral Section of the Iris.—Dr. Taylor, of Nottingham, removes cataract by making a section of the iris close to its upper edge of attachment. A small piece of the periphery of the iris is then excised, and the lens extruded through the section in the iris. It is necessary to open the capsule when the first section is made. The great advantage claimed for this operation is its not interfering with the pupil, which remains uninjured and central. (Dr. C. B. Taylor, p. 284.)

Chronic Conjunctivitis.—*Nitrate of Silver.*—The best strength for a solution of nitrate of silver is ten grains to the ounce. Mr. Liebreich at St. Thomas' Hospital relies almost exclusively on the daily application of this solution in the treatment of chronic inflammatory affections of the lids, and the superficial structures of the globe. (Mr. R. Liebreich, p. 277.)

Conical Cornea.—This otherwise incurable affection may be treated with favourable results by removing the top of the cone. Before doing this a fine needle, armed with silk ligature is to be passed across the cornea from side to side. During the operation this presses gently against and supports the iris and crystalline lens, and after removal of the cone the ligature is tied in front, so as to close the opening in the cornea. The cone of cornea is to be removed in its entire thickness, and the edges of the incision are to be directly across the thickness of the cornea. A cataract knife may be used for this purpose. The cornea is thrown into slight folds by the tightening of the ligature, but these subsequently disappear. The disadvantage of the operation is, that an opaque spot in the centre of the cornea results, but none of the patients (nine in all) complained of this, the sight being otherwise so much improved. (Mr. C. Bader, p. 272.)

Glaucoma.—We must not forget to distinguish between the two forms of glaucoma—the simple and the inflammatory. The simple form is exceedingly slow, often lasting two or three years. It is unattended by pain, and the only external signs of the disease are a few rather engorged episclerotic veins. The symptoms are increased tension of the globe, defect in vision all round the periphery of the field, the central vision remaining pretty good. Inflammatory glaucoma rarely extends over a term of more than a year, and usually but a few months. The tension advances by fits and starts, and is attended with severe ciliary neuralgia; there is also dimness of the whole field of vision. An attack of this kind generally subsides after a few hours, and the media, which during its continuance had been cloudy, again become clear. The attacks however recur again and again, with increasing severity and frequency, until the sight is completely destroyed. No remedy is of any use except iridectomy, and this only under the two following conditions: 1. The portion of iris must be excised to its very periphery. 2. It must be a sufficiently wide piece. (Dr. H. R. Swanzy, p. 300.)

Unrecognised Acute Glaucoma.—Cases of acute glaucoma are not unfrequently misunderstood by men who certainly ought to have recognised their nature. Out of sixty-seven cases, fifty-two were allowed to go on until the eye had suffered serious injury from delay. The most prominent symptoms are rapidly increasing far-sightedness and the appearance of haloes round lights in the evening; then occurs superficial inflammation of the eye, an ovoid dilatation of the pupil, stony hardness of the eye-ball. The last symptoms are cloudiness and roughness of the cornea, with loss of vision. The proper and only remedy is iridectomy. (Mr. E. Hart, p. 296.)

POLYPUS OF THE EAR.—After removal of polypus of the ear the best caustic for application to its place of growth is chloro-acetic acid, applied on a very small camel's-hair brush. In using this care should be taken not to touch the meatus; and provided this is done very little pain is felt. Potassafusa and chloride of zinc are unmanageable, and nitrate of silver is not sufficiently powerful. When polypi exist along with a perforation of the membrana tympani, as is generally the case, it is very necessary after removal of the polypus to keep the cavity of the tympanum free from secretion, and by means of appropriate treatment to induce a more healthy condition of its lining membrane and that of the Eustachian tube. A very good lotion for this purpose is one of sulphate of zinc and opium, and with this the external meatus may be filled, the affected side being uppermost and horizontal. The patient should then blow through the perforation, and a little of the fluid will pass into the Eustachian tube on his ceasing to blow. If a polypus is large, Wilde's snare armed with fine gimp is the best instrument for its removal. (Mr. W. R. Dalby, p. 312.)

PURULENT OPHTHALMIA IN CHILDREN.—In the treatment of this disease, careful cleansing of the eyes is most important. This should be carried out throughout the whole course of the affection. Cold should be applied to the closed lids for about three days, and then the application of nitrate of silver is to be commenced. It is no use applying a weak solution of silver, and pure nitrate of silver is too strong. A mixture of one part of nitrate of silver, with two parts of nitrate of potash, melted together, and run into a small iron mould is the proper thing. Or the end of an iron wire may be dipped repeatedly into the melted mass, until on cooling a sufficiently thick layer of caustic adheres to it. Each lid should be everted daily and rapidly wiped over with this once a day, and as the case improves every two or three days. (Mr. R. Liebreich, p. 262.)

STRUMOUS OPHTHALMIA.—In a bad case of this kind, the only treatment admissible is an antiphlogistic one, consisting in the instillation of a dozen drops or so of atropine solution (gr. iv. to ℥j.) in the course of the day, with purges of calomel, and restraint in a dark room. The child should not be allowed to lie on its face. The excessive spasmodic contraction of the lids (blepharospasm) is productive of much harm. The following plan was adopted by Von Graefe, and never fails, if carried out properly, to correct this. The child is raised up by two nurses, one supporting the trunk and hands, the other holding the legs, while the surgeon takes charge of the head and dips the face under water, holding it there for about ten seconds; the patient is then allowed to take a breath, and the same manœuvre is repeated several times. The effect is magical, the blepharospasm disappears, the child lies on its back, allows the eyes to be easily examined, atropine to be instilled, and becomes in every way as docile as could be wished. This process may require repetition as the spasm returns. The best topical application is Pagenstecker's yellow ointment, the composition of which, and mode of preparation, is given in the article from which this is taken. (Dr. H. R. Swanzy, p. 265.)

SUBSTITUTE FOR THE OPHTHALMOSCOPIC MIRROR.—An ordinary watch-glass reflects quite enough light to illuminate the posterior part of the eye sufficiently for observation. The observer of course looks through it. (Dr. J. S. Torrop, p. 311.)

WHITE ATROPHY OF THE OPTIC NERVE.—Dr. Fraser, of Paisley, reports a case of white atrophy of the optic nerve, produced by over-taxing the eyes, in weaving by gaslight. He applied the continuous galvanic current, placing the electrodes on the temples. They were tried in other situations, but this was found to be the best. Each time the current was passed the sight, as proved by test types, was found to be improved, the gain in visual power being usually three inches, one of which however only was permanent. Considerable improvement in the retinal circulation also took place. (Dr. D. Fraser, p. 304.)

MIDWIFERY AND THE DISEASES OF WOMEN, ETC.

Anæsthetics in Uterine Operations.—*Chloroform and Bichloride of Methylene.*—Chloroform is very objectionable on account of its occasionally causing vomiting during the operation. Ether is troublesome. Bichloride of methylene is the best anæsthetic, sickness after it being rare. (Mr. T. S. Wells, p. 374.)

Blackbee's Resilient Skeleton Speculum.—This simple and useful instrument will be at once understood by reference to the woodcut at page 375. It is the cheapest speculum there is, and offers the most extensive view possible of the entire circumference of the vagina and cervix uteri, permitting the easy application of remedies to their entire surfaces. It is very easy to introduce on account of its resiliency, and is self-retentive. (p. 374.)

Entry of Air into the Vagina.—Air only enters the vagina in one position of the body, viz., in the prone. In all other positions the walls of the vagina remain in apposition, and this is much favoured by the movements of respiration. The knowledge of this fact is of use in examination by means of the duck-bill speculum, for by placing a patient in the semiprone position, the falling forward of the abdominal viscera is favoured, air fills the vagina, counteracts the effects of respiration, and thus enables us to get a good view of the os uteri. (Dr. R. Barnes, p. 367.)

Fibrous Tumour of the Uterus.—Is it wise to only partially remove a fibroid polypus when we cannot get at the whole? Undoubtedly it is. The more however is removed the better, and the less the subsequent hemorrhage. In one case in which this was done not a trace of tumour could be detected nine months afterwards, the whole of the remaining part having been absorbed or come away as discharge. (Dr. T. Skinner, p. 344.)

Fibrous Tumour of the Womb.—*Hemorrhage.*—The best topical application is the small solid stick of anhydrous sulphate of zinc. It can be easily introduced quite up into the uterine cavity and is far less dangerous than liquid injections. A mixture of ergot and peracetate of iron or iron alum may be given internally. (Dr. A. Meadows, p. 349.)

Hysteria.—Hysteria requires two factors for its production, viz., a predisposing nervous state and some local determining cause, such as uterine or visceral disorder. The predisposing state may be so strong as to produce the disease without any second cause whatever. No doubt hysterical emotions are actually conceived in the brain, but the brain sympathises with the unhealthy state of other parts of the body. The local exciting causes are generally uterine or ovarian, but not invariably so. Of uterine causes it is generally the milder sorts, that are mucous membrane deep, which cause hysteria; and sometimes, by applying nitrate of silver to an ulcerated cervix, we most unwittingly bring on an attack of hysteria in patients who presented no previous sign of a tendency to it. Of visceral affections the most important exciting cause is that state of liver and stomach derangement which we call biliousness. The recurrence of hysterical attacks are only prevented in these cases by such measures as are best calculated to prevent biliary derangement. (Dr. E. J. Tilt, p. 371.)

Medicated Vaginal Pessaries.—It is no part of the function of the vaginal mucous membrane to digest fats; for this reason gelatine and glycerine in the proportion of one part of the former to four of the latter is preferable as a basis for medicated pessaries to cocoa butter which is generally employed. (Dr. A. Meadows, p. 349.)

Menorrhagia and Leucorrhœa.—*The value of Arsenic in.*—Arsenious acid is a remedy of considerable value in cases of menorrhagia, not depending upon some such cause as polypus, but simple hyperæmia of the uterus. It should be commenced in doses of two to six drops of liquid arsenicalis and very gradually increased. It is a good plan to give it in granules, each containing one milligramme, in which case the commencing dose is one to three granules three times a day. Arsenic has a decongestive action upon mucous membranes, and this renders it of great use in cases of too frequent recurrence of the catamenia with excessive discharge, and leucorrhœa in the intervening periods. These symptoms are generally accompanied with a good deal of debility. The first effect of the arsenic is to cause return of the appetite; and in two or three weeks the patient will be found to have improved in appearance and increased in weight. Very gradually the intercatamenial period will be lengthened, two or three days being gained each time, and the flow will be less abundant. The remedy should never be given in large doses, but for a considerable time. (Dr. J. H. Aveling, p. 360.)

OVARIOTOMY.—*Ligature and Return of the Pedicle.*—It does not appear probable in theory, and the result of experience proves it, that no harm results from tying the pedicle with silk and returning it into the cavity of the abdomen. The portion of the pedicle on the distal side of the ligature being surrounded by *warm* tissues does not lose its vitality. A case is related in which this practice was perfectly successful. (Dr. J. C. Gooding, p. 351.)

PUERPERAL FEVER.—*The Nature of.*—Puerperal fever is not the result of a simple purulent absorption, nor of inflammation of an ordinary type extending from the genital organs, but is a true diphtheritic affection. In most cases a diphtheritic deposit may be detected on the vagina, or in the small lacerations of the os uteri. In other cases it is confined to the site of the placenta or the upper part of the uterus, when of course it is invisible to the eye, but its existence may be detected by the microscope in the discharges. In true diphtheria it has been proved by microscopic research that the spores of the fungus penetrate to the blood vessels, whence they are carried over the whole system, producing a general disease. It has not actually been proved that a similar process takes place in the case of puerperal fever, but the probability is so great as almost to amount to certainty. (Dr. E. Martin, Berlin, p. 335.)

Puerperal Septicæmia.—Carbolic Acid.—It appears from a case related by Dr. Trask, of Astoria, U. S., that carbolic acid may be administered so as to impregnate the system with it. The result in this case was that the signs of septicæmia "were kept in abeyance." The acid was administered by the mouth, rectum, and vagina. A half drop of Calvert's solution was given in mucilage by the mouth every two hours. The solution for the rectum was one drop, increased to five drops to an ounce of mucilage, a half gill being thrown up after every dejection. There was a good deal of diarrhœa. The vaginal injection was not less than five drops to the ounce every three or four hours. Within twenty-four hours the diarrhœa ceased, and the case commenced to improve to some extent. The patient tasted the carbolic acid in everything. (p. 341.)

STRICTURE OF THE CERVICAL CANAL OF THE UTERUS.—The operation of division of the os internum and cervical canal by a hysterotome, as practised by Sir James Simpson, has now been on its trial for a sufficient length of time to permit of our judging of its merits. Dr. Protheroe Smith states that, although it gives relief at the time, the original constriction is increased by it. He had one case in which two ridges of cicatricial tissue existed corresponding to the two lateral incisions made many years before. As a substitute for incision, Dr. Smith has for many years practised dilatation by means of an instrument terminating in two blades, after the model of Heurteloup's lithotrite. The use of this instrument is contraindicated if there is any inflammatory condition, whether of the mucous lining or of the deeper tissues of the uterus. If hyperæmia exists it is best reduced by repeated scarifications of the labia uteri laterally at the commissures. He considers that the results of dilatation are more permanent than those of incision. (Dr. J. P. Smith, p. 353.)

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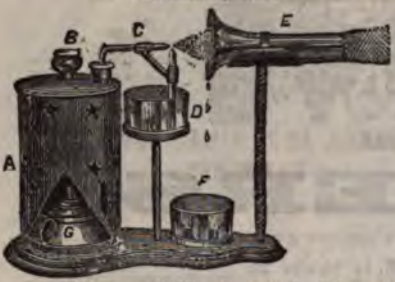

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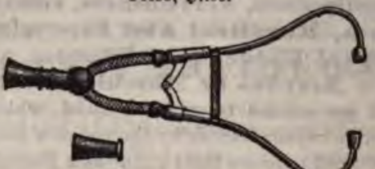

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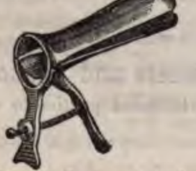

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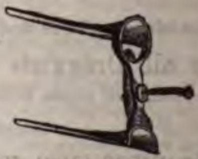

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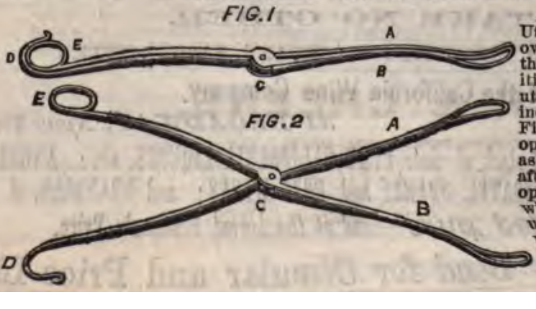

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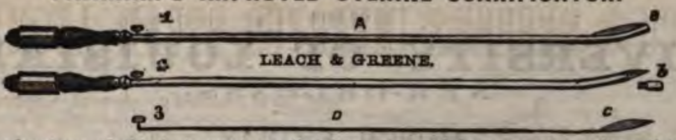

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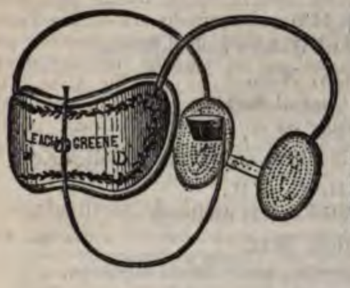

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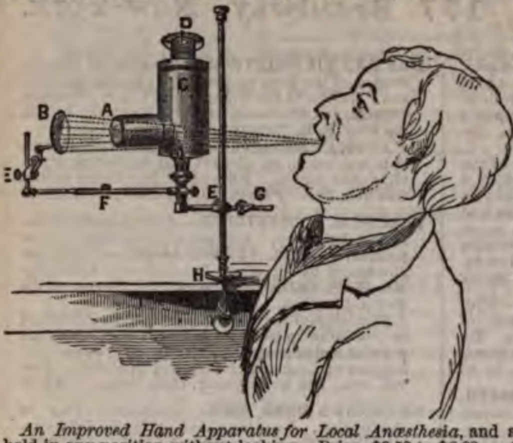

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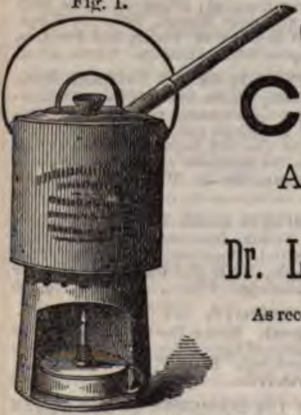

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